

Stephen J. Gould
and Immanuel Velikovsky
Essays in the Continuing Velikovsky Affair

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THE ORIGINAL VELIKOVSKY AFFAIR, AN IDEA THAT JUST WOULD NOT GO AWAY, By Irving Wolfe

The Velikovsky Affair is one of the blackest episodes in the history of science. That it could have happened at all is disturbing, but that it happened in the 20th century, the age of science, and in America, the embodiment of modern science, indicates that at the heart of our culture lies a powerful panic-stricken irrationality which is not scientific but horrific. We think we live in an age of reason, but the ugly, demented events of the Velikovsky Affair scream out that beneath the very shallow surface of our alleged reasonableness there is a deep foundation of terror which makes us respond hysterically and with uncontrollable rage to any intimation that our world is not safe. That is the lesson the Velikovsky Affair teaches us. It is not the story of one man, or of one moment, or of one group. It is an insight into the dark, buried, instinctual region of fear that underlies the whole of Western culture.

Before we begin to look at the details of the Affair, its intellectual and historical backgrounds must be clearly established. We are dealing, of course, with the Immanuel Velikovsky story, which the unfamiliar reader will grasp more easily if it is seen as falling into four separate areas:

1. Velikovsky's highly revolutionary, catastrophic theories,
2. the Velikovsky Affair, which is the sordid and vicious response to those ideas by mainstream, American science,
3. the impressive amount of evidence, especially from the space probes, which has accumulated in mainstream science since 1950 in support of Velikovsky's theories and predictions, and
4. catastrophist science, or research by Velikovsky's supporters in a number of fields, published in hundreds of scholarly articles, which constitutes a parallel universe to mainstream, traditional, uniformitarian science.

This book will deal only with Item 2, the Velikovsky Affair. It means we will not debate whether Velikovsky is correct, or how much the new evidence may support him, or what his followers have produced, for such issues, (however interesting they are in themselves), are irrelevant here. What we *will* do is look very closely at how Velikovsky has been treated by organized science. Our approach will be historical-interpretive, which is to say that we will present a chronology and psychology of the Affair by tracing its major events, whose excessive and unscientific nature will soon reveal the outrage and anger that underlies it.

This book, therefore, is not about whether Velikovsky is right or wrong about nature, it is about whether organized science has been right or wrong in its treatment of Velikovsky. In the course of investigating this question, reference will naturally have to be made to Velikovsky's ideas. This is done, however, not to support him, but rather to illustrate how mainstream science has tried to destroy him and his ideas through lies, misrepresentation, sneers, character assassination, suppression and blackmail. It is this *action by science* which is the Velikovsky Affair.

As for Velikovsky's ideas themselves, they were succinctly delineated by philosopher David Stove:

- "(I) A thesis of general catastrophism: there have been sudden major changes in the physical state of the earth due to agents not observed to operate at present.
- (II) A thesis of *extra-terrestrial* catastrophism; *i.e.* thesis I plus the clause that some of these agents have been extra-terrestrial.
- (III) A thesis of *historical* extra-terrestrial catastrophism; *i.e.* thesis II plus the clause that some of these extra-terrestrial catastrophes have taken place in historical times.
- (IV) The thesis of *Worlds in Collision*: *i.e.* thesis III plus the clause that one of these catastrophes was mainly due to comet-Venus, around 1500 B.C."¹

(It is to be understood, of course, that when Stove refers to "agents" or "extra-terrestrial agents," he does not mean little green men or friends of E.T., but *natural* causes or agents like comets, meteorites, asteroid debris or meteor showers).

Most of Velikovsky's adversaries do not seem to have ascertained the distinction between these theses, as a result of which different critics have mistaken different parts of the overall theory for the whole and their attacks have lacked focus. In my opinion, however, (as I will argue in the last chapter), it is thesis III, "*historical* extra-terrestrial catastrophism," which has provoked the corrosive rage and blind anger which we will see so often in the Velikovsky Affair. No scientist today would contest theses I or II, (that enormous natural catastrophes, of a magnitude and origin *unseen today*, have occurred to the Earth in the very far past, or that many of these catastrophes were caused by intrusions from space in the form of comets, meteor showers, meteorite fragments or asteroidal debris), as long as they are dated millions (or billions) of years ago, for this puts them *safely in the dim and different* past. It is thesis III which strikes the chord of Terror, for this brings the cataclysms into the *recent* past, meaning not only that they *continued* to occur into the age of present civilization, but, *much more terrifying*, that they could happen again soon, that they are normal. It is *this* prospect which traditional science cannot face, and for which it has hunted Velikovsky, and this panicked interaction is the Velikovsky Affair.

The Affair as a whole has now endured for 50 years, and, to grasp its shape and evolution, we can usefully divide it into two halves, which run roughly from 1946 to 1970, (what I will call the Original Affair), and then from 1970 to the present, (the Continuing Affair). This chapter will present a critical analysis of the first part, the Original Affair, to prepare the reader for the rest of the book, which will itemize the events of the second part, the Continuing Affair, after which the last chapter will present a full-scale investigation and interpretation of the phenomenon beyond the boundaries of the 20th century, *i.e.*, as it has persisted throughout our culture from ancient Greece to the present day.

The Original Affair begins about 1945 when Velikovsky, after five years of intensive research, started to show the draft of *Worlds in Collision* to scientists. It ends in the late 1960's, after his first four major books had been published and much subsequent evidence had accrued in his favor, but when he was still essentially fighting his battles alone. It has been extensively chronicled in four principal publications. The first is *The Velikovsky Affair: Scientism Versus Science*, (1966), by political scientist Alfred de Grazia, historian of science Livio Stecchini and engineer Ralph E. Juergens, the book which first brought the ugly story to widespread, public attention. It gives a glaring and irrefutable account of the hostility, blackmail, character assassination, misrepresentation, disinformation and repression evinced by science towards Velikovsky. Then comes *Stargazers and Gravediggers: Memoirs to "Worlds in Collision,"* (1983) a posthumous book by Velikovsky himself, (put together in 1954-1956 from files collected since 1940), where he gives a much fuller and autobiographical account of the Affair, accompanied by many new and very corroborative documents. Third is Henry Bauer's *Beyond Velikovsky*, a book quite critical of him, but whose first section helpfully summarizes and provides new data on the Affair. Lastly comes *The "Jewish Science" of Immanuel Velikovsky: Culture and Biography as Ideational Determinants*, (1990) by social historian Duane Vorhees, much of which has been serialized in the journal *AEON*. (Vorhees had access to Velikovsky's private papers and was able to contribute much additional material)². The story has also been retold elsewhere,

¹Quoted in Henry H. Bauer, *Beyond Velikovsky: The History of a Public Controversy*, (Urbana, Ill., 1984), p. 57.

²Alfred de Grazia, *The Velikovsky Affair: Scientism Versus Science*. New York: University Books, 1966; Immanuel Velikovsky, *Stargazers and Gravediggers*. New York: William Morrow and Co., 1984; Henry H. Bauer, *Beyond*

(notably in *Pensée* 1.1, C. J. Ransom's *The Age of Velikovsky* and *SISR* 4.4)³, but the four books I have cited are the most comprehensive sources for our purpose, and putting them together, especially in the light of very modern theories in group psychology and in the sociology of science, allows us to reconstruct and interpret the events more thoroughly and insightfully than ever before. (For convenience, they will be referred to as "de Grazia," "Stargazers," "Bauer" and "Vorhees").

One: The Original Affair

It occurs to me, as I set out now to recount once more the appalling events of the Original Affair, that the reader of 1990 may find it hard to believe that such things could have happened in 1950 in the U.S.A., for they seem more typical of 1350 in late-medieval Europe of the Inquisition, but I promise you they are true. Science reporter Eric Larrabee, who as we will see played a central role in the Original Affair, expressed the same trepidation in his "Introduction" to *Stargazers and Gravediggers*: "Readers too young to remember will find their incredulity strained, but it happened."⁴ This is how organized American science reacted to the picture Velikovsky painted.

I had debated several ways to tell the story, but I finally decided to simply set it down concisely, without exaggeration, and as accurately as I can.

* * *

The opponents of Velikovsky circulated a number of false accusations in order to discredit him, one of them being that he wrote *Worlds in Collision* all alone, in isolation (*i.e.*, in an ivory tower), without consulting *any* scientists. The truth is that from the very beginning, and continuing well into the 1960's, Velikovsky sought the advice of some of the major scientists of his time. These include (in some sort of chronological order) archaeologist John Garstang, anthropologist Franz Boas, social philosopher Horace M. Kallen, historian of science Harry A. Wolfson, Assyriologist Robert H. Pfeiffer, Egyptologist Walter Federn, astronomer Lloyd Motz, physicist Carl Friedrich von Weizsäcker, astronomer Gordon Atwater, astronomer Walter S. Adams, physicist Valentin Bargmann, archeologist Claude F. A. Schaeffer, cultural historian Jacques Barzun, electrophysicist Alfred Goldsmith, Egyptologist Etienne Drioton, literary critic Salvador de Madariaga, geologist Harry H. Hess and physicist Albert Einstein. Each of these people, whether or not they agreed with Velikovsky in full or even in part, listened to his ideas with interest and courtesy and responded as helpfully as they could.

Quite the contrary was the reaction of Harvard astronomer Harlow Shapley, (perhaps the most influential man in his field in America at that time), and it is with him that the Original Affair properly begins. As part of his campaign to get the best advice he could, Velikovsky approached Shapley in April of 1946 after Shapley had spoken at a Manhattan hotel, outlined very briefly his theory about "changes in the constitution of the solar system,"⁵ and asked Shapley to read the manuscript of *Worlds in Collision*, (hereinafter *WIC*). Shapley said he would only if someone whose opinion he respected recommended it to him. It was mutually agreed that philosopher Horace Kallen of Harvard would be suitable. Shortly afterward, Velikovsky wrote to Shapley and specified two tests that

Velikovsky: The History of a Public Controversy. Urbana, Ill.: University of Illinois Press, 1984; Duane Vorhees, *The "Jewish Science" of Immanuel Velikovsky: Culture and Biography as Ideational Determinants*, (1990).

³*Pensée*, 1.1, Portland, Ore., Issues 1-10, "Immanuel Velikovsky Reconsidered," publ. 1972-1975. Defunct.

C. J. Ransom, *The Age of Velikovsky*, Kronos Press, Glasboro, N.J., 1976; *Chronology and Catastrophism Review*. Publ. *SISR*, ed. Bernard Newgrosh, London: (January 1976), Vol. I, No. 1.

⁴Immanuel Velikovsky, *Stargazers and Gravediggers*, (New York, 1983), p. 13.

⁵*Ibid.*, p. 48.

could be performed as support for his theory: (1) That the Martian atmosphere "consists mainly of argon and neon" and (2) That "bands of gaseous hydrocarbon should be formed in the absorption spectrum of Venus."⁶

That first contact with Shapley was to have enormous consequences, for Velikovsky in good faith gave the manuscript to Horace Kallen, (they had been discussing his ideas informally for several years), and Kallen wrote to Velikovsky "The rigor of the scientific imagination that you show, the boldness of your construction fill me with admiration."⁷ With regard to the many items of evidence Velikovsky had assembled to show that ancient history was experience and not metaphor, "one would be hard put for it to challenge their cogency."⁸

Kallen duly communicated his enthusiasm to Shapley within a month. He called Velikovsky's manuscript "remarkable" and revealed that "after taking it up, I could not put it down." Velikovsky in his opinion "has built up a serious theory deserving of the careful attention of scholars," and it is to Kallen's credit that he recognized at once the enormous implications of the theory for traditional belief.

"If his theory should prove to be valid, not only astronomy, but history and a good many of the anthropological and social sciences would need to be reconsidered both for their content and explanation. If it should not prove to be valid, it would still be one of those very great guesses which occur far too infrequently in the history of human thought."⁹

These words of Kallen lead us to the core of the anger which followed, for the history of the Original Affair is characterized by two very opposed sorts of reactions by academic specialists to Velikovsky's ideas. A small number, like Kallen, actually read the manuscript objectively and responded helpfully. The majority, however, never read it but only reacted emotionally and with rage to second-hand summaries. That is what Shapley did. Never having seen the manuscript at all, he nevertheless responded very negatively to Kallen's letter about it, saying that "The sensational claims of Dr. Immanuel Velikovsky fail to interest me . . . because his conclusions were pretty obviously based on incompetent data."¹⁰ (Shapley, not having read the book, could have had no idea which data Velikovsky used). Then came a blanket, unconditional dismissal: if Velikovsky made sense, said Shapley, "then the laws of Newton are false" and astronomy has been wasting its time. "In other words, if Dr. Velikovsky is right, the rest of us are crazy."¹¹ (Shapley had not yet seen even the draft.)

Here we have a pattern which most of the guilty ones in the Original Affair were to follow—Shapley broke his promise and refused to read the text, (even after it had been recommended by a fellow scholar), he took refuge in the unassailability of Newton and he called Velikovsky "crazy." One might think at first that in overreacting this way he is merely protecting his turf, or clinging to familiar belief in a spirit of inertia. But, as I will explain in my final chapter, the true reason for his subsequent machinations is to be found in Kallen's observation that, if Velikovsky is correct, then all standard academic belief "would need to be reconsidered." As I will show in my last chapter, that is the origin of mainstream science's terror: if the theory of catastrophism is true, (*i.e.*, if traditional science's deeply felt beliefs about the permanence, stability and safety of the Solar System are wrong), then it is the *world-view* derived from those beliefs which was placed in peril, and the threatened individual reacted accordingly. At that time, however, Velikovsky did not anticipate this instinctual response, but naively felt that, after Shapley's very negative response, "the Shapley chapter was closed,"¹² meaning that Shapley was merely one more scientist

⁶*Ibid.*, p. 50.

⁷*Ibid.*, p. 51.

⁸*Ibid.*

⁹*Ibid.*, pp. 52-53.

¹⁰*Ibid.*, p. 53.

¹¹*Ibid.*, p. 54.

¹²*Ibid.*, p. 55.

who had declined to read the manuscript. How wrong Velikovsky was. Shapley, as Velikovsky later perceived, was very, very interested in Velikovsky's ideas. He was merely biding his time.

What precipitated the Affair was the reaction of the next major player, science reporter John J. O'Neill of the *New York Herald Tribune*. Velikovsky gave him the manuscript of *WIC* in August of 1946 and O'Neill promised to read it within several months, but he called Velikovsky after only a few days. "I have never read anything comparable," he said, perceiving that the book could "well compel science to reconsider its basic postulates."¹³ In O'Neill's next newspaper column, (August, 1946), the name and ideas of Velikovsky first came to public attention.

"The fact that the period covered by what we may call modern history has been a relatively quiescent era has lulled us into a state of false security and into a totally misleading philosophy concerning the earth and its possibilities . . . There has been built up in the minds of the people a belief that life, the world and the universe are on an extremely orderly basis."¹⁴

Then comes mention of the heretic and of the magnitude of his defiance.

"Dr. Immanuel Velikovsky . . . has assembled into a monumental work evidence from all the early civilizations that in the first and second milleniums before Christ tremendous terrestrial cataclysms took place . . . Dr. Velikovsky's work . . . presents a stupendous panorama of terrestrial and human history which will stand as a challenge to scientists to frame a realistic picture of the cosmos."¹⁵

The gauntlet had been thrown down.

The next event to bring Velikovsky closer to a collision with Shapley was the securing of a publisher. Many houses turned it down in the second half of 1946, but finally Macmillan expressed an interest. Editor James Putnam was assigned. He received several favorable reports from his readers, one of whom was Hayden Planetarium curator Gordon Atwater, who urged that Velikovsky's ideas "should be presented to the world of science in order that the underpinning of modern science can be re-examined."¹⁶ I will outline in a moment the critical roles these two men played in the Original Affair, and the devastating effect it had on their own careers; but the immediate result was a non-binding contract with Macmillan in 1947. Encouraged by that, and working closely with a copy editor, Velikovsky revised *Worlds in Collision* and in 1948 signed a full publishing contract with Macmillan. The book was typeset, Velikovsky left the country and, when he returned to New York in early 1949, the galleys were ready to be proofread. It all looked straightforward and simple. Velikovsky naturally expected *some* resistance, "yet the violence of this opposition, when it came, surpassed my expectation."¹⁷

What aroused the opposition was a flood of sensational pre-publication publicity. The editor of *Harper's Magazine* wrote to Velikovsky in March of 1949 that his people had seen the Macmillan galleys and "were fascinated."¹⁸ They asked if Velikovsky would agree to have excerpts appear in *Harper's* prior to the book's publication. Velikovsky finally agreed, and reporter Eric Larrabee prepared a condensation which appeared in *Harper's* in January of 1950 under the provocative title "The Day the Sun Stood Still." In it Larrabee warned that "Philosophy, science, religion—there is scarcely an area of knowledge or conviction invulnerable to Dr.

¹³*Ibid.*, p. 59.

¹⁴*Ibid.*

¹⁵*Ibid.*

¹⁶*Ibid.*, p. 65.

¹⁷*Ibid.*, p. 67.

¹⁸*Ibid.*, 68.

Velikovsky's detailed and documented denial that the earth's history has been one of peaceful evolution¹⁹ and the story became a sensation. It was quoted or reprinted in newspapers as far away as *Paris-Match*; it was described with a photograph of Velikovsky in the April *Vogue*; it was endorsed by literary reviewer Clifton Fadiman; it was summarized by Fulton Oursler for the *Reader's Digest* in March, and the same material was condensed in *Collier's* in February and March under the provocative title "The Heavens Burst," with the printed approval of popular theologian Norman Vincent Peale, all prior to the book's well-announced publication. At the same time, *Newsweek* praised Velikovsky as "a broad gauge savant with an incredible field of competence in the sciences."²⁰ Each of these issues of the magazines mentioned sold out, and Velikovsky became the subject of national interest and fascinated debate even before his first book had appeared. He was even scheduled to broadcast to Europe on the "Voice of America."²¹

This is what set Shapley going. "Almost immediately, it seems, Harlow Shapley began organizing a movement to prevent the publication of Velikovsky's *Worlds in Collision*."²² Knowing only what was contained in the *Harper's* summary, he wrote to Macmillan on the stationery of the Harvard College Observatory, asking if the rumor were correct that Macmillan "will not proceed to the publication of Dr. Velikovsky's 'Worlds in Collision.' This rumor is the first item with regard to the Velikovsky business that makes for sanity."²³ It was, of course, a blatant attempt to coerce the publisher into changing its mind, (for no such rumor existed), and Shapley reinforced the pressure by revealing that "a few scientists with whom I have talked about this matter . . . are not a little astonished that the great Macmillan Company . . . would venture into the Black Arts."²⁴ He called Velikovsky's idea "the most arrant nonsense of my experience" and reiterated that "the aforementioned rumor is a great relief."²⁵ Putnam replied within a week that Macmillan *would* publish the book which, however controversial, "should be brought to the attention of scholars in the various fields of science with which it deals,"²⁶ and his last sentence shows plainly that he understood Shapley's threat: "I cannot believe that our publication of this book . . . will affect your feeling toward our publications in the scientific field."²⁷ *The very next day*, Shapley wrote back to Putnam and battle was engaged. Velikovsky's ideas, Shapley said, were such "complete nonsense" that, when he had met Velikovsky in New York, "I looked around to see if he had a keeper."²⁸ He hoped that perhaps "only this "Worlds in Collision" episode is intellectually fraudulent," but there was no backtracking, for he warned that publication of it "must cut me off from the Macmillan Company."²⁹

A very different tone from Putnam's is to be found in George Brett's letter to Shapley a week later. Brett, president of Macmillan, was a businessman aware that the science publications which Shapley threatened were the heart of his company's sales. To mollify Shapley, Brett arranged to have the book re-reviewed at the last moment by new censors, even though it had already been approved by three prior readers and was *on the point of being printed*. What is appalling is that Brett knew that Shapley had still not read the book and he did not even ask him to do it then, but merely proclaimed his gratitude "that scholars take the trouble to caution a publisher as you have."³⁰ In any event, the new independent scientists approved the book and the project went ahead. Velikovsky observed

¹⁹*Ibid.*, p. 71.

²⁰Duane Vorhees, "Worlds in Collision: Reviews and Reviewers," *AEON*, Vol. III, No. 6, p. 16.

²¹Bauer, *op. cit.*, p. 4.

²²Vorhees, *op. cit.*, p. 16.

²³*Stargazers, op. cit.* p. 81.

²⁴*Ibid.*

²⁵*Ibid.*

²⁶*Ibid.*, p. 82.

²⁷*Ibid.*

²⁸*Ibid.*, p. 83.

²⁹*Ibid.*, p. 84.

³⁰*Ibid.*, p. 95.

"How close my book came to being scrapped a few weeks before the publication date,"³¹ and Brett hoped that Shapley would be satisfied.

Quite the opposite occurred. When Shapley saw that he had failed to prevent the publication of *Worlds in Collision* by Macmillan, he launched a full-scale pre-publication attack from several quarters "to stamp out the revolutionary doctrine,"³² in Velikovsky's words. In February of 1950, *Science News Letter* carried an article entitled "Theories Denounced," under which was written "using such phrases as 'nonsense and rubbish,' top astronomers, geologists, historians, archaeologists and theologians denounced statements by Dr. Velikovsky."³³ This group, (which appears to have been put together extremely quickly), included Shapley, (the "top astronomer"), who, announcing that he was speaking on behalf of all "his fellow astronomers,"³⁴ was, of course, the one to call the book "rubbish." We shall not debate here the scientific issues they raised, except to note that they were stoutly rebutted, especially by Velikovsky. What is more to the point is that Shapley was the president of Science Service which published *Science News Letter*, and that it also reviewed the book after publication dismissing it as "science fiction."³⁵

Next came an article in *The Reporter*, in March of 1950, by the astronomer Cecilia Payne-Gaposchkin, (a subordinate of Shapley's at Harvard), whose title was "A Thing Imagination Boggles At." She, too, had seen only the *Harper's* article, yet she recorded her "incredulity and derision" of the *book*, and she, too, not surprisingly, called it "rubbish."³⁶ Again, we will not review here the scientific points she raised, beyond noting that a number of people have very seriously questioned them. It is the tone and the innuendoes which reveal the unscientific bitterness behind her efforts, for she calls the book "a sloppy parade of jargon," compares it with the Great Moon Hoax of the 19th century and insinuates that Dr. Velikovsky's motives are monetary, (*i.e.*, that he is a charlatan).³⁷ Four weeks later, in the same magazine, Larrabee accused her of demolishing the book's condensation, not the book itself, and she replied *then* that she had just read the book, which "is better written and more fully documented than the popularizing previews, but is just as wrong."³⁸

One may guess the urgency felt by the Harvardists by noting that her article was mimeographed and hundreds of copies were distributed "at Harvard's expense,"³⁹ sent out on the letterhead of the Harvard College Observatory to universities, journals, magazines and newspapers even *before* it appeared in *The Reporter*. Furthermore, to make sure no one missed it there, ads were placed in *The New York Times* announcing its appearance in *The Reporter*, and *Science News Letter*, (apparently feeling that its own four-man demolition crew of a month earlier had failed), proclaimed in March of 1950 that *The Reporter* had just published "the first detailed scientific answer to Dr. Immanuel Velikovsky's theory."⁴⁰ At no time before or since in the history of scientific debate has an article been so widely disseminated *before* it was published, nor has that much money ever been spent to advertise in newspapers that it would appear soon. This was done, as Velikovsky wrote, "with the unconcealed purpose of influencing the reviewers."⁴¹

In February of 1950, two months before *Worlds in Collision* was published, the newspaper *The Compass* reprinted Larrabee's article and praised Velikovsky. *A day later*, Shapley wrote to the editor, Ted Thackrey, calling

³¹*Ibid.*, p. 87.

³²*Ibid.*

³³*Ibid.*, p. 89.

³⁴Vorhees, *op. cit.*, p. 18.

³⁵*Ibid.*, p. 23.

³⁶*Stargazers, op. cit.*, p. 91.

³⁷*Ibid.*, p. 94.

³⁸*Ibid.*, p. 95. [However, later in 1977 Gaposchkin, in the *Harvard Crimson*, admitted she had not read any of Velikovsky's books (see *Kronos*, V. 4, pp. 51-52) C.G.]

³⁹Vorhees, *op. cit.*, p. 16.

⁴⁰*Stargazers, op. cit.*, p. 94.

⁴¹*Ibid.*, p. 92.

Velikovsky a "crank" and his book "the most successful fraud that has been perpetrated on leading American publications."⁴² (It had not yet been published). He compared it to the Flat Earth tractates, bemoaned the current "age of decadence" and recommended that Thackrey read Payne-Gaposchkin's article, which he happened to enclose.⁴³ He ended with the bizarre statement "You know, of course, that I personally am a sympathetic friend of the thwarted and demented," but nevertheless asserted that Velikovsky's ideas were "pure rubbish, of the level of astrological hocus-pocus."⁴⁴

Thackrey's reply was unambiguous. First, he openly derided the quality of Shapley's research: "at the time your views were expressed . . . not you, nor Dr. Gaposchkin, nor the professors you cite—not one—had read the manuscript or the book."⁴⁵ Then, he expressed shock at Shapley's attack on "Dr. Velikovsky, a man of unusual integrity and scholarship, whose painstaking approach to scientific theory is at least a match for your own."⁴⁶ He considered that Shapley had erred "by the totally unscientific and viciously emotional character of your attack upon Dr. Velikovsky," and that "it is impossible for me not to be alarmed at the intensity and character of the attack."⁴⁷ Then came the plain charge that Shapley had "successfully damaged Dr. Velikovsky's work," that Shapley had "seen fit to unleash a series of attacks"⁴⁸ and that in his opinion Shapley's acts were "morally and criminally slanderous and libelous,"⁴⁹ and he asked Shapley "to consider your course of conduct . . . before proceeding further in your campaign . . . to damn a theory about which you obviously know nothing."⁵⁰

Shapley wrote back the very next day, protesting that "the only hot communication I have made was this letter to you,"⁵¹ and that many scientists *independently* agreed with him, but Thackrey knew better. He wrote to Shapley that in his opinion "the chief inspiration for these adverse views stems from Dr. Harlow Shapley,"⁵² that Shapley's letters to Macmillan were "so sizzling that your letter to me might seem tepid by comparison,"⁵³ that the many professors who attacked Velikovsky had not "reached their conclusions completely independently of discussions with you"⁵⁴ and that "you and Mrs. Gaposchkin made extensive and successful efforts to suppress the book."⁵⁵ In his reply, Shapley tried to trivialize the whole thing. He referred to Macmillan as "the once reputable publisher," called the book an "atrocious," compared Velikovsky to McCarthy and yet denied "that in some way I was carrying on a crusade against Dr. V. Of all the astronomers from whom I have heard comment, I am the mildest and most forgiving."⁵⁶

I have taken some pains to establish the heated atmosphere of those first months of 1950, in order that the reader might grasp how thoroughly upset the Harvard group was and to what lengths they were willing to go, especially Shapley. With that done, we can move ahead more quickly now, although the reader may still be surprised at what followed, even after these many instances of rudeness, duplicity and scorn.

⁴²*Ibid.*, p. 97.

⁴³*Ibid.*

⁴⁴*Ibid.*, p. 98.

⁴⁵Vorhees, *op. cit.*, p. 23.

⁴⁶*Stargazers. op. cit.*, pp. 88-89.

⁴⁷*Ibid.* p. 99.

⁴⁸*Ibid.*

⁴⁹*Ibid.*, p. 100.

⁵⁰*Ibid.*

⁵¹*Ibid.*, p. 102.

⁵²*Ibid.*, p. 108.

⁵³*Ibid.*, p. 109.

⁵⁴*Ibid.*

⁵⁵*Ibid.*, p. 110.

⁵⁶*Ibid.*, p. 111.

1. *Worlds in Collision* was published in April of 1950 and shot to the top of the best-seller lists, averaging a thousand (1000) copies per day.
2. Gordon Atwater, curator of the Hayden Planetarium and Chairman, Astronomy Department, American Museum of Natural History, had planned a special planetarium show at his institution, "Our Battle-Scarred Earth," to coincide with the publication of the book. At the same time, he prepared a very complementary article on Velikovsky for the magazine *This Week*, to appear the day before the book did.
3. Astronomer Otto Struve, (a friend and associate of Shapley's and President of the American Astronomical Society), wrote to Atwater in March to determine if Atwater supported Velikovsky. The reply was that he did, "that science must investigate unorthodox ideas calmly and with an open mind."⁵⁷ This apparently determined Atwater's fate, for, as Juergens put it, "the last few weeks before *Worlds in Collision* made its appearance were spent in strategic maneuvering by the leaders of the resistance forces."⁵⁸
4. The first step by the opposition was quick and brutal. A week before *Worlds in Collision* appeared, a colleague of Atwater's walked into his office and spat in his face, (how's that for scientific behaviour?), Atwater was fired on the spot and told to clear out of his office in minutes, (although his salary was continued for several months), and the projected planetarium show on Velikovsky was immediately cancelled. As Atwater later reported, "There was sheer terror and panic at the Hayden."⁵⁹ Vorhees adds laconically "that Harlow Shapley was a member of the museum's board of directors."⁶⁰
5. *This Week* was pressured not to publish Atwater's favorable review of *WIC*, but, on O'Neill's advice, it did. The article was cautious but supportive. The book, Atwater said, "has been the subject of a storm of controversy that has swept across the nation . . . [It] will have an explosive effect in the world of science."⁶¹ Velikovsky did "a laborious research job in many fields . . . before he was ready to weave them together"⁶² and the result is a very "unusual approach to some of the world's great problems."⁶³ Atwater recognized what might happen—"the opening impact of this theory—due to its sensational nature—is certain to arouse violent hostility"—but he hoped that sanity would prevail. As we shall see, it did not.
6. Just before publication, Struve wrote to O'Neill at the *Herald Tribune*, asking him to compose a negative review of *WIC* because O'Neill had already written favorably of the ideas before publication. O'Neill did not agree, and planned to write a series of articles about Velikovsky. When the book appeared, O'Neill was not assigned to review it, as he had expected. Who was? You guessed it, Shapley's colleague Otto Struve. In his hands, the book was called mystical, non-logical and ignorant of astronomy, just as Payne-Gaposchkin (of Harvard) had said. It was compared to astrology and flying saucers, and its theory of cometary collisions was said to be impossible, yet six months earlier a comet had collided with Mars, and Struve himself, in reviewing the astronomical events of 1950, would write of "solid bodies in the solar system whose orbits intersect in such a manner as to produce occasional collisions."⁶⁴ This pattern of a double

⁵⁷*Ibid.*, p. 114.

⁵⁸ Alfred de Grazia, *et al.*, *The Velikovsky Affair*, (London, 1966), p. 29.

⁵⁹Clark Whelton, quoted in Vorhees, *op. cit.*, p. 19.

⁶⁰*Ibid.*

⁶¹*Stargazers, op. cit.*, 114-115.

⁶²*Ibid.*, p. 115.

⁶³*Ibid.*

⁶⁴*Stargazers, op. cit.*, p. 120.

standard was to become a very familiar one: if organized science speaks of it, it is true, but if an outsider like Velikovsky proposes it, it is ridiculous.

7. When the book appeared, a well-organized (and expensive) campaign was set in motion to discredit it, a "full-scale, public offensive aimed directly against Velikovsky."⁶⁵

"In the next few months, 'a surprising number of the country's reputable astronomers descended from their telescopes to denounce *Worlds in Collision*,' to quote the *Harvard Crimson* . . . Newspapers around the country were barraged with abusive reviews contributed by big-name scientists; some of these writings were syndicated to ensure better coverage."⁶⁶

One of the outraged scientists was Otto Neugebauer, an authority on ancient astronomy, who wrote an attack of Velikovsky for the journal *Isis* that was afterwards "mailed far and wide in reprint form"⁶⁷ at the expense of his institution. He called the book "a 389-pages-long list of absurdities,"⁶⁸ but, much more aggressively, he said the book was a "crackpot publication" which "attains, however, an exceptionally high degree of distortion of scientific literature."⁶⁹ That is to say, Velikovsky is *accused of lying*. The specific charge is that Velikovsky altered his source by substituting 33'14" for 3'14" in a quotation from Franz Xavier Kugler. To Neugebauer this was heinous, and Velikovsky agrees.

"The reader must say: 'Velikovsky magnified the difference between the two systems tenfold.' And since Neugebauer twice quoted Kugler, in German and in English . . . the impression must be very damaging."⁷⁰

It turns out, however, that it is Neugebauer who is wrong, for Velikovsky quoted only the correct value. That is to say, Neugebauer misrepresented Velikovsky. What is more significant, however, is that, when it was Velikovsky who was alleged to have falsified his data, Neugebauer considered it a capital sin, whereas, when it was Neugebauer himself *who distorted his source*, he passed it off as a "simple misprint of no concern."⁷¹ The falsehood was never corrected *nor the charge withdrawn*, neither in *Isis*, nor in the reprint, which continued to be "circulated by an interested group long after its errors had been pointed out."⁷² It became part of standard scientific literature.

These first events, distressing as they may appear, and wholly unscientific as they were, were only the beginning, mere shadow boxing. The main event began seven weeks after *WIC* appeared and began its rapid climb up the charts. Velikovsky was suddenly called to a meeting with George Brett, president of Macmillan, who asked Velikovsky *to release him from the publishing contract*, even when *WIC* was his No. 1 book, and allow it to be transferred to Doubleday. Why? "Tremendous pressure is being exerted against our company by a group of

⁶⁵Vorhees, *op. cit.*, p. 19.

⁶⁶de Grazia, *op. cit.*, p. 31.

⁶⁷de Grazia, *op. cit.*, p. 34.

⁶⁸*Stargazers, op. cit.*, p. 169.

⁶⁹*Ibid.*

⁷⁰*Stargazers, op. cit.*, p. 172.

⁷¹de Grazia, *op. cit.*, 34.

⁷²*Ibid.*

scientists."⁷³ Three quarters of Macmillan's business was textbooks, but Macmillan was being boycotted. "Professors . . . have refused to see our salesmen."⁷⁴ This, of course, was blackmail, and behind it were a number of scientific organizations and individuals banded together to force Macmillan to cease publishing Velikovsky: "in the textbook department they are alarmed by the violence of the opposition to your book."⁷⁵

Velikovsky was shocked, but Brett begged him to agree, and, after several days, he did. In June of 1950, only two months after the book had appeared, *and when it was the number one seller*, it was taken over by Doubleday, which subsequently went on to publish six more books by Velikovsky. This was the first step in Macmillan's ordeal of punishment and sacrifice before it could be re-admitted into the good graces of organized science again. Brett had hoped that it all would occur quietly, but *The New York Times* soon revealed most of the sordid details. "The greatest bombshell dropped on Publishers' Row in many a year exploded the other day," the article began.⁷⁶ Reference was plainly made to "pressure against Macmillan by an important segment of its customers—outraged scientists, teachers and textbook buyers,"⁷⁷ and that "Following some stormy sessions by the board of directors, Macmillan reluctantly succumbed, surrendering its rights to the biggest money-maker on its list."⁷⁸ Nothing like it had happened in American publishing before or since.

The next act of penance by Macmillan was almost inevitable, but totally undeserved. About two weeks after the book had been given up, James Putnam, associate editor at Macmillan, the man who had brought to the company its top seller, was fired as a sacrificial lamb after twenty five years of devoted and earnest service.

News of the book's transfer became almost as great a sensation as the book itself. Syndicated columnist George Sokolsky brought it to the attention of the entire nation, attacking the boycott and asking if Shapley was behind it. "Scientists tend to become dogmatic like theologians," he wrote, "assuming that anyone who does not belong to their particular trade union ought to be silenced . . . Macmillan owes the country an explanation."⁷⁹ The truth was now out, and science had to reply. One would think the scientists involved would hide in shame, but the opposite occurred: they angrily proclaimed their heroism. As an example of the indignant responses Sokolsky elicited, astronomer Paul Herget wrote to him, calling both him and Velikovsky frauds. "He is certainly a fraud, writing a book which is so obviously prejudiced and untenable, and calling it scientific."⁸⁰ Similarly, astronomer Dean B. McLaughlin wrote to Oursler at the *Reader's Digest* that *Worlds in Collision* was "a book that scientists confidently appraise as mere rubbish and the most flagrant intellectual fraud ever foisted on the public."⁸¹ Its errors were so manifest that "One could write a voluminous book . . . completely demolishing Velikovsky's thesis. I doubt that any scientist or group of them will waste their time that way."⁸² (He apparently fails to see the irony that a large group of scientists *had* wasted their time by trying to demolish the thesis without recourse to facts). Lastly, he takes pride in being part of the blackmail of Macmillan. "I am frank to state that this change was the result of pressure that scientists and scholars brought to bear on the Macmillan Company."⁸³ (It is to be noted that Velikovsky had seen a letter from McLaughlin to Brett several weeks earlier, in which he called Velikovsky a charlatan and said that the book was "all lies" and then admitted that he had not read it and never would).⁸⁴

⁷³*Stargazers, op. cit.*, p. 131.

⁷⁴*Ibid.*, p. 132.

⁷⁵*Ibid.*

⁷⁶*Ibid.*, p. 137.

⁷⁷*Ibid.*

⁷⁸*Ibid.*

⁷⁹*Ibid.*, p. 145.

⁸⁰*Ibid.*, p. 146.

⁸¹*Ibid.*, p. 147.

⁸²*Ibid.*, p. 148.

⁸³*Ibid.*, p. 149.

⁸⁴*Ibid.*, p. 133.

Macmillan gave in because of what Velikovsky calls a "campaign of collective letter writing" to blackmail the publisher into dropping *Worlds in Collision*.⁸⁵ We can get an idea of how extensive and frantic this campaign was from an anecdote reported by historian of science Livio Stecchini,⁸⁶ who was asked in 1950 by his department head to write a letter of protest against *WIC*. When he replied that he was not familiar with the book, he was instructed that the departmental secretary would compose the letter, and all he had to do was sign. This occurred all across the country in a frenzy of anger and indignation, as if Velikovsky were a disease that had to be wiped out. So intense was the communal outrage of organized science that *Newsweek*, in July of 1950, spoke out against "a small group of professors . . . accused of a major assault on academic freedom."⁸⁷ It said that "a boycott had actually been started against the company" because *Worlds in Collision* "had driven the vast majority of the nation's scientists into a highly unacademic fury,"⁸⁸ and reported that Leonard Lyons, of *The New York Post*, had named Shapley as the leader, for "Most of the attacks on Dr. Velikovsky sent to Macmillan had been from astronomers, and the bitterest had been from members of the Harvard Observatory."⁸⁹

As soon as Doubleday took over *Worlds in Collision*, it, too, began to receive threatening letters. Chemist David Grahame, for instance, warned that "you, too, may find yourself kept busy answering letters of indignation from scientists the country over . . . I trust that you can be dissuaded."⁹⁰ Astronomer Fred Whipple, (Shapley's assistant), accused Doubleday of acting at "a considerably lower ethical level than that of Macmillan"⁹¹ and refused to revise his textbook, *Earth, Moon and Planets*, because he did not want to be a fellow author with Velikovsky. (Velikovsky notes with amusement that, in the *Astronomical Journal* for October of 1950, Whipple postulated that a comet had collided with the asteroids in -2700 and again in 450).⁹² Once more, the double standard.

More embarrassing for Shapley, the student-edited *Harvard Crimson* proclaimed the boycott right in Shapley's front yard. Under the accusatory title "Shapley Brands 'Worlds in Collision' a Hoax," and the subtitle, "Scientists' Attacks, Pressure Make Macmillan Call Off Publication," it described the tumult which Velikovsky's ideas had generated and spoke of scientists reviewing a book they had not read, naming specifically Shapley and Payne-Gaposchkin, and supporting it with Thackrey's accusations against Shapley, (quoted above). It admitted that "The evidence tying Shapley to any organized boycott attempt . . . remains circumstantial,"⁹³ and it printed a signed denial by Shapley—"Several attempts have been made to link such a move to . . . some organization or to the Harvard Observatory. The idea is absolutely false"⁹⁴—but it concluded "That pressure had been exerted seems evident."⁹⁵ Shapley then tried crudely to turn the guilt against Velikovsky and Macmillan. "The claim that Dr. Velikovsky's book is being suppressed is nothing but a publicity promotion stunt. Like having a book banned . . . it improves the sales."⁹⁶

By this time combat was fully engaged. Velikovsky's book sold by the thousands and many people wrote to encourage him. A few even spoke up in his defense, notably Kallen, Larrabee and O'Neill, but the main body of organized science continued in its implacable hatred of the man and his books, which was expressed in a national campaign of criticism so venomous that a Doubleday editor called it "the most savage reviews a book of non-fiction

⁸⁵*Ibid.*, p. 147.

⁸⁶*Ibid.*, fn., p. 153.

⁸⁷*Ibid.*, p. 151.

⁸⁸*Ibid.*, pp. 148-149.

⁸⁹*Ibid.*, p. 152.

⁹⁰*Ibid.*, p. 153.

⁹¹*Ibid.*, p. 154.

⁹²*Ibid.*, p. 155.

⁹³*Ibid.*, p. 160.

⁹⁴*Ibid.*, p. 161.

⁹⁵*Ibid.*, p. 160.

⁹⁶Bauer, *op. cit.*, p. 25.

has received for a long time."⁹⁷ A noticeable characteristic of the reviews of *WIC*, in those first months after it was published in 1950, was the highly unscientific, very emotional and uncontrolled language of these scientific reviewers, as if they were so angry that they could only sputter with rage. And they also could not seem to agree just *why Worlds in Collision* was so horrible. The result was a series of outlandish attempts to compare Velikovsky to a staggering number of disreputable people and ideas, as if only by analogy to well-known crookedness and foolishness could the magnitude of his evil be described. Here are a few examples, *almost all of them written by scientists*.

1. In February, an astronomer and an engineer called Larrabee's presentation "a mixture of divination, studied ignorance, haruspices' palaver, and pseudoscientific half-truths, in other words . . . just plain hokum."⁹⁸
2. In March, the *Christian Science Monitor* decided that "Not since Captain Heinie Hasenpfeffer was reported sailing into New York harbor with a cargo of subways and artesian wells has there been a better candidate for P. T. Barnum's Hall of Fame . . . Baron Von Munchausen, Paul Bunyan, fairy stories and legends of Santa Claus are as entertaining as Velikovsky." The reviewer then added, ". . . if any reputable scientist comes forth publicly to back Velikovsky—I for one promise to . . . equip a safari to search for the sidehill wampus."⁹⁹
3. In April, the aforementioned scientist, Paul Herget, wrote in the *Cincinnati Enquirer* that if Velikovsky is going to talk of comets, he "might equally well insist that the State of Washington somehow rose up and threw a silver dollar across the Rappahannock River."¹⁰⁰
4. The reviewer of the *San Francisco Chronicle* called the book "scientifically worthless and at the same time boring" and said "We should expect better scholarship of a senior at the university."¹⁰¹
5. Astronomer Frank Edmondson, in the *Indianapolis Star*, called *WIC* "unquestionably the most outrageous collection of nonsense since the invention of the printing press . . . It is annotated clap-trap." He was equally furious at Atwater, whose "(. . . empty-headed!!) discussion was given wide circulation" and who was guilty of being quoted on the book jacket, whom the scientist said "is just as big a screwball as Velikovsky."¹⁰²
6. Astronomer Frank S. Hogg, in the *Toronto Globe and Mail*, wondered why Velikovsky, in his use of legends, "has not accompanied the Ute legend of cottontail by tales of Henny-Penny, Humpty-Dumpty, or even Paul Bunyan and his blue ox, Babe."¹⁰³
7. In the *New Yorker*, literary critic and author Alfred Kazin offered a different view. He called *WIC* "preposterous and intellectually primitive in the extreme . . . a pathetic, ominous, and superstitious piece of work"¹⁰⁴ whose purpose was to legitimize arguments for a monolithic world order.

⁹⁷Vorhees, *op. cit.*, p. 17.

⁹⁸*Ibid.*

⁹⁹*Ibid.*, p. 18.

¹⁰⁰*Ibid.*, p. 19.

¹⁰¹*Ibid.*, p. 20.

¹⁰²*Ibid.*, p. 22.

¹⁰³*Ibid.*, p. 24.

¹⁰⁴*Ibid.*, p. 25.

8. In the newspaper *Truth Seeker*, the reviewer did not agree. He said *WIC* was "full of preposterous prevarication" and was acclaimed only because it proved Biblical narratives to be true. "It is Buck Rogers out of fundamentalism."¹⁰⁵
9. Harvard astronomer Donald Menzel, (an associate of Shapley's), writing in *Physics Today*, wondered why Velikovsky had not included in his evidence the myths of Paul Bunyan and Babe, or Hey Diddle-Didle, in which the cow is the comet and the dish is a flying saucer which uses the spoon as a paddle.¹⁰⁶
10. Philosopher Thomas McTighe, in *Best Sellers*, wrote that "the American publishing enterprise has been set back at least twenty-five years with one publication of a senseless piece of work." He did not feel that *WIC* upheld the Bible, but was "a sly and cunningly contrived attack against the entire Judaeo-Christian heritage of God and the truth" and advised that it should be placed on the Catholic "Index of Prohibited Books."¹⁰⁷
11. Mathematician and watchdog of science, Martin Gardner, in the *Antioch Review*, called *WIC* "a tissue of absurdities" and compared Velikovsky to Wilhelm Reich, Ignatius Donnelly and L. Ron Hubbard, author of *Dianetics*.¹⁰⁸
12. L. Sprague de Camp, a science-fiction writer, also compared Velikovsky to Donnelly, and added Hoerbiger, and called *WIC* "a farcial farrago of preposterous amphegory."¹⁰⁹
13. Geologist Harrison Brown, in the *Saturday Review of Literature*, called *WIC* "a shining example of book and magazine-publishing irresponsibility." He was particularly angered "by the irresponsible publicity" the book had been given, but saw the reason for it: "the book . . . bodes good only to those on the receiveing end of the cash line."¹¹⁰
14. The same accusation of greed was made by geology specialist Kirtley Mather, (also of Harvard), who wrote "If [*Worlds in Collision's*] publishers had announced it as a "science-fiction thriller" under the title "Forever Venus," there would have been no basis for adverse criticism."¹¹¹ (He was referring to *Forever Amber*, a popular erotic novel of the time, also published by Macmillan). Neugebauer also decided that the motive was financial—"The Macmillan Company can congratulate itself on having found a very effective method for extracting money from a wide public"¹¹²—and the prestigious journal *Science*, (published by the AAAS), announced that it had held off attacking Velikovsky for a year because it was aware "of the financial success which often accompanied some well-meaning denunciation of unworthy . . . works."¹¹³
15. Scientist John Pfeiffer, in *Science*, (July, 1951), "ranked Velikovsky's book with 'Grimm's fairy tales and the *Rubaiyat*'"¹¹⁴ and called for a representative "body of American science" to speak out against it.

¹⁰⁵*Ibid.*

¹⁰⁶*Ibid.*, p. 27.

¹⁰⁷*Ibid.*, p. 28.

¹⁰⁸*Ibid.*

¹⁰⁹*Ibid.*, p. 30.

¹¹⁰*Ibid.*, p. 24.

¹¹¹*Ibid.*, p. 27.

¹¹²*Ibid.*, p. 28.

¹¹³*Ibid.*, p. 31.

¹¹⁴*Ibid.*, p. 32.

16. British historian J. B. S. Haldane said, "I could write as convincing a book . . . to prove that monkeys had originated from men" and called Velikovsky a "successful hoaxer," and astronomer Otto Struve most inelegantly relegated *WIC* to "the screwball fringe of science."¹¹⁵

These attacks on Velikovsky were so obviously unscientific and emotion-laden and irreconcilable among themselves that even a populist newspaper like the *New York Daily News* editorialized against them.

"If we might presume to offer the scientific brotherhood a tip, it would be to get busy trying to disprove Velikovsky with facts and figures and lay off trying to promote boycotts aimed at his book."¹¹⁶

Organized science, however, was so unable to heed this advice that, *more than a year later*, mathematician J. S. Miller explained in *Harper's* why he had switched to Velikovsky's side.

"The glaring paucity and the barren weakness of explicit criticism . . . have impressed me. There have been vitriolic and abusive utterances filled with fever but amazingly bare of fact."¹¹⁷

These are preposterous and self-demeaning acts by people who are not normally preposterous or self-demeaning. They are betrayals of the standards of science, and we have to wonder why; but I shall postpone the answer to that question until my last chapter. What is of interest to us now is to note that these people, almost all of them established scientists, are crying out in *moral* indignation at the *evil* of Velikovsky's book, almost as if it were the Middle Ages and he was a heretic who had to be exposed, stopped and punished. There is nothing scientific about their responses. It is blind emotion propelled by outrage.

The second anomalous and disturbing syndrome to appear in the Original Affair, (and which has continued to this day), is equally unscientific. It is the distressing sequence of events in which Velikovsky is criticized in print by scientists, often quite wrongly. He then writes a careful reply, pointing out each of the errors of the critics, but only the attacks were published. Velikovsky's rejoinders were not, and the false accusations alone remain in the literature, and become part of the lore of organized science, not merely uneradicated, but unchallenged. They keep being quoted as if they had never been exposed. This is not merely the construction of bad science, it is suppression of debate, denial of the right to reply and blockage of access to the ears of the scientific community. It is totally contrary to the ideals of science, (being more typical of a totalitarian dictatorship), but that is how organized science, for the most part, has handled Velikovsky—with intolerance and uncivility and high-handed fanaticism. Here are a few examples of this phenomenon.

- a. In 1950, at its annual meeting, the American Association for the Advancement of Science, (hereinafter the AAAS), organized a panel discussion on publishing responsibility, chaired by speech specialist Warren Guthrie and including geologist Kirtly Mather of Harvard and several publishers' representatives. They sought a method to regulate the publication of books so as to make them "acceptable to the scientific fraternity."¹¹⁸ Among the proposals were a review board, a set of very firm editorial principals and/or a jury of the author's peers, to prevent what Velikovsky did, who "bypassed astronomers and geologists and went straight to the general

¹¹⁵Bauer, *op. cit.*, p. 23.

¹¹⁶Vorhees, *op. cit.*, p. 27.

¹¹⁷*Ibid.*, p. 31.

¹¹⁸*Stargazers, op. cit.*, p. 188.

public."¹¹⁹ Velikovsky was held up as someone who had become successful with the gullible masses because he had evaded the judgment of his peers, and this must not be allowed to happen any further. (It was a session about censorship). The panel understood, however, that book publishers have to sell books, and "even the most arrant nonsense might occasionally justify publication—even as does a *Forever Amber* or *Anthony Adverse*," (both published by Macmillan). But serious works would have to be written either by accepted science writers or scientists, or by approved ghost writers who would adhere to the Club rules and beliefs, but never by a maverick like Velikovsky, who does not follow the rules.¹²⁰ Velikovsky, himself, of course, who was the cause and topic of the meeting, was not invited to hear or respond. It was a trial without the defendant allowed to be present, and the prosecutor was also the judge.

- b. At the same meeting, quite appropriately, Macmillan completed its ordeal of punishment. Representative Charles Skelley, completely glossing over the blackmail by scientists, spoke of his company "voluntarily" giving up its rights to "a book that the panel regarded as unsound . . . at heavy financial loss." Velikovsky described it quite correctly as confession and penalty, "the public castigation of a publisher,"¹²¹ and it seems to have worked. Skelley had Confessed, Putnam had paid the Penance, Macmillan had made its Atonement and expressed its Contrition, and now it could be Absolved by the AAAS, (of whom Harlow Shapley was an officer).
- c. A symposium entitled "Some Unorthodoxies in American Science" was organized by the American Philosophical Society in April of 1952, two years after the publication of *WIC* and significantly just before Velikovsky's second book, *Ages in Chaos*, was to appear. There was to be a paper on Velikovsky by Payne-Gaposchkin, and Velikovsky attended the meeting with his wife and O'Neill. The first speaker was I. Bernard Cohen, (historian of science, Harvard), who referred to "an inertia of the mind, or a resistance to change, or a kind of scientific orthodoxy," conditions which "prevent scientists from accepting the "logical" consequences of their own discoveries."¹²² He then spoke of Velikovsky in this light, and said (as O'Neill reported) that "The degree of violence with which a new idea is rejected by scientific orthodoxy may prove to be an index of its importance."¹²³ A more aggressive (and far less scientific) tone was adopted by Payne-Gaposchkin, (also of Harvard), whose remarks were directed straight at *Worlds in Collision*. This was her third attack on Velikovsky, and it "was no less venomous than her earlier ones."¹²⁴ In it, she "repeated most of her earlier arguments" and then misrepresented Velikovsky's quotations to make it appear that he had distorted his evidence.¹²⁵ She ended with the totally inappropriate unscientific statement, "His supporters imagine that we are shaking in our shoes. This is partly true: we *are* shaking, but with laughter."¹²⁶ Finally, psychologist Edwin G. Boring, (also of Harvard), further attacked Velikovsky, making him "the sole target of his humor."¹²⁷ (It was a carefully-integrated Harvard show). Velikovsky was then given half an hour to respond, which he did, point by point. As Velikovsky recounts it, "My answer was directed to astronomers,

¹¹⁹*Ibid.*, p. 189.

¹²⁰*Ibid.*, pp. 190-191.

¹²¹*Ibid.*, p. 188.

¹²²*Ibid.*, pp. 247-248.

¹²³*Ibid.*, p. 249

¹²⁴Vorhees, *op. cit.*, p. 34.

¹²⁵de Grazia, *op. cit.*, p. 41.

¹²⁶*Stargazers, op. cit.*, p. 253.

¹²⁷*Ibid.*, p. 254.

geologists, and historians . . . I made it clear that the conflict is not between my theory and astronomical facts, but between astronomical facts and the teachings of astronomers."¹²⁸

When the APS published the proceedings of that meeting, however, Velikovsky's response was *not* included, but a paper by Donald Menzel, (also of Harvard), *was* published, even though Menzel did not speak at the symposium. Naturally, Menzel attacked Velikovsky: "If Velikovsky wants quantitative discussion, let us give him one."¹²⁹ As it turned out, Menzel's attempt to refute Velikovsky was a disaster,¹³⁰ and led him into controversy for a dozen years; but the point here is that Velikovsky was refused permission to respond to the attacks of Payne-Gaposchkin in the pages of the APS, and was not even told about the Menzel paper. The members of the Society who received the publication got only the Harvard side of the debate.

- d. For ten years, Velikovsky had consulted with Robert H. Pfeiffer, Chairman, Department of Semitic Languages, Harvard, and Curator, Semitic Museum, on his historical reconstruction, which was to occupy four books. Although Pfeiffer did not fully agree with Velikovsky, "he was unfailingly benevolent" and wrote of the first book in 1949 that "If Dr. Velikovsky is right, this volume is the greatest contribution to the investigation of ancient times ever written."¹³¹ When that first volume, *Ages in Chaos*, was published in 1952, several remarks by Pfeiffer appeared on the dust jacket, authorized by him. Within two weeks, Shapley wrote to Pfeiffer, putting pressure on him to recant his support. The threat involved a prestigious meeting soon to be held at Harvard "before the members of the faculty and the intelligentsia of the campus,"¹³² at which Shapley had been asked (or had asked) to comment "on Velikovsky, the dowsers, and the wave of credulity."¹³³ Pfeiffer was offered an escape—"The statement is pretty obviously out of context"—and he was asked to confirm this for Harvard, "so that unfair conclusions will not be drawn." He was also told what to do if he had been misquoted, "whether you are inclined to protest." Pfeiffer, however, resolutely upheld Velikovsky, and shortly afterward received an angry letter from astronomer Edwin Carpenter, who wanted to know if he "really intended to support the new book with the weight of his own professional judgment," thereby bringing the world of book publishing to its "ethical nadir."¹³⁴ Admirably, Pfeiffer the *historian* remained undaunted by both of these *astronomers*, neither of whom ever asked him why in his professional capacity he believed that Velikovsky's *historical* ideas should receive "objective investigation."¹³⁵ They simply tried to quash the book.
- e. Velikovsky also discussed his theories with Albert Einstein from 1953 to 1955. They had known each other since the 1920's, but renewed their acquaintanceship when both lived in Princeton and Velikovsky sought his advice on matters relating to celestial mechanics. As a result, Einstein read *Worlds in Collision* and *Earth in Upheaval* and also Velikovsky's talk to the Princeton Graduate College Forum, 1953, entitled "Worlds in Collision in the Light of Recent Finds in Archaeology, Geology, and Astronomy," which meant he was up-to-date on discoveries after 1950 which supported *Worlds in Collision* and was interested in debating them: "often he [Einstein] asked me

¹²⁸*Ibid.*, p. 256.

¹²⁹Vorhees, *op. cit.*, p. 34.

¹³⁰de Grazia, *op. cit.*, pp. 41-42, 55-56, 59-60.

¹³¹*Stargazers, op. cit.*, p. 259.

¹³²*Ibid.*, p. 262.

¹³³*Ibid.*, p. 269.

¹³⁴*Ibid.*

¹³⁵de Grazia, *op. cit.*, p. 23.

not to go away when it was late, but to spend more time in discussion."¹³⁶ In 1955 Einstein read *Stargazers and Gravediggers*, where the Affair is exposed, and made a number of marginal notations. After reading the text, he felt "that Shapley's behaviour could be *explained* but in no way *excused*."¹³⁷ He then commented, with good humor, that he admired "the straightforwardness of Thackrey [sic] who has compelled the roaring astronomical lion to pull in to some extent his royal tail without fully respecting the truth."¹³⁸ Einstein liked Velikovsky's terrestrial evidence—"the historical arguments for violent events in the crust of the earth are quite convincing"¹³⁹—but he disagreed with his planetary explanation. When, however, radio noises were detected coming from Jupiter, as Velikovsky had predicted, Einstein was impressed and offered to help get certain tests performed.

Shortly afterward, Einstein died and an interview given just before his demise was published in *Scientific American*. (The interviewer, curiously enough, was I. Bernard Cohen of Harvard). In it, Einstein appeared to refer to *Worlds in Collision* very derogatorially, regretting "that scientists in the U.S. had protested to publishers about the publication of such a book" because "Such a book," Einstein is alleged to have said, "could not do any harm . . . Left to itself, it would have its moment . . . and that would be the end of it."¹⁴⁰ He was also said to have called its author "crazy."¹⁴¹

Velikovsky was "hurt" and "angered" and wrote a long and detailed letter to Cohen, describing his many interactions with Einstein, (the letters, the notes, the discussions), and asked Cohen to publicly revise his account of what Einstein said. He even invited Cohen to consult Velikovsky's files on Einstein at Princeton, but Cohen never did. So slanted was the interview that Einstein's executor wrote a letter to *Scientific American*, disowning Cohen's remarks. "As executor of Einstein's estate and as one who has the responsibility to protect his scientific and literary interests, I feel compelled to say that I deeply regret Professor Cohen's statements."¹⁴² In reply to *this*, Cohen retreated, but only partially.

"Professor Einstein . . . was speaking in general terms about the above-mentioned issue and was using the book only as an example . . . there is no basis for concluding that Professor Einstein might not have had a friendly feeling for the author . . . or . . . some interest in his work."¹⁴³

Nevertheless, Cohen never printed Velikovsky's letter or recounted the details of the Velikovsky-Einstein discussions in *Scientific American*, and, therefore, mainstream science never heard of them. It took nearly thirty years until Velikovsky published *Stargazers and Gravediggers*, the book he had been withholding at the request of his wife since the mid-50's, for mainstream science to finally read of those discussions.

¹³⁶*Stargazers, op. cit.*, p. 289.

¹³⁷*Ibid.*, p. 290.

¹³⁸*Ibid.*, p. 291.

¹³⁹*Ibid.*

¹⁴⁰*Ibid.*, p. 298.

¹⁴¹*Ibid.*

¹⁴²*Ibid.*, p. 302.

¹⁴³*Ibid.*

- f. The next incident also involves suppression by *Scientific American*. In 1956, Velikovsky published *Earth in Upheaval* and the publisher, Doubleday, tried to place an ad for it in that magazine. The request was absolutely refused. The book, however, was reviewed by the magazine when it appeared, and the reviewer was atmosphericist Harrison Brown, who six years earlier had dismissed *Worlds in Collision* as being so full of errors "that he estimated would fill a letter 'thirty pages in length.'"¹⁴⁴ Brown, who was not any more qualified to review the new book than he had been to evaluate the first one, ignored it—"He did not mention a single datum from it. Nor did he assail or refute a single statement. He was still in the emotional state created by my *Worlds in Collision* . . . and he frankly admitted that he 'boils'."¹⁴⁵ Much worse, he accused Velikovsky of creating false impressions by innuendo, especially about the suppression of *Worlds in Collision* and the alleged interest of Einstein, and to prove it he quoted from *Ages in Chaos* and *Earth in Upheaval*. (We must remember that these were perhaps the two most damaging accusations made by Velikovsky's supporters, and, therefore, the ones that mainstream science would be most eager to dismiss). The problem is that in his quotes, Brown carefully omitted Velikovsky's citations and proof to make it look as if Velikovsky had none, and then accused him of that. When the omitted passages are restored, as Velikovsky does in *Stargazers and Gravediggers*, Brown's purpose becomes clear. He is trying very hard to refute Velikovsky's allegation that, in Brown's own words, "The scientists . . . have organized themselves into a sort of anti-Velikovsky club which is extremely powerful and which cajoles or threatens all persons who look favorably upon Velikovsky's theories."¹⁴⁶ Brown's flagrant duplicity, of course, merely proves Velikovsky's point. But Velikovsky was never given the opportunity to refute Brown in *Scientific American*, and, as a result, mainstream science continues to believe only in the portrait Brown painted.
- g. Larrabee wrote a reply to Brown which was published in *Scientific American*, in which he placed little value on Brown's review. He rejected Brown's assertion that the cause of "the over-emotionalism of scientists about Velikovsky is the amount and nature of the publicity" he received, saying that the true cause "seems to me to lie in the nature of the challenge Velikovsky offered," and that Brown "does not review the new Velikovsky book . . . instead, he offers us a description of his own mental processes."¹⁴⁷ Velikovsky also wrote a reply to Brown, specifically on Brown's attempt to trivialize Velikovsky's relation with Einstein, which his editor at Doubleday, Walter Bradbury, called "a wonderful answer and I hope it is printed exactly as written." The letter was sent to Denis Flanagan, editor of *Scientific American*, because "it was now *Scientific American's* second innuendo on the subject"¹⁴⁸ and because Flanagan had already personally seen Velikovsky's files concerning Einstein and knew the truth, but of course Flanagan refused to publish it. Velikovsky did not let the matter rest, but sent him a copy of *Earth in Upheaval* and asked him to decide for himself if Brown's accusations were false, and was finally told by Flanagan that, because "your books have done incalculable harm to the public understanding of what science is and what scientists do,"¹⁴⁹ *Scientific American* would say nothing in favor of Velikovsky, nor would it criticize any attack upon Velikovsky. "Thus," wrote Velikovsky, "I was not given a chance to answer where it mattered—in the magazine that published the accusations."¹⁵⁰

¹⁴⁴de Grazia, *op. cit.*, p. 32.

¹⁴⁵*Stargazers, op. cit.*, p. 312.

¹⁴⁶*Ibid.*, p. 313.

¹⁴⁷*Ibid.*, p. 315.

¹⁴⁸*Ibid.*, p. 316.

¹⁴⁹*Ibid.*, p. 317.

¹⁵⁰*Ibid.*, p. 316.

- h. The same situation continued in the 1960's, with an added characteristic: each time Velikovsky was mentioned favorably, or even *mentioned*, organized science leaped to the attack in an attempt to demean him. It is a phenomenon that persists, recurring in 1974, 1980 and 1990, as the chapters in this book will illustrate, but we will begin with 1963 and the publication of Eric Larrabee's "Scientists in Collision" in *Harper's*. In it, Larrabee reviews the record of the many successful predictions by Velikovsky and the confirmatory discoveries of the space age. "Science," Larrabee sums up, "has been heading in Velikovsky's direction" and therefore "His dismissal and suppression by the scientific community require of scientists an act of agonizing reappraisal."¹⁵¹ This aroused astronomer Donald Menzel to action, (who had replaced Shapley as Director of the Harvard College Observatory). He at once composed a "highly emotional essay," sent it to *Harper's*, and then replaced it "with a version less abusive to Larrabee and more abusive to Velikovsky."¹⁵² Interestingly, it contained the same far-fetched attempt at guilt by association that we had seen in the 1950's— "Velikovsky," said Menzel, "has been as completely discredited as was Dr. Brinkley of the goat-gland era."¹⁵³

Menzel may have been stung by Larrabee's pointing out a monumental error he had made, where he had calculated that the sun would have to have a potential of 1019 volts for Velikovsky to be right, which Menzel the Harvard astronomer said was impossible. Larrabee had pointed out that an Australian physicist, V. A. Bailey, had calculated *just this* potential on the sun. (*Nature*, 1960) Menzel was embarrassed and wrote to Bailey, asking him to recant his calculations, *not because they were wrong*, but because they were "casting doubt on the continuing efforts of Menzel and other American scientists to discredit Velikovsky."¹⁵⁴ The response by Bailey was indignantly negative: he would not abandon his careful work merely "to accomodate the anti-Velikovsky forces."¹⁵⁵ What is more, he wrote a rebuttal to Menzel, pointing out "a simple arithmetical error in Menzel's calculations, which invalidated his argument."¹⁵⁶ The reader may guess the outcome— *Harper's* refused to print Bailey's remarks unless they were reduced to a letter, while "Menzel was permitted to correct the arithmetical error . . . without acknowledging the effect of the correction on his argument."¹⁵⁷

As for Menzel's attempt to refute Velikovsky's successful predictions, Larrabee exposed his errors regarding *every one* of them in what Juergens called "a classic example of the demolition of a scientists' arguments by a non-scientist."¹⁵⁸ Bailey added to Menzel's discomfiture in his letter to *Harper's* the next year, when he chided the Harvard scientist for holding "certain out-of-date views about the material contents of interplanetary space" and warned people like him "to adopt a cautious attitude towards the astronomical ideas on which they were reared." The destruction of Menzel was completed by Lloyd Motz, an astronomer whom Velikovsky had consulted and who had written a very supportive letter to *Science* in 1962. Motz did not agree with Velikovsky, but nevertheless considered him "a serious and dedicated investigator."

¹⁵¹ de Grazia, *op cit.*, p. 54.

¹⁵² *Ibid.*, p. 55.

¹⁵³ *Ibid.*

¹⁵⁴ *Ibid.*, p. 56.

¹⁵⁵ *Ibid.*

¹⁵⁶ *Ibid.*

¹⁵⁷ *Ibid.*

¹⁵⁸ *Ibid.*, p. 58.

"His writings should be carefully studied and analyzed because they are the product of an extraordinary and brilliant mind, and are based upon some of the most concentrated and penetrating scholarship of our period."¹⁵⁹

Velikovsky, however, was never allowed to speak for himself on this point in this magazine or in any of the ones controlled by the interlocked network of mainstream astronomy.

- i. Again and again, Velikovsky attempted to call the attention of organized science to recent discoveries about space which supported his theories and his successful predictions. In 1964, he sent a letter to *Science* editor Philip Abelson, to put these developments in a positive light, but Abelson's reaction was typical—he "returned Velikovsky's paper without reading it and published instead a facetious letter from a Poul Anderson, who claimed that 'the accidental presence of one or two good apples does not redeem a spoiled barrelful.'"¹⁶⁰ (Anderson was a science *fiction* writer). Undaunted, Velikovsky wrote a new article, "Venus, a Youthful Planet," again containing much new evidence in his favor, and geologist Harry Hess, President, American Geological Society, recommended that it be published in the *Proceedings* of the American Philosophical Society (or APS). After close to half a year, Hess was told that the APS Publications Committee was "split into two belligerent camps." It was then thought to publish the paper as a letter, to "permit publication without implying approval," but this, too, was rejected. The committee then sought the advice of new readers "outside the circle of Mr. Velikovsky's critics," but, even after their approval was obtained, the paper was finally refused.¹⁶¹ In the end, it was never published, in whole or in part, by the APS.

In the same year, Velikovsky was advised to present his arguments in the *Bulletin of the Atomic Scientists*, a more philosophical journal, but the editor Eugene Rabinowitch saw the situation just as Abelson had, deciding that "a widespread reawakening of interest in Velikovsky's theories . . . required remedial action." Consequently, rather than give space to Velikovsky, or even equal space, Rabinowitch assigned reporter Howard Margolis to write a demolition piece, the "vulgar and thoroughly irresponsible article, 'Velikovsky Rides Again,'" which was "filled with misrepresentation and misquotations, jeers and sneers, bald statements of unfounded charges, and dogmatic presentations of received theory as fact."¹⁶² Margolis' article contained so many errors that Alfred de Grazia, on behalf of the journal *The American Behavioural Scientist*, demanded that the *Bulletin of the Atomic Scientists* publish a retraction. Rabinowitch finally agreed "to print an article presenting the views of Velikovsky, should it be written and submitted by a scientist of standing."¹⁶³ The very upstanding Prof. Harry Hess submitted "Venus, a Youthful Planet," the article which had been refused by the APS, but Rabinowitch did the same as the APS; he broke his promise and rejected it for the specious reason that "the *Bulletin* is not a magazine for *scientific* controversies," and because, (even though it was Hess who recommended the paper), "not a single qualified scientist has raised his voice in favor of Velikovsky's theories."¹⁶⁴ Juergens adds as we would expect that "Rabinowitch admitted that he had not read Velikovsky's books."¹⁶⁵ In the same year de Grazia, in *The American Behavioural Scientist*, catalogued 54 "points of ignorance and

¹⁵⁹*Ibid.*, p. 60.

¹⁶⁰*Ibid.*, p. 51.

¹⁶¹*Ibid.*, pp. 61-63.

¹⁶²*Ibid.*, pp. 64-65.

¹⁶³*Ibid.*, p. 67.

¹⁶⁴*Ibid.*, p. 68.

¹⁶⁵*Ibid.*, p. 69.

misrepresentation" in Margolis' article, charged that he "misquoted or misrepresented . . . the works of Dr. Velikovsky," and called for public admission of those errors—"Since you are wrong in fifty-four ways already, it ill behooves you to increase your score."¹⁶⁶ No retraction of Margolis' blunders, however, or acknowledgement of Velikovsky's merits, ever appeared in the *Bulletin*. Its readers only got the view published by the editor, and that became the "truth" about Velikovsky.

- j. When Delta wished to announce that *Worlds in Collision* and Velikovsky's second book, *Earth in Upheaval*, would appear in paperback, *Scientific American* and *Science* both "refused to publish the ad."¹⁶⁷ Later, when one of Velikovsky's books was unfavorably reviewed in *Scientific American*, that magazine refused to publish a rebuttal by Velikovsky.¹⁶⁸ Similarly, after *Science* had published a scurrilous retort to a letter of support on Velikovsky's behalf by the scientists Bargmann and Motz, it "would not print Velikovsky's response."¹⁶⁹ The editor wrote,

"At least half of Velikovsky's ideas have been proved wrong and he has done little to substantiate the remainder. In view of this, he is not to be taken seriously."¹⁷⁰

No list was given showing which "half" of Velikovsky's ideas were in error, nor was any mention made of Velikovsky's continuous attempts "to substantiate the remainder" in articles that the mainstream magazines refused to publish. His letter was simply rejected because the magazine *knew in advance* that Velikovsky was wrong (and *where* he was wrong) and, therefore, censored his letter *because he could not be right*. Four years later, the Franklin Institute invited Velikovsky to lecture in Philadelphia, but "the Rittenhouse Astronomical Society . . . refused to permit its room to be used. Much was made of this in the local press, and Velikovsky's talk was given in the Free Library, across the street."¹⁷¹ Yet again, when the Jet Propulsion Laboratory reported on the findings of its Venus probe Mariner II in *Sky and Telescope*, (published by Harvard), it "deleted four pieces of data . . . items that could be seen as lending plausibility to Velikovsky's claims."¹⁷² Lastly, when *Newsweek* in 1963 planned "to call attention to Velikovsky's predictions and their fulfillment by Mariner II," the article "was abandoned following a telephone conversation between a *Newsweek* editor and Harlow Shapley."¹⁷³ This is, as Juergens wrote, merely part of a pattern in which "Influential scientists continue to exert pressure against any sort of favorable mention of Velikovsky."¹⁷⁴

- k. Lastly, there is the story of the venerable British Museum and "Velikovsky's attempts to obtain C-14 dates of Egyptian objects."¹⁷⁵ At issue was Velikovsky's contention that many alleged

¹⁶⁶*Ibid.*, p. 70.

¹⁶⁷*Ibid.*, p. 71.

¹⁶⁸Bauer, *op. cit.*, p. 58.

¹⁶⁹*Ibid.*, p. 56.

¹⁷⁰*Ibid.*, p. 59.

¹⁷¹*Ibid.*

¹⁷²*Ibid.*, p. 58.

¹⁷³de Grazia, *op. cit.*, p. 74.

¹⁷⁴*Ibid.*, p. 73.

¹⁷⁵Bauer, *op. cit.*, p. 70, taken from *Pensée* 6.

dynasties and monarchs of ancient Egypt have to be moved hundreds of years closer to the present. Ever ready to have his theories tested, Velikovsky predicted that wood pieces from the tomb of Tutankhamon, when measured by the Carbon-14 (or C-14) process, should show dates about 500 years later (more recent) than conventional theory. "Velikovsky . . . had stated his expectation that . . . short-lived materials from Tutankhamon's tomb should yield dates ca. 1840 B.C.," said *Pensée*.¹⁷⁶ When the samples were tested by the British Museum, they "were dated to ca. 846 B.C. (BM 642 A) and ca. 899 B.C. (BM 642 B) respectively."¹⁷⁷ The results, however, which powerfully confirmed Velikovsky and went directly against mainstream theory, were "never published by the British Museum." Why? Bruce Mainwaring, of *Pensée*, was told by the director of the laboratory "that results which deviate substantially from what is expected are often discarded."¹⁷⁸ When Dutch physicist G. W. van Oosterhout tried to obtain the results, he was told by the British Museum that its C-14 "laboratory has made no measurements on material from the tomb of Tutankhamen."¹⁷⁹ Oosterhout, accordingly, wrote to Mainwaring, "This is much worse than what you said. Deviating results are not only not published, it is even denied that they have been found."¹⁸⁰

These are typical examples from the Original Velikovsky Affair, to which Bauer adds details from the 1970's, (part of the Continuing Affair), which, however, we can quickly review here because they demonstrate the same animosity. For instance, when the pro-Velikovsky journal *Kronos* published *Velikovsky and Establishment Science*, (1977), a special issue put out directly in response to the AAAS's *Scientists Confront Velikovsky*, (1977), *Science News* refused "to carry advertisements for that book and for Ransom's book *The Age of Velikovsky*."¹⁸¹ (Shapley had by then passed away, but his influence at that publication seems to have persisted). In the same way, "*American Scientist*, *Sky and Telescope*, and *Scientific American* had refused to accept advertisements for *Pensée*."¹⁸² (These magazines were all controlled by the Harvard-oriented network). The explanation given by *Scientific American* for its decision was that it had objected to ads about Velikovsky because no scientist "finds any interest whatever in anything he has to say"¹⁸³ (*i.e.*, it was censorship in advance). Bauer, therefore, concludes that there was a "virtually monolithic opposition to Velikovsky in academia in the 1950's,"¹⁸⁴ and that "there is ample evidence that a number of scientists sought to prevent Velikovsky from publishing books and articles and even from giving lectures."¹⁸⁵ What Bauer says is true and important. I find fault with Bauer, however, when he then tries to get above it all by saying that "Intemperate language, *ex cathedra* statements, unsupported generalizations, argumentation *ad hominem*, evasiveness, and sophistry are to be found aplenty on both sides."¹⁸⁶ This, I feel, is a grossly unjust, almost senseless observation, for no one has cited even one instance of any Velikovsky supporter who did the dirty, sleazy, lying things done to Velikovsky by the likes of Menzel, Brown, Margolis, Cohen, Struve, Herget, Payne-Gaposchkin or Shapley, all of them scientists, most of them from Harvard, and none of them able to act in an ethically professional or scientific manner when it came to Velikovsky.

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¹⁷⁶Bauer, *op. cit.*, p. 79.

¹⁷⁷*Ibid.*

¹⁷⁸C. J. Ransom, *The Age of Velikovsky*, (Glassboro, N.J., 1976), p. 174.

¹⁷⁹*Ibid.*

¹⁸⁰*Ibid.*

¹⁸¹Bauer, *op. cit.*, p. 78.

¹⁸²*Ibid.*

¹⁸³*Ibid.*, p. 79.

¹⁸⁴*Ibid.*, pp. 64-65.

¹⁸⁵*Ibid.*, p. 59.

¹⁸⁶*Ibid.*, p. 62.

Thus ends our run-through of the first phase of the Velikovsky Affair. There is no doubt that these things happened, for the proof is undeniable. The question that must come to our minds, however, is this—*Why* did they occur? I am not speaking merely of petulance, insult or irritation, or even prejudice, for *that is not what happened*, but of burning hatred, acts of dishonesty, and implacable malevolence. Velikovsky's opponents lied, cheated, faked and tried to destroy not merely a man's works but his reputation and dignity. They misrepresented, distorted, and misinformed, used innuendo, sarcasm, and guilt by association, to assassinate his character, with almost no reference to his actual ideas. This is not merely not scientific, it is a betrayal of every ideal of science *which these people have been taught*, and that is what we have to try to explain.

Shall we merely call them scum and pass it off as that? I do not think so. We cannot simply attribute these evils to character, as if *all* of the people who behaved abominably in the Velikovsky Affair were abominable. While there *are* bad apples in every barrel, some of Velikovsky's attackers were probably not arrogant, ruthless bigots in daily life, even though that is how they behaved when it came to Velikovsky. Such an answer is not sufficient: we have to seek a solution in the *condition*, not in the individuals who constitute it. The problem we have to deal with, therefore is more *social*, and it is this: how could it be that a large number of sane, adult, generally conservative men and women, highly trained in science, not only behaved like insane childish animals in the Velikovsky Affair, but felt, "to a man," that their insane childish behaviour was not merely permissible, but obligatory and actually laudable? This is what is problematic, that in their minds, what they did was not dirty and dishonest, but a good and righteous thing.

Consider the evidence. (1) Velikovsky said in a lecture in 1973 "that *Worlds in Collision* "caused an excitement that no other book in the history of science did cause," provoking more than 4,000 articles."¹⁸⁷ This is far from normal. It indicates a vast lack of control. (2) When Harrison Brown wrote an inept and misleading review of Velikovsky's *Earth in Upheaval* for *Scientific American* in 1956, Eric Larrabee, of *Harper's*, asked why, "Despite their repeated assertions that he will soon be forgotten, scientists seem unable to leave Velikovsky alone."¹⁸⁸ Brown answered that "he continues to write books, and this, in effect, compels us not to leave him alone,"¹⁸⁹ but we must wonder what the root of the compulsion really is. (3) The *Harvard Crimson*, a student newspaper, put its finger on the problem as early as 1950.

"In a world where crackpot scientific theories appear and pass unnoticed every day, some began to wonder: If there is nothing to Dr. Velikovsky's thesis, why were so many people trying to discredit and silence him?"¹⁹⁰

The *Crimson* offered the expectable answer that

"Dr. Velikovsky draws his proofs from a wide range of fields and sciences . . . Hence, if his theories, or any large part of them, are found to be valid, scientists in a great many fields will have to change the underpinnings of their life's work."¹⁹¹

This is true, but again we have to look deeper. We have to ask more specifically just what *sorts* of underpinnings will have to be changed. An indirect indication is given by Velikovsky himself, who wrote about the

¹⁸⁷Vorhees, *op. cit.*, p. 15.

¹⁸⁸*Stargazers, op. cit.*, p. 315.

¹⁸⁹*Ibid.*

¹⁹⁰*Ibid.*, p. 159.

¹⁹¹*Ibid.*

years 1955-1956 "In four out of eleven issues in the space of eleven months, *Scientific American* had dedicated its columns to me. Nobody kicks a dead dog, says the proverb."¹⁹²

Our working question, then, becomes this: Why cannot Science simply ignore Velikovsky? Why does it call for battle stations every time he is mentioned? Why does it feel he must be forever suppressed? I think that our best clue is to be found in the *irrationality* of science's response to Velikovsky, and in this instance, it is a question of the *degree* of irrationality. That is to say, after we admit that most of the responses by Shapley *et al.* were emotional and irrational, we have yet to deal with the extremity of the irrationality. For instance, even though astronomer Harlow Shapley denied any role in the Affair, saying it would be wrong to have done that—"I didn't make any threats and I don't know anyone who did"¹⁹³—fellow astronomer Paul Herget, almost as if he "were jealous because Shapley was getting all the credit,"¹⁹⁴ was not at all ashamed of *his* intellectual blackmail, but boasted with pride "I am one of those who participated in this campaign against Macmillan," and of Shapley he said, "I do not believe he was in any sense the leader in this campaign. I was a very vigorous participant myself."¹⁹⁵ Similarly, astronomer Dean B. McLaughlin wrote that he had tried to destroy *Worlds in Collision* because it was "more than an attack on science, it is an attack on reason, especially it is a boomerang attack on religion!"¹⁹⁶ He, therefore, assailed the book *as a moral duty*, because "All of us who write have a very genuine responsibility to the public."¹⁹⁷ Ransom observes that, when deeply-believed ideas are placed in jeopardy, "During these irrational moments, some scientists feel that, *at any cost*, the public should be shielded from untruths."¹⁹⁸ The beleaguered scientists re-confirm their beliefs by wrapping them in the mantle of *duty*. McLaughlin, for example, also calls *WIC* a fraud, (he seems unable to make up his mind what the book's worst fault is), and "It is our duty to the public to prevent such fraud."¹⁹⁹ Similarly, when Yale geologist Chester Longwell was asked why he had attacked *Worlds in Collision* in the *American Journal of Science*, (which he happened to edit), and why the attack had also appeared under huge colored headlines in his local paper, the *New Haven Register*, (hardly a scientific publication, but one which might readily bow to the wishes of a major scientist in its area), he replied that he had given "attention to such patent nonsense" because "our chief concern is to focus attention on the publisher."²⁰⁰ That is to say, Macmillan had to be stopped *in any way possible*. This is panic.

A much more open accusation of the book's *menace to society* was made at a meeting of the AAAS in 1950, where, at a panel discussion convened especially to discuss the problem of Velikovsky, the chairman, Warren Guthrie, called *Worlds in Collision* a great "danger to civilization."²⁰¹ The same attitude of horror-struck panic about Velikovsky was displayed by Payne-Gaposchkin, who was *frightened* by his success. "The public reaction isn't funny," she wrote. "It is rather terrifying, a sort of intellectual mass-hysteria."²⁰² So many people were being fooled by his ideas, she said, that "we are in an age of intellectual catastrophe,"²⁰³ and it *must be brought to a stop*. (She appears to have missed the irony of referring to Velikovsky as a "catastrophe.")

Everywhere there are intimations of *disaster* if people believe Velikovsky, and this is our best clue, for it helps us to really explain the violent excesses of the Affair. It is not merely indignation, it is not merely self-protection, nor laziness, nor inertia. The answer, I think, is this: if in his ideas about recent planetary catastrophes

¹⁹²*Ibid.*, pp. 315-316.

¹⁹³de Grazia, *Ibid.*, p. 29.

¹⁹⁴Ransom, *op. cit.*, p. 8.

¹⁹⁵*Stargazers, op. cit.*, p. 148.

¹⁹⁶*Ibid.*

¹⁹⁷*Ibid.*

¹⁹⁸Ransom, *op. cit.*, p. 234.

¹⁹⁹*Stargazers, op. cit.*, p. 149.

²⁰⁰Vorhees, *op. cit.*, p. 27.

²⁰¹Quoted in *Stargazers, op. cit.*, p. 191.

²⁰²Vorhees, *op. cit.*, p. 23.

²⁰³Vorhees, *op. cit.*, p. 17.

Velikovsky is a fraud and a liar and a menace to society, he is *evil and dangerous*, and, therefore, nothing is dishonorable in the battle against him and his concepts. We must remember that America had just finished an all-out war in Europe and Asia, where it was desirable to kill and spy on and demean the "Japs" and the "Nazis" and go after unconditional surrender, and now Science, (the true new Spirit of America), was in the same kind of war against Velikovsky. Consequently, anything went in the pursuit of victory. The goodness of the cause cleansed every battle act of moral taint, and that is how these people were able to behave disgustingly and not be disgusted by their behaviour: science was engaged in a do-or-die moral battle against the utter wickedness of Velikovsky, and all was fair. In my last chapter, we shall ask (and answer) *why* it was that science had to see the situation in this way, *i.e.*, what very deep and primitive emotions led it to construct this self-freeing and self-forgiving (not to mention self-glamorizing) view of itself *vis-à-vis* Velikovsky. For the moment, we shall be content to note the presence of this immense polarity—Science is very, very good, and Velikovsky is unutterably horrid.

That insight, I believe, brings us a step closer to grasping the real nature of the Velikovsky Affair. It explains why the acts of Velikovsky's scientific detractors consistently belie their own words and ideals about science, why they are completely unable to overlook him and why they can only picture him as irredeemably evil. Velikovsky for example has given us instance after instance of a seeming two-facedness on the part of his opponents: Shapley preaches open-mindedness, Brown urges fairness, Rabinowitch insists upon open hearing, Abelson advocates a constant tolerance for radical new ideas, yet in virtually the next breath they all utterly violate these principles with regard to Velikovsky. Where he is concerned, gone are respect, civility, generosity or due consideration, to be replaced by impoliteness, scorn, vituperation, exasperation, closed-mindedness and rejection out of hand. These are very unscientific acts performed by very scientific people in defiance of their own scientific principles concerning such behaviour, and on the surface the dichotomy between what they did and what they said cannot be rationalized: these people consistently and irresistibly betrayed their own self-professed standards. They were intemperately, obliviously hypocritical.

I do not think, however, that they can merely be dismissed as hypocrites. On the contrary, most of them probably believed sincerely in the general concepts of fair play that they spouted. The problem, I think, is that they did not feel that these values were to be applied universally. One *did* have to be polite and fair and open-minded with fellow scientists because they had earned such treatment, being members of the right club, but one did *not* in any way owe the same treatment to a person like Velikovsky, *because he was the enemy*. The evidence indicates that in their minds, (as in the mind of the medieval Church), the world was divided into watertight compartments labeled "Good" and "Bad," and the bad did not deserve justice, honesty or even mercy. That is why, (just like the Church), there was no conflict in their scientific consciences about their brutal behaviour toward Velikovsky. He was a Heretic beyond the Pale, he did not merit the ideals, and consequently no ideals were betrayed. There was, therefore, no need for any scientist to feel guilt at being involved in the dishonesty, misrepresentation, character smear or blackmail which occurred. On the contrary, (just like the medieval Church), these scientists felt and confessed a great joy and rightness and necessity in how they behaved toward Velikovsky, as if the dirty, dishonorable, demeaning things they did were a moral obligation, and God (or at least the god of mainstream science) were looking down upon them and approving. *They could not be evil*. Nay, as the quotations I have cited indicate, they were heroes, saving society and protecting its best interests. That, strange as it seems, is how mainstream science ultimately saw itself with regard to Velikovsky, as Defender of the True Faith. It is bizarre, it is perverted, it is not reasonable, it is certainly not scientific, and this is the problem we will try to solve.

* * *

A solution of a certain kind is provided by the new sociology of science, where science is no longer approached "apologetically," (that is, in awe as the source of unquestionable truth), but is correctly treated merely as organized social interaction. I will apply these sociological insights extensively in my final chapter, but a glimpse of some of the central points now may help the reader to understand *sociologically* the puzzling excesses of the scientists who attacked Velikovsky. It appears that the problem lies in the way that scientists are trained to relate to the world. Sociologist W. O. Hagstrom, for instance, describes the very narrow and constrictive ideological

environment of scientists. Scientific research, he says, is a gift donated by the individual, and there is, "as in all gift-giving, the expectation of return gifts (of recognition)."²⁰⁴ As a result,

"The desire to obtain social recognition induces the scientist to conform to scientific norms . . . it also influences his selection of problems and methods."²⁰⁵

This sort of subservience, of course, "tends to produce individual conformity"²⁰⁶ and an overwhelming group mind:

". . . scientists all over the world are fairly-well agreed as to what further investigations . . . would be appreciated or not . . . These men practically form a unit. It is a relatively small community . . . and modern methods of communication have knit it into one [Erwin Schrödinger, quoted by Hagstrom]."²⁰⁷

Within this tightly-controlled circle and the territory it rules, deviants are restrained by powerful "Sanctions to enforce community"²⁰⁸ and by the intense socialization of the individuals, which

". . . produces scientists who are strongly committed to the values of science and who need the esteem and approval of their peers."²⁰⁹

People like these are not very open to radical or disturbing ideas, and could tend to react against them as one.

Sociologist Michael Mulkay specifically uses the Velikovsky Affair to discern how a commitment to science's communal values and a desire for science's institutional esteem are what is truly important to scientists. In his study of the reception of Velikovsky's ideas, he readily admits that Velikovsky "broke the rule of communality" by going directly to the public, "But this relatively minor infringement . . . cannot account for the violence of scientists' responses." The true reason, he feels, was "Velikovsky's rejection of those theoretical and methodological paradigms accepted at that time . . . Velikovsky's massive departure . . . led many scientists into an emotional rebuttal."²¹⁰ It was as if Velikovsky's book, which not only "challenged basic theoretical assumptions in a number of disciplines" but "put in question the essential methodology of modern science,"²¹¹ was seen as a *moral crime* against science, and the normal sociological results as Mulkay lists them are a litany of the Affair.

"As a consequence of Velikovsky's nonconformity . . . Shapley and others felt justified in abrogating the rules of universalism.

²⁰⁴W. O. Hagstrom, "Gift-Giving as an Organized Principle in Science," *Sociology of Science*, ed. B. Barnes, (Harmondsworth, Eng., 1972), p. 106.

²⁰⁵*Ibid.*, pp. 110-111.

²⁰⁶*Ibid.*, p. 112.

²⁰⁷*Ibid.*

²⁰⁸*Ibid.*, p. 113.

²⁰⁹*Ibid.*, p. 117.

²¹⁰Michael Mulkay, "Cultural Growth in Science," *Sociology of Science*, ed. B. Barnes, (Harmondsworth, Eng., 1972), p. 128.

²¹¹*Ibid.*, p. 132.

"They judged the man instead of his work, and in this way failed to live up to the demands of organized skepticism, for they did not subject Velikovsky's claims to rigorous examination before assessing the validity of these claims.

". . . the scientific community also failed to allow its members unrestricted access to the information presented by Velikovsky. Thus the rule of communality was infringed.

"The same disposition to restrict dissemination of Velikovsky's ideas can be seen in the . . . sanctions against the publishers of *Worlds in Collision* Not only did these actions violate norms of communality but they also infringed the individual scientist's right to make up his own mind."²¹²

Mulkay, therefore, calls the Velikovsky Affair a "widespread failure within the scientific community" to live up to its own proclaimed norms, which to him "demonstrates a basic flaw in the functional analysis" of science which is predicated upon those norms.²¹³

"According to the functionalist approach we should have predicted that Velikovsky's work would have been subjected to detailed and critical examination As we have seen, this did not happen. Instead we find extensive deviation."²¹⁴

Like Hagstrom, he attributes this "deviation" (*i.e.*, the excesses of the Affair) to the faults of scientific education and socialization, which in his opinion produce "rigidity rather than flexibility," fixed mind sets and, therefore, "cognitive rigidity."²¹⁵ The cause of the Velikovsky Affair is, therefore, Velikovsky's violent intrusion into this closed world, which produced such extreme "dissonance"²¹⁶ that science became concerned at all costs to remove it.

"It is clear that widespread consideration of Velikovsky's claims . . . would have generated extreme dissonance for many scientists. This possible development was avoided in a number of ways."²¹⁷

Mulkay then lists *as a normal consequence* all of the "negative responses" to the threat of Velikovsky which we have noted, (pre-publication rebuttals, refusal of access to journals, dismissing of supporters, sanctions against publishers and refusal to test predictions), plus explaining away his predictions "as the luck which any charlatan is likely to enjoy."²¹⁸ What he presents as the Velikovsky Affair is therefore an instance of terrified, instinctive, emotional group reaction to a threat to its most cherished self-image, in which individual scientists

²¹²*Ibid.*, pp. 128-129.

²¹³*Ibid.*, p. 130.

²¹⁴*Ibid.*, pp. 130-131.

²¹⁵*Ibid.*, pp. 131-132.

²¹⁶*Ibid.*, p. 132.

²¹⁷*Ibid.*, p. 133.

²¹⁸*Ibid.*

responded in a blind, tribal fashion, jettisoning standards and ideals in their panic to eliminate the attacker. That, says Mulkay, is why they did what they did, and also why they could not see that it was wrong. It *is* a very accurate picture, but we must, nevertheless, keep in our minds the anterior question—What was it *really* that was placed in danger by Velikovsky? Was it the norms alone? Is that why mainstream science reacted with such anger and outrage? Or could it have been the *picture of the world* created by those norms?

* * *

The 1960's witnessed more of the same, because of three explosive developments. First among these was the remarkable success of Velikovsky's predictions, especially because they arose directly out of his theories and, therefore, *appeared to lend them credence*. Here are ten examples:

1. Velikovsky said that space is not a vacuum, but is filled with magnetic fields and electrical charges on interplanetary bodies. Beginning in 1960, it was discovered first that the Earth, and then nearly every celestial body and system, has a magnetosphere, and that they act and interact.
2. Velikovsky argued for axial tilts and pole shifts of the Earth. Considered impossible in 1950, today it is merely a question of how many times these events have occurred.
3. Velikovsky said that some of the oil found in the Earth is not terrestrial and billions of years old, but extra-terrestrial in origin and only a few thousand years old. No geologist agreed in 1950, but today it is considered sound speculation.
4. Velikovsky dated the most recent Ice Age to 3500 (not 35000) years ago, and said it began and ended suddenly. This too has since been fully confirmed.
5. Whereas most cosmologists in 1950, (including all of Velikovsky's critics), expected Venus' surface temperature to be close to that of the Earth's, Velikovsky said it would be very hot. Recent probes indicate it is near to 1000° F.
6. Velikovsky also said that Venus could rotate differently from the rest of the planets. It was discovered in 1962 that it has a retrograde (or backward) spin, leading one scientist to ask if "Venus was created apart from the other planets . . . perhaps in a collision of planets."
7. Velikovsky said, as a result of his vast re-dating of ancient history, that the hitherto-undeciphered Minoan B script would be an early form of Greek, something forbidden by mainstream dating. In 1953, this was confirmed, to the astonishment of the world.
8. Velikovsky said that Jupiter is not a cold and inert body, but is active enough to produce radio noise. This was discovered in 1954 and announced in the newspapers and at scientific meetings in 1955. Shapley himself called it the major discovery of the year, but science had no explanation for it.²¹⁹
9. Because the Moon had to have been heavily affected by the catastrophes, Velikovsky predicted that the Moon's rocks would show *remanent magnetism* "induced . . . by an external magnetic field."²²⁰ "Several months before . . . *Nature* published a note stating that no remanent magnetism was expected,"²²¹ but it was found.

²¹⁹*Stargazers, op. cit.*, pp. 293-295.

²²⁰Ransom, *op. cit.*, p. 143.

²²¹*Ibid.*

10. In the realm of archaeology and ancient history, Velikovsky made a number of very radical predictions whose success may be perceived in Bauer's terse summary: "evidence of early human culture *had* been found in Siberia . . . pre-Columbian civilizations in Mexico *were* a thousand years older than previously believed; archaeological digs in the Middle East *had* found that "the entire area was shaken and devastated . . . by natural disasters of a scope and severity which modern experience has nothing to compare."²²²

The record is astonishing. It has been set forth here, however, not to try to prove that Velikovsky must be totally or partly correct because of them, (for such questions are not the point of this book), but rather to illustrate *how mainstream science has reacted to them*. As you might by now expect to hear, the response was typically phobic. Essentially, the general body of scientists was kept ignorant of these developments because the important scientific publications tried never to mention them. Whenever news about them did emerge, of course, the response of mainstream science to Velikovsky's advance predictions was sad and predictable. Even though each one was a logical product of some part of Velikovsky's overall catastrophic theory, able to be drawn as a reasonable consequence directly from his data and speculation, mainstream science adopted the consistent and loudly-proclaimed attitude that these predictions did not in any way prove, or give validity to, or even lend the slightest credence to, Velikovsky's ideas, but were merely "lucky guesses" *unconnected* to the theories. (They never considered, *in terms of pure probability alone*, how many lucky guesses a man might chance to make if his ideas were totally foolish). To cement this attitude in the public mind, an organized campaign of vituperation and sarcasm was instituted to separate the predictions from any relevance to the theories, and soon in every mainstream science magazine they were dismissed as "lucky hits," "ad hoc guesses" or mere nonsense. One radio astronomer wrote "that even Velikovsky is entitled to a "near miss" once in a while,"²²³ (missing the irony that Velikovsky's whole theory is predicated upon a cometary near-miss), while Menzel of Harvard, for example, almost foamed at the mouth at the inability of the public to discern such obvious error.

"Since the idea is wrong, any seeming verification . . . is pure chance. It has no more validity than do his thousands of other erroneous suppositions."²²⁴

We are not told which these thousands of suppositions are, nor is any proof offered of their wrongness, there is merely the universal assertion that *everything* about Velikovsky, every single statement, is unutterably untrue. This is violent, angry intellectual assassination, and, as long as it succeeded, mainstream science must have felt that it had once more put the lid on Velikovsky.

Tempers were exacerbated, however, when several well-known individuals in science refused to go along with the suppression of Velikovsky's success and spoke out on his behalf. These included Horace Kallen, (Dean, New School for Social Research), who had first defended *Worlds in Collision* in the *Saturday Review of Literature* in 1951.

"There is a widespread and dangerous disposition to consider science as in some sense holy In the life of the mind the communicants of such a religion of science figure as so many more dogmatists of another intolerant cult

"A current instance of this . . . is the aggression against the original publishers of Velikovsky's *Worlds in Collision*."²²⁵

²²²Bauer, *op. cit.*, p. 50.

²²³Ransom, *op. cit.*, p. 113.

²²⁴Bauer, *op. cit.*, p. 45.

²²⁵Stargazers, *op. cit.*, p. 285.

In the 1960's, he again pleaded for due respect for Velikovsky's ideas, (*Yale Scientific Magazine*, 1967), whether or not "his approach and conclusions might be scientifically unacceptable,"²²⁶ and in 1973, in *Pensée 1*, in an article entitled "Shapley, Velikovsky and the Scientific Spirit," he exposed for the first time many of the precise particulars of Shapley's underhandedness. A second was Harry Hess, Chairman, Space Science Board, National Academy of Sciences, who had also followed the successes of Velikovsky's predictions all along. Hess had tried unsuccessfully in 1963 and 1964 to get a record of them published by the American Philosophical Society and then by the *Bulletin of the Atomic Scientists*, (above) and finally in 1964 he catalogued them in *Harper's* in what physicist V. A. Bailey called "the impressive testimony to the worth of Dr. Velikovsky's predictions contained in the recent letter of that outstanding scientist, Prof. H. H. Hess of Princeton."²²⁷ In 1956, Hess had tried to get a number of Velikovsky's revolutionary proposals to be tested during the International Geophysical Year, but that was refused, although later, as Velikovsky notes, Hess "was pleased to let me know that the claims the IGY would not investigate were confirmed by independent research."²²⁸ In the 1960's, Hess wrote to Velikovsky

"Some of these predictions were said to be impossible when you made them. All of them were predicted long before proof that they were correct came to hand. Conversely I do not know of any specific prediction you made that has since proven to be false."²²⁹

The most notable intervention, however, was an open letter written to the very prestigious magazine *Science* by physicist Valentin Bargmann and astronomer Lloyd Motz, who called attention to the confirmations of Velikovsky in a way and in a place which science could no longer ignore or merely trivialize. "One after another of Velikovsky's 'wild hypotheses' have achieved empirical support," wrote Juergens in 1966, "but not until December 1962, in the Bargmann-Motz letter to *Science*, was his name ever linked in the pages of scientific journals with any of these 'surprising' discoveries."²³⁰ Giving precise evidence of what Velikovsky had said and when, the letter asked, in de Grazia's words, for a recognition of

"Velikovsky's priority of prediction of the hot surface temperature of Venus, of the existence of the magnetosphere around the Earth, and of the radio noises emanating from Jupiter."²³¹

Their letter ended

"Although we disagree with Velikovsky's theories, we feel impelled . . . to establish Velikovsky's priority of prediction of these . . . points and to urge, in view of these prognostications, that his other conclusions be objectively re-examined."²³²

As Juergens notes, "The Bargmann-Motz plea for scientific good sportsmanship won no response in the journals of science."²³³ Velikovsky, nevertheless, was so encouraged by the letter that he, "on January 29, 1963,

²²⁶Bauer, *op. cit.*, p. 54.

²²⁷de Grazia, *op. cit.*, p. 59.

²²⁸*Stargazers, op. cit.*, p. 326.

²²⁹de Grazia, *op. cit.*, p. 177.

²³⁰*Ibid.*, p. 52.

²³¹*Ibid.*, p. 176.

²³²Bauer, *op. cit.*, p. 42.

²³³de Grazia, *op. cit.*, p. 55.

submitted to *Science* magazine a more complete presentation of recent empirical evidence of the correctness of some of his statements."²³⁴ This was the article "Additional Examples of Correct Prognosis," which contains massive support for his theories from the latest discoveries at that time in astrophysics, planetary atmospheres, petrology, lunar science, geophysics, archaeology, ancient history and oceanography. *Two days later*, "On January 31, the article was back in his hands with a formal letter of rejection."²³⁵ Even after Bargmann and Motz, the publications of mainstream science refused to mention the name of Velikovsky.

What was worse, Larrabee, too, upheld Velikovsky in print, (in *Harper's*, 1963), pointing out a large number of his successful predictions, not merely three, which prompted the watchdog Menzel to more irrationality—"the methods advocated by Velikovsky and Larrabee represent a return to the dark ages. They no more represent science than the practice of voodoo represents medicine."²³⁶ Menzel never bothered to point out just *how* Larrabee's attempt to shine light on this issue was a retreat to the dark ages, nor, (more to the point), how his attempt to obscure the success of the predictions was a step into the light. The attempt at smear by association (Velikovsky equals voodoo) was the extent of his substantive remarks, and its pathetic and feeble emotionalism was matched by Isaac Asimov, who wrote the profound retort that "any set of nonsense syllables placed in random order will make words now and then."²³⁷ No effort was shown by these blindly antagonistic people to seriously consider Velikovsky's ideas even for a moment. On the contrary, they acted as if the theories were not simply wrong, but had to be attacked and extirpated at the slightest mention. (It is, of course, up to the reader to choose which side practised "voodoo" and retreated to the totalitarianism and viciousness of the "dark ages").

In very sharp contrast to the puerile simpleness of Menzel and Asimov was the rational, objective behaviour of Lloyd Motz, even after he had been reprimanded by the astronomical fraternity for offering *any* support to Velikovsky. In reply to this castigation, Motz, in a letter to *Harper's* the following year, reiterated his original skepticism about Velikovsky, but affirmed with more vigor than before what the correct course for mainstream science should have been.

"I do not support Velikovsky's theory but I do support his right to present his ideas and to have these . . . considered by responsible scholars and scientists as the creation of a serious and dedicated investigator."²³⁸

Would that Shapley or Menzel or Asimov had been able to rise to this appropriate realm of behaviour, but they could not. The result of Motz' second letter, however, in terms of the Velikovsky Affair, was that Velikovsky's successful predictions had now become an accepted (and uncomfortable) piece of knowledge in the intellectual world for perhaps the first time. Up to then, every major science publication had refused to print either Velikovsky's presentation of this evidence or discussion of it by his supporters, thus effectively *keeping this knowledge suppressed*; but after that it was out, and it was a major defeat for Shapley's side, not to be contained by any merely childish attempt at damage control.

The second major development in favor of Velikovsky in the 1960's was the spectacular intervention of the journal *The American Behavioural Scientist* (or *ABS*). The editor, Alfred de Grazia, had first heard the name of Velikovsky from historian of science Livio Stecchini, but did not become interested until he read and was entranced by Velikovsky's *Oedipus and Akhnaton*. A meeting with Velikovsky about the suppression of his ideas was soon arranged, (all three lived in Princeton), and de Grazia was given the opportunity to view Velikovsky's "archive of materials on the case. It was astonishingly rich and ordered."²³⁹ After studying the files carefully and discussing them with Velikovsky, de Grazia "concluded . . . that the history of science had few, if any, cases that were so well

²³⁴*Ibid.*, p. 177.

²³⁵*Ibid.*

²³⁶Bauer, *op. cit.*, p. 43.

²³⁷*Ibid.*, p. 51.

²³⁸*Ibid.*, p. 53.

²³⁹de Grazia, *op. cit.*, p. 3.

documented."²⁴⁰ The result was an *American Behavioural Scientist* special issue on what it came to call the Velikovsky Affair. de Grazia enlisted a panel of nine distinguished readers to review the articles written by himself, Stecchini and Ralph Juergens and then published the special issue in September, 1963 which as expected incurred lively and sometimes acrimonious debate, for here was a lot of the dirt out in the open. Three years later, the original material, plus additional articles by Juergens, Stecchini, and Velikovsky himself, was published as a book, *The Velikovsky Affair*, which was an intellectual bombshell. "Because of the eminently successful campaign of defamation in the 1950's," wrote Juergens, "the name Velikovsky became anathema among editors and science writers But the article by Larrabee in *Harper's* for August 1963 and the special issue of the *American Behavioural Scientist* in September 1963 initiated a fermentation process in scholarly circles and on college campuses." He adds with regret, however, that this new interest in Velikovsky "up to now, has been unreflected in either the general or the scientific press."²⁴¹

The reason that the *ABS* special issue, and then *The Velikovsky Affair* which grew out of it, created a sensation is because in their pages was material presented by Juergens and de Grazia which contained many blatant examples of the devious and underhanded behaviour of organized science. It also presented strong evidence by Stecchini that the "Affair" was a disease of long-standing in Western culture, and that much of science was actually in Velikovsky's favor, an argument also made powerfully by Velikovsky's own account of the success of his predictions. The documentation was undeniable, and it led de Grazia in his "Introduction to the Second Edition" to this conclusion:

"It is difficult for someone, in the face of the evidence offered [sic], to contradict the book's two main ideas: that Dr. Velikovsky was unjustly treated, and that he maintains a set of propositions that must be seriously considered by the sciences and the humanities."²⁴²

De Grazia states that in 1963 he "was first drawn to the sociological and legal aspects of the Velikovsky Affair," and frankly admits that in 1966 he was "much more committed intellectually to Dr. Velikovsky's approach than I was when this material was first published,"²⁴³ but, nevertheless, he is confident he can objectively solve the problem of whether Velikovsky's book was wrong "or was there lurking in it an alternative model of cosmogony?"²⁴⁴ His two-part answer is firm, and its support of Velikovsky's theories defines why organized science was upset by the book:

". . . there is no 'fact' in the great and varied growth of today's science that is 'true' enough to block a complete cosmogonic model that is antithetical to uniformitarianism; there is enough of 'fact' to supply the construction of a revolutionist model."²⁴⁵

The cat was now well and truly out of the bag, and mainstream science had to start scrambling all over again (with less success) to put it back in.

The third important breakthrough of the 1960's was the flood of new data, especially that provided by the space probes, which denied the essentially 18th-century astronomy of Shapley and created a picture which lent possibility to Velikovsky. Readers unfamiliar with astronomy will not be aware how much has changed in that field

²⁴⁰*Ibid.*, pp. 3-4.

²⁴¹*Ibid.*, p. 78.

²⁴²*Ibid.*, p. 4.

²⁴³*Ibid.*, pp. 4-5.

²⁴⁴*Ibid.*, p. 5.

²⁴⁵*Ibid.*

from 1950 to the present, so it must be emphasized that the Velikovsky Affair *in its scientific dimension* began as a battle centered about the traditional astronomical beliefs current in 1950, which disfavored Velikovsky. Since the space age, however, the combat has shifted for what has been discovered since then favors Velikovsky and casts doubt upon the mainstream astronomy of 1950. That is to say, the battle in 1950 seemed to be (and was portrayed by science to be) a combat between The Truth, (which Science had), and errors of Ignorance put forth by Velikovsky, but that is now understood to be merely establishment propaganda by the side in power. Astronomy, geology and biology have changed so much since 1950, and changed always to the disadvantage of uniformitarianism, that with the help of hindsight we can now perceive the Affair in a much more objective light. It was not a struggle between black and white, but between a choice of fuzzy greys wrongly presented as black or white. Physicist C. J. Ransom expressed both points in 1976:

"... what were thought in 1950 to be facts were merely opinions, and Velikovsky challenged nearly every major opinion about the recent history of the Earth and the Solar System."²⁴⁶

We must reiterate, however, (because challenge by itself is not proof), that what tilted opinion anew toward Velikovsky in the 1960's was that the new discoveries of science had moved toward him and away from the "opinions" of 1950, and it is the appearance of so many of these items of scientific support at one time in the 50's and 60's that made Velikovsky interesting once again.

* * *

By the mid-1960's, therefore, a decade and a half after the Velikovsky phenomenon was supposed to die, here he was, stronger than ever. Science had tried its best in the 1950's, when his first four books had been published, to ignore him or demean him, hoping he would go away, and to a large extent it worked, as far as the general public was concerned; but now, suddenly, he was on everyone's lips again. The book *The Velikovsky Affair*, and the support by Larrabee and Bargmann-Motz and Bailey, and the new discoveries, (plus perhaps an altered intellectual climate), had brought the Velikovsky story to a new generation of readers, academics and students. In relatively short order, (to the distress of mainstream science), Velikovsky was resurrected. He became part of university courses; the Affair (even with great resistance) entered the permanent record of the history of science, and in the second half of the decade he was invited to speak at more than a dozen campuses and meetings and institutions across the U.S. Far from being eradicated in its first decade, catastrophism experienced a rebirth in its second decade, and this is what set the stage for the next phase of the battle, 1970-1990, which we, for convenience, we will call The Continuing Velikovsky Affair.

Two: The Continuing Affair

This brings us to the 1970's. I have called the period which begins here the *Continuing Velikovsky Affair* because of the different form which the battle between Velikovsky and mainstream science took. What I mean is this: in the first 25 years, (from 1945 to 1970), Velikovsky himself was the principal adversary and target of mainstream science, whereas, starting in the 1970's, he was no longer alone in the sense that there was now an organized, eloquent and active support group behind him. This does not mean that he was literally alone in the 50's and 60's. As I have already shown, he was offered frequent and long-lasting support by people like science editors John J. O'Neill and Eric Larrabee, by hard scientists like Horace Kallen and Harry Hess, by ancient history chroniclers and archaeologists like Robert Pfeiffer, Claude Schaeffer and Eugène Drioton, and then by the contributors to *The Velikovsky Affair*, political scientist Alfred de Grazia, engineer Ralph Juergens and historian of science Livio Stecchini. These people vigorously spoke up on Velikovsky's behalf, exposing the errors and

²⁴⁶Ransom, *op. cit.*, p. 11.

misbehavior of science, writing letters against unfair and lying reviews, and inviting him to speak at college campuses.

What was different in the 1970's was the appearance of a new generation of Velikovsky supporters, organized in a new way. The turning point, in my opinion, was the founding of the journal *Pensée* in 1971 by David and Stephen Talbott. They had decided to publish a special issue on Velikovsky, but soon realized that the topic was immense and ongoing, and the result was the landmark series of ten issues "Immanuel Velikovsky Reconsidered." In my opinion, it was this initiative, above all else, which gave new life to catastrophism; for within a short while a whole new group of supporters had clustered about the journal to form a coherent, well-qualified and aggressive unit such as Velikovsky had never seen before.

Included were not only the Talbotts, but many of the people who would play a major role in catastrophism since then. *Pensée* gave way in 1976 to *Kronos* under the editorship of Lewis Greenberg, which ran until 1988, and between them those journals brought to the fore such people as William Mullen, C. J. Ransom, Lynn Rose, Robert Bass, Israel M. Isaacson, (a Ph.D. candidate in archaeology at a major northeastern, American university who, because of violent pressure from his superiors, was forced to adopt this pseudonym), Frederic Jueneman, Earl Milton, Henry Zemel, Roger Wescott, Clark Whelton, Dwardu Cardona, Warner Sizemore, Jan Sammer and myself, as well as the late Zvi Rix, Antoinette Patterson, David Griffard, and Bronson Feldman. These people are specialists in the hard sciences, the social sciences, and the humanities, and I list them not to convince you that Velikovsky must be right, (for that, as I have said, is not the purpose of this book), but to indicate the size and competence of his new body of North-American supporters. Later in the decade, in England, the Society for Interdisciplinary Studies was founded under the impetus of Harold Tresman and has published continuously since its formation its journal, the *S.I.S. Review*, (renamed in 1987 the *Chronology and Catastrophism Review*), as well as its *Workshop* for research in progress. The *SIS* is particularly strong in archaeology and ancient history, boasting dozens of qualified contributors in those areas, and it has published important (sometimes quite revisionist) work in those fields. In 1989, the journal *AEON* appeared, edited by David Talbott, Dwardu Cardona and Ev Cochrane, and in 1993 Charles Ginenthal began to produce the journal *The Velikovskian*. As well, the historical journal *Catastrophism and Ancient History*, edited by Marvin Luckerman, was produced in California for about 15 years, and each of these publications has attracted new scholars in addition to those I have already listed.

The result is that, since 1971, a substantial group of catastrophist researchers was and is at work on Velikovsky-type problems, whereas he worked almost alone in the 1950's, and these journals have published hundreds of scholarly articles on catastrophism, which constitute a powerful body of evidence aside from the books that Velikovsky wrote. This did not exist in the first phase of the Affair. It must be admitted, however, that not all of this work has directly followed Velikovsky, for there are a number of interesting deviations being developed by some of his supporters, but I do not consider it strange that what Velikovsky initiated, just like the ideas of Freud or Marx, has gone far beyond the original formulation and taken some paths unenvisioned by the first theorist. It only means that Velikovsky's concept, like theirs, is very rich and fertile. Perhaps it is best, then, if we refer to the field beginning in the 1970's as *catastrophism*, of which Velikovsky's ideas are a seminal part, and say that a very major difference between the periods of the Original and Continuing Affairs is that in the first phase it was almost exclusively Velikovsky versus Science, whereas now, catastrophism in its totality has become an ongoing, burgeoning, vital discipline, involving many dozens of scholars, where debates may occur not only between mainstream and radical concepts, but even among branches of catastrophism itself.

In addition, 1981 saw the founding of the Canadian Society for Interdisciplinary Studies, (of which I am president), and it has been our task to hold an Annual Seminar every year at which ongoing projects are presented in an informal and highly productive manner, while a German society was founded in the 1980's under the guidance of Gunnar Heinsohn and Heribert Illig. Active groups also exist in Australia, (led by Wallace Thornhill), in Israel and in New York, where Henry Zemel and Clark Whelton run the Society for Historical Research, while an association of specialists in the sciences and arts functions on the west coast of North America, led by David Talbott.

It is therefore evident that, with the appearance of these journals and the formation of these different editorial and research groups, catastrophism was now on a new footing and the Affair had entered a different dimension. In particular, the battles no longer consisted mostly of sniping and counter-shots, (a sneer here, a rebuttal there), but of more extensive intellectual engagements, speaker against speaker, article against article, and even book versus book. This has generated different kinds of attacks on Velikovsky, and it is the history of this second phase, more formally enacted but no less embittered and unsavory than the first, which the following

chapters of this book will present. The events are often more drawn out, the issues perhaps larger and debated at greater length, but the depth of outrage and blind animosity is unfortunately the same.

The major battle in the 1970's was the American Association for the Advancement of Science (or AAAS) symposium on Velikovsky, held in San Francisco in 1974, which will be fully critiqued by Charles Ginenthal and Lynn Rose in later chapters of this book. To appreciate the depth of the emotions involved, however, requires a review of certain events prior to 1974, especially since 1965, which might have stimulated the animosity and apprehensiveness of mainstream science. They can be listed in short order.

- a) 1965, a Velikovsky Symposium is held at Brown University.
- b) 1967, a Velikovsky special issue of *Yale Scientific Magazine* is published, which looks at his ideas with less animosity than before.
- c) 1969, an article by Velikovsky is included in *The New York Times* special issue "Men Walk on Moon," published at the time of the historic first moon landing. Lynn Rose adds, however, that "his article was mysteriously pulled from the Late City Edition It may also be noteworthy that the microfilm version of *The New York Times* uses the *Late City Edition*."
- d) 1971, Velikovsky Conference in Switzerland.
- e) 1971, Velikovsky interviewed at length by the Canadian Broadcasting Corporation in Toronto.
- f) 1972, Velikovsky lectures at Harvard.
- g) 1972, Velikovsky lectures at SUNY-Buffalo and McMaster universities.
- h) 1972, Velikovsky lectures to NASA at its Ames Research Center.
- i) 1972, Velikovsky conference at Lewis and Clark College, Portland, Oregon.
- j) 1973, Velikovsky lectures at the IBM Research Center.
- k) 1973, Velikovsky lectures at the University of North Carolina.
- l) 1973, announcement of an honorary degree to be awarded to Velikovsky by the University of Lethbridge, Alberta, Canada, in 1974.
- m) 1973, announcement of a huge conference on Velikovsky to be held at McMaster University, Hamilton, Ontario, in 1974.
- n) Velikovsky invited to speak at the Duquesne History Forum, Pittsburgh, in 1974.
- o) Velikovsky invited to speak to the Philosophy of Science Association at Notre Dame, 1974.

As I have already said, prompted by the appearance of *The Velikovsky Affair* in 1966, and by a spate of successful scientific predictions by Velikovsky, and above all, by the new evidence of Solar System catastrophe being brought back by the space probes, there was a vigorous and widespread renewal of interest in Velikovsky in the last half of the 1960's. As this new list of lectures and symposia indicates, his popularity continued unabated into the 1970's, culminating in *four* symposia on his ideas in 1974 alone. To the defenders of the uniformitarian *status quo*, it must have seemed as if the tiger of catastrophism was at their very gates.

Mainstream reaction was not long in coming. Faced with the popularity of *Pensée*, with its great success at eliciting scholarly articles of high caliber, at annihilating puerile criticism, and at calling public attention to the merits of catastrophism, the august and ponderous American Association for the Advancement of Science (or the AAAS) decided to go into action to "take care of this Velikovsky thing." This is where the second segment of the *Affair* truly begins. There had been opposition to Velikovsky earlier in the 70's, but it had usually taken the form of scholarly debate, with articles against Velikovsky which appeared in various journals being countered by replies from pro-Velikoskians, often in the pages of *Pensée*. This is, of course, acceptable and desirable. It is how reasonable academic discourse should be conducted. The actions of the AAAS, on the contrary, were nothing like that. They were shameful and dishonest, as the account by Ginenthal clearly demonstrates; here we have the old familiar symptoms again, (animosity, trickery, outrage, scorn), showing that the disease of Velikovskyitis, which had afflicted mainstream science from 1945 to 1970, had not been eliminated, but had merely lain dormant and

would now reappear in all its old virulence. Ginenthal's essay lays bare the deviousness, the machinations and the downright fury of the AAAS, complete with lies, misrepresentation, character assassination and blockage of response.

The other chapters expose further instances of the same old story: the urge to annihilate the hysterical fear *that anyone might believe Velikovsky*, the compulsion *to keep catastrophism from being said*. David Talbott, founder and former co-editor of the journal *AEON* and author of *The Saturn Myth*, (1980), and Ev Cochrane and Dwardu Cardona, co-founders and present publishers of *AEON*, dismember in separate articles the short-sighted and misleading criticisms of Velikovsky's use of sources by Bob Forrest. Lynn Rose, professor of philosophy, raises new personal issues of emotionalism and bias surrounding the AAAS symposium. Charles Ginenthal, founder and editor of the journal *The Velikovskian*, and author of the exposé *Carl Sagan and Immanuel Velikovsky*, (1995), reviews the AAAS symposium and demolishes its pretence at objectivity or fairness. He then goes on in separate articles to critically evaluate a number of anti-Velikovsky pieces by different contributors which appeared in the magazine *The Skeptical Inquirer*. Devastatingly, he lays bare the ferocious anti-Velikovsky agenda which underlies them, orchestrated by the magazine's editor, Paul Kurtz, who displays a hatred of catastrophism and evidence of a uniformitarian bias much like that exhibited by Velikovsky's rabid critics of 1950. Among Ginenthal's targets are mathematician Martin Gardner, astronomer James Oberg, Chinese reference librarian Henrietta Lo and astronomer David Morrison. He invalidates the criticism and exposes the hostility of these people to show how *unskeptical* the publication really is. A skeptic, after all, is one who doubts that anyone has the truth, whereas a fanatic is sure he has the one, only and final truth. Ginenthal shows that, while Kurtz may be skeptical about Velikovsky, he is fanatical about mainstream science and, therefore, the title of his magazine is a lie. *The Skeptical Inquirer* never inquires skeptically into traditional belief.

In a further chapter, Ginenthal turns his attention to biologist Stephen Jay Gould and finds that he displays the same split personality when it comes to Velikovsky, *i.e.*, criticizing him for alleged scientific faults and then allowing these faults without protest in anti-Velikovsky writers and even displaying them in his own work. There is, says Ginenthal, a double standard in science whereby mainstream scientists can use, without reprimand, any tactic, however faulty, to attack Velikovsky, but Velikovsky is to be faulted on the slightest excuse wherever possible.

In another chapter, Ginenthal looks at the criticisms of Velikovsky by chemist Henry Bauer in his book, *Beyond Velikovsky*, and concludes that, as a scientist, Bauer hardly knows what he's talking about and that his scientific attacks on Velikovsky are too often incorrect and futile. I then follow with a chapter which evaluates Bauer in his role as would-be sociologist of science and philosopher of science, and I demonstrate that in these fields he is equally incompetent, as a result of which his book fails to make any serious point, either about Velikovsky himself, or about the behaviour of science regarding Velikovsky. Also, Ginenthal and I look at the contributions of Isaac Asimov to the Velikovsky Affair and find that he is consistently ignorant, rude, ill-informed, simplistic and childish. He seems arrogantly unaware of the vast amount of material which was available to him in the form of research data by 1974, (when he wrote the article I criticize), but merely echoes the pathetic errors of 1950, 25 years after they were made, as if nothing had happened in between. (Perhaps nothing *had* happened in his mind). As a result, he adds little if any merit and merely makes a spectacle of himself.

There is also a major *contretemps* concerning the role of Carl Sagan in the AAAS Affair and its aftermaths, but it has not been included in this book because the material is too vast (*i.e.*, Sagan is simply too full of mistakes). Charles Ginenthal, however, has dealt with it all in his new book *Carl Sagan and Immanuel Velikovsky*, (New Falcon Publications, 1995), a text on Sagan alone which is almost as large as this book. There, he exposes dozens of errors *in science* by Sagan, to establish (a) that Sagan's heralded criticisms of Velikovsky are consistently erroneous, and (b) that the schoolboy quality of Sagan's mistakes demonstrates an inability on his part to control himself, (*i.e.*, to behave like a competent scientist), which is a classical symptom of the rage and foolishness to which blindly reactive mainstream critics are doomed by the Velikovsky Affair. Mention *will* be made of Sagan here and there in this book as required, but Ginenthal's book is a marvelous demolition piece, and readers are advised to look at it in complement with this one.

In my concluding chapter, I survey the continuing Velikovsky Affair and seek the real reasons why it recurs. By taking the question back before Velikovsky to Darwin, Lyell, Newton, the Scholastics and, finally, to Aristotle and Plato, I show that the urge to suppress any mention of catastrophe and to encourage a belief in uniformitarianism has been a disease rampant in our culture for 2,500 years, and that the Velikovsky Affair *per se* is merely the latest in a long chain of similar outbreaks. Finally I offer a formal solution, (allied to the one Velikovsky

put forth in all his writings), which is that we shall never be free of the trauma induced by the catastrophes, (which I call *catastrophobia*), until we face and accept and assimilate the truth of catastrophism.

Three: The Larger Picture

To complete our introduction, two deeper and more inclusive points have to be established. The first is that, in the earlier parts of this chapter, I have presented the principal typical events of the Velikovsky Affair as they occurred over the past half century. It is a telling and disturbing story, but it can only be the first step, for it is less important to discover *what* happened than *why*. That is to say, while the bulk of this book, including this first chapter, will present evidence to substantiate the existence of a shocking, unreasonable, unexpected tendency in modern culture, we must go on to use this evidence to try to determine *why* it occurred. That will be done fully and formally in my last chapter, as I said a moment ago, where I will expand our analysis to trace the existence of the phenomenon throughout the *entirety* of Western culture, not merely in the U.S. in the last half of the 20th century, but in the 19th and 18th centuries in England and France, and then in the renaissance and medieval worlds, and then all the way back to the classical periods of Rome and Greece. This will provide us with a much larger and more powerful body of data, whose recurrent similarity will definitively indicate how the impulse behind the Velikovsky Affair has played a major role, at every major turning point in science's history, in the suppression of disturbing evidence and the selection of scientific thought according to human desire. We have always believed what we want to believe.

Naturally, we will not continue to call this tendency the Velikovsky Affair with reference to those earlier periods, since they antedate Velikovsky, but perhaps we can refer to it as The Affair; for the phenomenon is, in its essence, a continuous and ineradicable characteristic of Western culture—a collective, pathological inability on our part to face the prospect that our world may not be eternally stable, quiescent, *or safe*. I will not analyze the deep cause of that pathology here, since it must await the full presentation of the evidence first, (*i.e.*, the following chapters), but the reader is asked to be aware, as those specific chapters are being digested, that the Velikovsky Affair of the late 20th century, which this book will demonstrate, is part of the *Affair* in general and has its origin not in any single nation or group of individuals at a single time, but in Western culture itself. That is to say, it will be argued in my final chapter that The Affair could only have happened *because of what Western culture is*, which is the product of what Western culture has experienced and how it has reacted to that experience. Therefore, what the Velikovsky Affair finally tells us, in its largest dimension, is something about ourselves. It is a window into our past and present.

Second, I must emphasize again that all of the enraged and vindictive excesses of science, no matter how flagrant or uncharacteristic they may have been, do not mean that Velikovsky *must* be right. What they unquestionably *do* mean, however, is that Science was very, very wrong. Even simplistic apologists of science like Asimov and Bauer implicitly admit this, (however much Asimov and his ilk try to deny it explicitly), when they unjustly accuse Velikovskians of applying to him the *Galileo analogy*: *i.e.*, if Velikovsky was treated as badly as Galileo, he must be as right as Galileo. As for the substantive issue, certainly, to compare someone to Galileo merely in terms of treatment can carry no *intellectual* weight concerning the person's theories, but I do not know of any Velikovskian who has done it. On the contrary, his followers admire him *for the hundreds of pages of intriguing evidence* he offers in *each* of his books, (which critics like Asimov conveniently ignore). I have mentioned this here, therefore, not to initiate a debate on the merits of Velikovsky's ideas, but the opposite—to readjust our focus back to the Affair, which these scientists *themselves* prove has occurred when they compare Velikovsky to Galileo. The question of whether Velikovsky is right or wrong must be left to different books *because it is not the point of this book*. What *is* the point here is the misbehavior of organized science, and our objective therefore will be to demonstrate that such misbehavior did occur and continues to occur on a massive and long-lasting scale.

It is from this sociological, rather than ideological, foundation that we can move to our final purpose, which is remedial, rather than intellectual. To put it succinctly, this book is not merely a revelation of the deranged underside of our collective personality, but a program to cure it. Like all good medical research, it proceeds from symptom to diagnosis to prescription. The evidence is all of the appalling items in the Velikovsky Affair. The interpretation, of course, derives straight from catastrophism and the solution accords with the established principles of trauma psychology. It is all here for the reader to see and judge. We think it is conclusive. Read it yourself and see what you think.

* * * * *

THE AAAS SYMPOSIUM ON VELIKOVSKY, By Charles Ginenthal

"Another perennial attitude is summed up in the words Us–Them. Here the world is divided in two: the children of light [the scientific establishment] and the children of darkness [Velikovsky and his advocates], the sheep and the goats, the elect and the damned.

"Every social program can be analyzed without much study: all one has to look for are the sheep and the goats.

"There is room for anger and contempt and boundless hope; for the sheep are bound to triumph.

"Should a goat have the presumption to address a sheep, the sheep often do [sic] not hear it, and they never hear it as another I. For the goat is one of Them, not one of Us.

"Righteousness, intelligence, integrity, humanity, and victory are the prerogatives of Us, while wickedness, stupidity, hypocrisy, brutality—and ultimate defeat belong to Them."

Walter Kaufman's
"Prologue" to *I And Thou*,
by Martin Buber,
(New York, N.Y., 1970) p. 13

"I am disturbed to see a growing gap between the standards upheld by scientists when they face other scientists and the standards they uphold when they face the public. In public it evidently becomes more difficult for scientists to wrestle their egos to the ground, and this leads them to say things they would never try to get away with among colleagues."²⁴⁷

The genesis of the symposium held on Immanuel Velikovsky began with a letter by Walter Orr Roberts, a past-president of the AAAS (American Association for the Advancement of Science) to Stephen L. Talbott, editor of the journal, *Pensée* dated July 18, 1972. Roberts had read the first issue of *Pensée* containing the article "Velikovsky Reconsidered," and thus suggested,

"Perhaps the AAAS could be interested in holding a symposium on scientific logic using the Velikovsky case as a specific study. Perhaps the symposium should be narrowed down For example, one might take the matter of Velikovsky's arguments regarding electrical charges on the sun and in the planetary system

²⁴⁷Tony Rothman, "Preface," *Science à la Mode*, (Princeton, N.J., 1985), p. xi.

"In any event, I do agree with the editors . . . [of *Pensée*] that the public deserves a better assessment of the validity of Velikovsky's work than it has received to date"248

Talbott replied, "We are quite pleased with the way things are 'loosening up,' and Velikovsky's work is receiving serious discussion. We can easily foresee the day when the professional/technical journals will have taken up the matter and we can bow out,"249 to which Roberts answered, on January 3, 1973, that Carl Sagan had endorsed the idea of a symposium but was, like me, far too busy to do very much. Roberts felt that in order for such a symposium "[t]o be profitable it would have to be enormously carefully done."250

Thereafter, Dr. C. J. Ransom, a member of the AAAS, submitted a proposal to hold this meeting dated June 11, 1973.

"An analysis and critique of the views of Immanuel Velikovsky (*Worlds in Collision*) as they bear on the question of the age and characteristics of the planet Venus. In view of the increasing interest in Velikovsky's work among scientists and the general public, such a symposium restricted to one topic, could perform a valuable service in setting the discussion of Velikovsky's theories on a sound, cautious and scientific footing

"Such a symposium is likely to draw as diverse and multidisciplinary (and emotionally charged) an audience as any AAAS gathering. Attendance would probably be high. Care in the planning would have to be taken to insure a cautious, low-key, fair and profitable proceeding."251

Although Ransom's proposal, as such, was not accepted in detail, the AAAS felt that *it* should be in complete control, and thus Donald Goldsmith of the State University of New York at Stonybrook, Ivan King, of the University of California at Berkeley, and Owen Gingerich, of Harvard, came to be the AAAS organizers in charge of things. King visited Velikovsky to present the idea; Goldsmith wrote to Velikovsky on July 30, 1973 to confirm the invitation and to name the three speakers who would discuss Velikovsky's work. Prof. Carl Sagan, of Cornell University, Prof. J. Derral Mulholland, of the University of Texas, and another, who were well acquainted with Velikovsky's views, were to speak.

In late summer of 1973, Talbott invited King to submit a statement to be published on the symposium and its purposes for publication in *Pensée*. What King wrote in that statement follows:

"What disturbs the scientists is the persistence of these [Velikovsky's] views, in spite of all the efforts that scientists have spent on educating the public. It is in this context that the AAAS undertakes the Velikovsky symposium. This is not a debate on the correctness of Velikovsky's views of the planetary system; none of us in the scientific establishment believes that such a debate would be remotely justified at a serious scientific meeting. Nor do we intend it, on the other hand, as an occasion for public castigation of a heretic."252

In other words, the aim of the meeting was, according to King, not "remotely justified" based on the scientific aspects of Velikovsky's work, since "none of us in the scientific establishment believes" the validity of Velikovsky's ideas. The aim was to educate the public and put a stop to the "persistence of these views."

²⁴⁸*Pensée*, Vol. 4, No. 2, (Spring 1974), p. 24.

²⁴⁹*Ibid.*, pp. 24-25.

²⁵⁰*Ibid.*

²⁵¹*Ibid.*, pp. 25-27.

²⁵²*Ibid.*, p. 28.

Velikovsky protested this description and prejudiced statement coming from one of the major organizers of the meeting. King felt that he was presenting the proper approach and stood by his statement. Can anyone imagine that, with such a prejudiced attitude, King could be expected to act fairly as chairman of the gathering? In spite of this, Velikovsky went ahead to confront the scientists.

On February 25, 1974 at 8:00 a.m., a crowd of about 1,000 gathered with the press corps at the Grand Ballroom of the Hotel St. Francis. Many of those present had read in that morning's *San Francisco Examiner* that "Velikovsky, Gell-Mann, Seaborg, Sagan, Margaret Mead—the brightest stars in the science galaxy—are in town this week for the 140th meeting of the American Association for the Advancement of Science."²⁵³ As announced in *Science*, "This was the confrontation with conventional science which Velikovsky, the archcatastrophist had sought since the early 1950's When it finally came the encounter lasted seven hours, four on the morning of 25 February and three more in the evening. At the end, each sphere—Velikovsky, and the panel of scientists who volunteered to debate him—declared itself the winner" ²⁵⁴

Science News declared, "It was time for a scientific day of atonement:"

". . . partly because of a persistent sense of guilt over what Velikovsky's harshest critics now concede was a shabby breach of academic freedom, the AAAS invited the 77-year-old author to debate his views in a public forum."²⁵⁵

But, of course, there was much more to it than that. As Walter Sullivan, science editor for the *New York Times*, noted in his report from San Francisco, "[i]t is evident that 24 years after *Worlds in Collision* first appeared, there is a revival of interest in his [Velikovsky's] theories."²⁵⁶ The events that ensued revealed that, as Ivan King remarked, it was the persistent interest and belief in Velikovsky's ideas among the public that was the basis of the meeting and not to atone for the degradation earlier heaped upon Velikovsky by the scientific establishment. The aim of the symposium's organizers and speakers was to destroy that interest once and for all.

With the following words Ivan King opened the gathering:

"Today we are going to consider a set of ideas that have at their core a completely unconventional picture of the planetary system Most scientists would say that this picture is totally impossible, because it violates many of the most firmly established principles of physics. To this Dr. Velikovsky would reply that there is overwhelming evidence that these events really did occur, and that if they cause difficulty for the scientists, it's up to the scientists to resolve their own problems.

"No one who is involved in the organization of this symposium believes that Dr. Velikovsky's ideas are correct. Yet millions of people have read his books and after more than 20 years of condemnation by the scientific establishment he still has a large and often devoted following. It is for this reason that we believe that discussion of his ideas at a meeting of the AAAS is a public service. It is in this spirit that we present this morning's symposium."²⁵⁷

As Mark Washburn aptly stated, "[i]ndeed, the scientist[s] [at the AAAS symposium] actually seemed to relish the opportunity to dispose of Velikovsky once and for all."²⁵⁸ Or as Charles Fair stated that same year—1974, "[a]cademia will never let him [Velikovsky] get away with it . . . it will see him dead first."²⁵⁹

²⁵³*Ibid.*, p. 23.

²⁵⁴*Ibid.*

²⁵⁵*Ibid.*

²⁵⁶*Ibid.*

²⁵⁷*Ibid.*, p. 31.

²⁵⁸Mark Washburn, *Mars at Last*, (New York, 1977), P. 96.

²⁵⁹*KRONOS*, Vol. V., No. 4, (Summer 1980), p.61.

One way to make sure that Velikovsky would be dead before the public at this meeting was to not allow him enough time to investigate the attacks that would be hurled at him that day. And to make doubly sure he would not have any time to do so was to withhold from him the opponents' papers that would be presented until the very day of the symposium except one, that of Norman Storer, a sociologist who would not attack Velikovsky's theory nor impugn his evidence. Velikovsky was expected to run down the information in the various papers, gather sources of rebuttal, and organize them all in less than a few hours. In a civilized court of law, such proceedings would be illegal and barred, and the judge or lawyer, if he had a shred of common sense and decency, would never allow such behavior. The same ought to be true of a scientific proceeding, but this was not the case with the Velikovsky AAAS symposium. Velikovsky received the papers attacking him that morning and had to be ready to respond to them almost immediately. He was kept in the dark until the trap was sprung. The reporters who came to cover the meeting never mentioned this!

Tom Van Flandern calls this the "sneak attack." "This simply means catching your opponent by surprise with a question or challenge he has had no time to prepare for. In science, where we seek truth, not victory, it is important to give someone plenty of advance notice about challenges that will be mounted to his ideas."²⁶⁰ But the aim of the symposium had nothing to do with fair play!

To begin with, the arrangements made were originally to allow "as many panelists speaking *for* your [Velikovsky's] theories as there were panelists speaking *against* your theories."²⁶¹ This was changed. "The AAAS people set up the program so that four panelists would speak against your [Velikovsky's] theories and you alone would be allowed to speak in your defense. Not a single scientist working with you was allowed to participate in the panel discussion. This violated the AAAS promise . . ."²⁶²

The equal time arrangements went this way. Each of Velikovsky's four opponents was allowed to speak for a certain length of time. Velikovsky was then given one-fourth of all their time to answer all their accusations. Sagan received half an hour, Mulholland half an hour, Huber half an hour, Michelson half an hour, Storer half an hour and Velikovsky half an hour. This was then considered fair and honorable to the organizers of the AAAS.

Because of the danger that can come from only one person or group having all or most of the access to the media in the United States, there are legal provisions to give political candidates vying for public office or pressing a political issue equal access to the media and also equal time to rebut criticism. Only in totalitarian countries are these provisions not permitted. One person or group has nearly all the access and time to present its views. Opposition to such views is not permitted. This method of suppression of dissenting views is simply totalitarian in nature, and few would have any doubt that the aim of those who practice this approach are involved in propaganda.

This kind of unfairness enrages me and reminds me of a story about a group of bigots who shanghaied a black man. They put him into a ring with four opponents to have a boxing match, but to be sure of the outcome, they tied the man's hands behind his back. When the bell rang for the first round the poor man realized he had best defend himself in some way. He suddenly realized his heavily shod feet were free and he began kicking his opponents before they could come close enough to punch him. After a few of them were felled from his kicks, the organizers of the fight began to shout, "fight fair! fight fair!" Ivan King similarly felt it "unfair" that the length of Velikovsky's response was somewhat greater than the organizers of the meeting had permitted. He found Velikovsky's use of this somewhat lengthier time to attempt to answer as many of the charges brought against his work by his opponents as deplorable. What he could not seem to understand was that what was truly deplorable was to invite a 78 year old man to a debate with younger men and expect him, with one-fourth of all their time, to raise a reasonable defense.

Interestingly, not a single newspaper man covering the symposium noticed or commented on this time consideration aspect of the debate. Robert Gillette of *Science* saw nothing wrong with such a procedure; neither did Charles Petit of the *San Francisco Chronicle*, nor George Alexander of the *Los Angeles Times*; nor Norman Melnick of the *Examiner*, nor James Hazelwood of the *Oakland Tribune*, nor Walter Sullivan of *The New York Times*. This incapacity of trained journalists to cover the dishonest nature of how the playing field had been rigged against

²⁶⁰Tom Van Flandern, *Dark Matter, Missing Planets & New Comets*, (Berkeley, Calif., 1993), p. 361.

²⁶¹Lynn E. Rose, "Foreword," *KRONOS, Velikovsky and Establishment Science*, Vol. III, No. 2, p. III.

²⁶²*Ibid.*

Velikovsky played right into the organizers hands, and the press became, in this sense, agents of Velikovsky's opponents.

One of the reporters, noticing that Velikovsky was growing tired after the debate had stretched on for five and a half hours, commented that Velikovsky was not capable of being his own best spokesman. Yet he never thought to raise the question that Velikovsky had been entrapped, and that the debate was unfair because Velikovsky's opponents so outnumbered him, and that the time given for Velikovsky to answer the attacks was so limited. What kind of journalists are such men?!

Here then are the comments of the men of the fourth estate: Robert Gillette wrote in *Science*, "[m]ost of the reporters present in the Grand Ballroom of the St. Francis Hotel seemed to regard Velikovsky as the loser." Charles Petit of the *San Francisco Chronicle* wrote of "the thoroughness with which a panel of scientists dismissed and refuted the ideas [Velikovsky] has developed during the past 35 years," and described Velikovsky's refusal to accept such dismissal as "dogged." George Alexander of the *Los Angeles Times* announced that "the consensus was that Velikovsky came off a poor second in the debate." According to the *Inquirer*, ". . . his [Velikovsky's] theories seemed to be torn to shreds." "As he left, a scientist muttered, 'A real crackpot.'" (*Chronicle*). The *Chronicle of Higher Education* wrote, "So why, if Mr. Velikovsky's theory is so full of holes, has it proved so popular?"²⁶³

When one has one's hands tied, it's hard to fight; and bigots know this full well! Finding a dead black man hanging from a tree with his hands tied behind his back was, of course, the eventual outcome of the boxing match!

Interestingly, there had to be a justification by the AAAS organizers for excluding certain supporters of Velikovsky's work from taking a direct and active part in the debate. Professor Lynn E. Rose was excluded, according to their logic, because he was going to discuss and debate science, and his expertise was in the area of philosophy, not the hard sciences. This would have made perfect sense if the organizers did not immediately break this rule by having a statistician, Peter Huber, Professor of Mathematical Statistics, present a paper to oppose Velikovsky's hypothesis, which was based on cuneiform writing which Huber considered a "hobby" of his. The irrationality of removing a member from the panel who would defend Velikovsky's work because he was going to talk about matters outside his field of expertise, was then justified by the organizers by keeping a member on the panel who would attack Velikovsky while he was going to talk about matters outside his field of expertise. No lawyer or jurist would fail to recognize in this procedure an inconsistency of simple logic and an absence of ethical consideration.

However, Velikovsky had also proposed another panelist, Dr. C. J. Ransom, who is a plasma physicist and is, therefore, from the hard sciences, a position demanded by the symposium organizers. Now what was to be done? Well, of course, there is always a way out, and one was found to exclude him, as well. It was argued that Ransom, though a scientist, worked in industry and not for a university; therefore, he, too, was rejected. The aim was to allow Velikovsky no help or support from competent advocates; the aim was to tie his hands.

This would be similar to the bigots who tied the black man's hands behind his back saying, "since you're not an expert boxer, we don't want you to make a fool of yourself by using irrelevant defenses. For one of your opponents we are going to make an exception because we understand just how such a contest is to be organized." No one in the press, as far as I know, has ever dealt with this issue of Velikovsky's defense either. When one wishes to carry out a Star-Chamber proceeding, it is always best to make sure the person under assault is isolated, surrounded and disarmed. If he is old and becomes easily tired, that also helps.

As Robert Anton Wilson remarked, ". . . remember the debate on Dr. Immanuel Velikovsky's comet heresies staged . . . at the AAAS meeting . . . where the panel consisted of Sagan [and three] . . . other opponents of Velikovsky and poor old Velikovsky, himself? That . . . edge led to a certain amount of bad temper since it made Velikovsky feel [Carl] Sagan's gang was mobbing him"²⁶⁴

It was into this highly antagonistic, scientific, AAAS meeting that Velikovsky entered, hoping against hope that the press would, at the very least, recognize the disgraceful nature of the court of inquisition that had been arranged by the American Association for the Advancement of Science. That hope was sadly misplaced, and the press, bless them, noticed nothing at all amiss with the procedural nature of the debate.

²⁶³*Pensée, op. cit.*, pp. 44-46.

²⁶⁴Robert Anton Wilson, *Right Where You Are Sitting Now*, (Berkeley, Calif., 1992), p. 72.

As Earl Ubell, science reporter for a major television network, remarked, the job of journalists is to expose "What scientists . . . do, not what you [scientists] say about what you do, is ultimately what ought to be getting reported."²⁶⁵ Unfortunately, as we will see, the press did not follow this advice at all, but applauded the entire performance.

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²⁶⁵Earl Ubell, *Proceedings: Symposium on Medicine and the Media* (Univ. of Rochester, October 9-10, 1975), p. 182.

NORMAN STORER'S EXCUSES

"Ultimately . . . every scientist that individually and collectively fails to confront abuses and wrongdoing in the system is contributing to corruption in the system."²⁶⁶

It is thus ironic that the first speaker at the symposium, Norman W. Storer, Professor of Sociology at Baruch College, which is part of the City University System of New York, was going to discuss the proper norms of science and the Velikovsky affair. He was to suggest that scientists operated "by a distinctive set of relationships."²⁶⁷

One of the norms of science is not to slander a man and destroy his reputation behind his back. But Storer apparently cares nothing about this norm of decency. As Lynn Rose points out elsewhere in this volume, "when Storer thinks the whole world is watching, he flaunts his 'determined, dogged neutrality.' But when he thinks that no one will know about it, he says that Velikovsky is 'quite out of his tree.'"

Storer's remark is sheer duplicity and shows that he is deeply biased, ready to spread malicious, vicious, gossip! His neutrality is a polite deceit which the press unfortunately bought. If Storer was truly honest, he would have been proud to say in public what he said in private so that the press and the public would get a good look at the way he really operates. What he apparently was indifferent to were the very norms of scientific behavior being carried out, and in which he was acting as a participant in the gathering. He too, like the newsmen at the conference, never discussed the norm of allowing a man whose work is under attack, being given all papers attacking his views well in advance of a conference so that he would have sufficient time not only to read these attacks, but to research them, and having sufficient time set aside to fully answer these attacks. His was the only paper presented to Velikovsky well in advance of the debate. These are fundamental norms of science, but Storer apparently did not mention them. Why?

While Storer mouthed the ideals of science, he was taking part in a modern-day scientific witch hunt. This was false neutrality.

The approach to these normative standards that Storer discussed are those of sociologist Robert K. Merton of Columbia University. In his *Social Theory and Social Structure*, (1951), Merton stressed the interrelationship between social theory and empirical research. It is an idealized Utopian picture of science and the nature of the way Merton thought science operated. According to Broad and Wade,

"The ideology of the scientific profession has emerged from the writings of three groups of professional observers, the philosophers, historians, and sociologists of science. Each of these three groups has imposed its own professional biases on its description of how science works. All have looked to science to embody the ideals so lacking in the real world. They have read into science the virtues of justice, of fairness, of lack of prejudice, of desire for the truth, of taking a man and his ideas on their merits alone regardless of his prestige or qualifications or status. They have described the scientific scene through spectacles made in Utopia.

"Thus philosophers such as the Vienna Circle have examined science as a logical and purely empirical process. Sociologists [such as Robert K. Merton] have posited 'norms' of the scientific ethos, contending that science is characterized by 'organized skepticism,' the acceptance of ideas on their merits, and the disinterested pursuit of truth. Historians animated by the idea of progress of which scientific knowledge seems so shining an exemplar, have sought to portray

²⁶⁶Jon Kalb, in Robert Bell's, *Impure Science*, (New York, 1992), p. 36.

²⁶⁷Norman W. Storer, "The Sociological Context of the Velikovsky Controversy," *Scientists Confront Velikovsky*, (Cornell University Press, 1977), p. 29.

scientific history in terms of its success, its great men, and its moral object lessons that show reason triumphing over superstition."²⁶⁸

What Storer has presented is an unreal, idealized picture of science.

What Storer wishes to analyze is, why had the scientific community behaved toward Velikovsky in a manner totally in contradiction to these norms during the 1940's and 1950's.

"The analyses by de Grazia [Juergens, and Stecchini in *The Velikovsky Affair*] and others of how the scientific establishment responded to Velikovsky's assertions seem amply to document charges that in certain cases both norms of science and the norms of common courtesy were violated. Indeed, the affair has become a classic case study used to flay 'scientism' for its hypocrisy and its self-serving censorship of ideas that threaten established scientific dogma.

"*The Scientific Community Under Attack*. If we are to stick to the sociological perspective, viewing science as a community, we must pay attention also to its social environment. I suggest that this environment during the late 1940's and early 1950's was particularly important in shaping the community's response to Velikovsky. During this period, science and indeed all intellectual enterprise seemed to be under attack by right-wing forces in American society. The Cold War was at its chilliest, Congress was obsessed with atomic secrets and Senator Joseph McCarthy was waiting in the wings. Blacklists were drawn up, loyalty oaths were required, and to be a scientist was to be a potential traitor; the Oppenheimer Hearings in 1954 were perhaps the culmination of this growing national paranoia.

"Scientists quickly adopted a defensive posture, and one could hardly expect them to welcome with open arms another apparent attempt to discredit established scientific knowledge. If we add to this fact that Velikovsky could be only marginally distinguished then from the myriad of eccentrics who have always assailed science, perhaps the initial response to his work can be understood.

"*Velikovsky [was] . . . an outsider.*"²⁶⁹ (Emphasis added)

In essence, Storer claims that the flagrant excesses against Velikovsky by the scientific community in the earlier Affair are only pathological because of the hysteria in the United States created by the Cold War and the spectra of witch hunts, blacklists, etc. Therefore, if Storer is correct, one would expect the scientific community to react toward a revolutionary concept presented by an Insider of science, during a time when no such hysteria existed in the United States, by employing all the usual healthy norms of scientific behavior.

As a case in point, the controversy generated by Halton Arp over the nature of red shifts, is an ideal example which discloses that Storer's explanation is without a shred of substance. Halton Arp was decidedly not an outsider to science; he was, in fact, an astronomer who was acclaimed both nationally and internationally. Yet he challenged one of the central, modern paradigms of establishment science by offering evidence that the red shifts are not indications of galaxies moving away from each other in an expanding universe. If he is right, the strongest evidence for the Big Bang theory would be destroyed and that most prestigious theory could collapse (no pun intended).

To be historically and sociologically accurate, all this took place in the 1980's, long after the hysteria of the cold war of the 1950's had died. Scientists were no longer required to take oaths of loyalty; no blacklists were being enforced, scientists were not considered potential traitors, there was no growing national paranoia. But, did the scientific community respond to Arp's work with all the idealistic norms of healthy behavior one would expect under these conditions, as Storer argues?

²⁶⁸William Broad, Nicholas Wade, *Betrayers of the Truth*, (New York, 1982), pp. 129-130.

²⁶⁹Storer, *op. cit.*, p. 36.

Geoffrey Burbidge, another world respected scientist, described the sociological techniques used by the scientific community to destroy Arp in the 1980's, just as they used against Velikovsky in the 1940's and 1950's, and continue to use even today.

"The community of astronomers is totally polarized by this argument [about the meanings and interpretations of red shifts]. Most do not want to hear about it. The strong disbelievers hold that those who propose or believe in this hypothesis are variously naive, mistaken, ignorant of how to do statistics, overly zealous or worse. They claim that [the results Arp presents as evidence] are not reproducible, that we have no theory to explain these phenomena, and that we should recant, and that in fact, the red shift controversy is over: that is, the status quo has been maintained. This last statement is often made in meetings to which the proponents of unorthodoxy are either not invited, or allowed to speak."²⁷⁰

As to the sociological norms of science which Storer has presented, Burbidge states,

"The other part of the learning process has been unpleasant, probably because I have a strong instinct for fair play. It may be argued that this is no substitute for good judgment. But neither are the tactics that have been used by those who want to maintain the status quo. These include interminable refereeing [*i.e.*, holding up the publication of papers that support Arp's position for long periods of time and requiring endless corrections to frustrate publication] blackballing of speakers at meetings, distortion and misquotation of the written word."²⁷¹

These same tactics were used on Velikovsky. Thus, it becomes clear that there is no basis for Storer's position of normative science breaking down when the scientific community is under attack by outside forces.

In the Arp affair, science was not under attack by right-wing forces, cold war hysteria, obsession by Congress with atomic secrets, blacklists, loyalty oaths and a growing national paranoia. But the scientific community acted overwhelmingly in similar fashion as it did in the Velikovsky affair.

In this case, in complete contradiction to Storer, without all these assaults on science, "[t]he norms of science and the norms of common courtesy were violated." And I add that Storer's *psychological rationalization* of the scientific community's behavior in the Velikovsky affair is a "classic case study used to flay Velikovsky for science's own hypocrisy and its self-serving censorship of ideas that threaten established scientific dogma!"

It is only *rationalization* that Storer has employed; blind, psychological rationalization to explain the Velikovsky affair, and the enduring grudge with which the scientific community has continued to attack Velikovsky. The *real animus* against Velikovsky, and also against Arp, is that *they challenged the established authority* by proposing totally different theories to explain established dogma. That is precisely why Galileo was attacked. One could just as easily claim that the entire reaction of the Church to Galileo had to do only with the Reformation. Since the Church was under attack by Protestant groups all over Europe, they would respond, as Storer claims, in that they "quickly adopted a defensive posture, and one could hardly expect them to welcome with open arms another apparent attempt to discredit established . . . knowledge."

But what Storer and all apologists of the scientific establishment's behavior toward Velikovsky share in common is their rationalization of suppression. As Jerome J. Langford pointed out with regard to Galileo, an

". . . important issue dramatized in the tragedy of Galileo [Velikovsky, Arp and every other heretic] involves the right of an individual to challenge established authority when he feels he must. There is great pressure in almost every institution whether Church, State, University or

²⁷⁰Geoffrey Burbidge, "Anatomy of a Controversy," *Sky & Telescope*, Vol. 75, (January 1988), p. 40.

²⁷¹*Ibid.*, p. 42.

Corporation for the individual to conform to the *established norm*. (Emphasis added) The natural instinct of every major institution is to look to the security of the past, to methods and practices canonized by experience. The man who dares to be different or to think differently is all too often labeled as a traitor, a heretic, or a misfit The problem is not that conformity is always evil, but when it becomes a blanket with which to smother dissent, free discussion, and the right to believe as one sees fit, then it is dangerous both to the individual and to society

". . . I do believe that powerful institutions in our time feel deeply threatened, just as the Church was put on the defensive by the shattering dissent of the Reformation. And institutions, like people, tend to react quickly, forcefully and sometimes destructively when they operate out of fear. The dangers to individuality and to freedom are far greater today than they were in Galileo's time simply by reason of the mechanisms for social control that have been spawned by the technological revolution."²⁷²

How the scientific community acted has been amply demonstrated in two books, *The Velikovsky Affair*, and Velikovsky's memoir, *Stargazers & Gravediggers*. That is, that men associated with Velikovsky and his books were fired from their jobs and one was blacklisted. Misrepresentations were spread and organized suppression of Velikovsky's views were promulgated by the scientific community. Duane Vorhees pointed out, in his serialized biography of Velikovsky, that

". . . the political left that was being victimized by the unfair, even hysterical tactics of the right was fully capable of using those same tactics in non-political contexts [on Velikovsky]. These tactics included lies, threats, character assassination, self-appointed guardianship of established beliefs, group organization for mutual protection of self-interest against perceived threats from others, censorship of unwelcome ideas for the public good, behind-the-scenes maneuvering, and so on."²⁷³

The elaborate pretense of objectivity by Storer to understand such behavior and the disdain with which he refuses to condemn the actions of those involved in the Velikovsky affair borders on contempt if not collusion. There is simply no excuse for such behavior.

As for Storer's accusation that

". . . *Velikovsky was an Outsider*, it was clear, for instance, that Velikovsky did not possess credentials that would have signaled scientists that the ordinary norms of science should govern their relations with him

"His ideas seemed obviously in conflict with accepted physical facts and laws, and his failure to show how these apparently logical inconsistencies could be explained was another indication that he should be classed as a nonscientist."²⁷⁴

But this is sheer prejudice. The ideal of science, if it is open, as Broad and Wade pointed out, is based on "taking a man and his ideas on their merits alone, regardless of his prestige, or qualifications, or status."²⁷⁵ If, as Storer suggests, Velikovsky's ideas are so far beyond the pale of science, why did Albert Einstein write to Velikovsky, "[t]he historical arguments for violent events in the crust of the earth are quite convincing."²⁷⁶ As early

²⁷²Jerome J. Langford, *Galileo, Science and the Church*, 3 ed., (Ann Arbor, Mich., 1993), p. 186.

²⁷³Duane Vorhees, "Velikovsky in America," *Aeon*, Vol. III, No. 4, (December 1993), p. 58.

²⁷⁴Storer, *op. cit.*, pp. 36-37.

²⁷⁵Broad and Wade, *loc. cit.*

²⁷⁶Immanuel Velikovsky, *Stargazers & Gravediggers*, (1983), p. 291.

as 1946, after reading *Worlds in Collision* in manuscript form, Einstein wrote, "There is much interest in the book which proves that, in fact, catastrophes had taken place which must be attributed to extraterrestrial causes."²⁷⁷ Einstein also stated, "not once, and not twice, but also in the presence of his secretary, '[T]he scientists make a grave mistake in not studying your book (*Worlds in Collision*) because of the exceedingly important material it contains."²⁷⁸

Since Albert Einstein took Velikovsky and his ideas on their merits alone, regardless of Velikovsky's prestige, or qualifications, or status, by what standards or norms has Storer decided that Velikovsky is not to be taken seriously simply because he is an outsider? Was Einstein an ignoramus or a fool who could not distinguish between real and pseudoscience? What Storer really means is that a certain group within the scientific community suffers from a xenophobia to new ideas, and that they have decided, on their own, to classify Velikovsky by a whole host of uncomplimentary and demeaning terms which is contrary to judging a man's work or ideas on their merits and not on the merits of the man. Xenophobia is precisely the implication of Storer describing "Velikovsky as an outsider." Albert Einstein, thank God, was not so sunk in this kind of fetid swamp that he well saw the treatment of Velikovsky by the establishment for what it really was. He viewed the behavior of those who took part in the Velikovsky affair clearly and said their behavior was "MEAN" and "MISERABLE."²⁷⁹ [Capitalization added]

It is worse than blindness on Storer's part to see "mean" and "miserable" behavior as understandable! What norms is Storer talking about?

One of the norms Velikovsky did not follow, according to Storer, which made him unacceptable to the scientific establishment, was that,

". . . because he sought vindication from the lay public through the popular press rather than through communication with scientists in archival literature, he was criticized for failing to play the game properly—even though access to the scientific journals through regular channels was probably not open to him."²⁸⁰

This, then, is the double bind in which Velikovsky found himself. Storer states that Velikovsky, to play the game of science properly, should have published his work in peer reviewed scientific journals. Since he didn't follow this route, he was not following the norms of science. But Storer knew full well, and admits as much, that these channels of access to the scientists were "not open to him." The double bind is rejecting Velikovsky's ideas based on the norm of publishing in the acceptable scientific journals while knowing he could not do so! Because this door of expression was closed to Velikovsky, he published his concepts in book form. Storer's norm for Velikovsky was that he either publish in the journals that would never accept his material for publication, or that he never publish at all! It would be the same situation as the bigots during the boxing match shouting at the black man, "play by the rules, play by the rules, fight with your hands."

To put this into perspective, it is clear that Galileo found himself in a somewhat similar situation when he wrote his book, *Dialogue Concerning the Two Chief World Systems—Ptolemaic and Copernican*. The accepted, established view of Galileo's time held that the Earth was the center of the universe which did not move, and all the planets and stars revolved around it. In Galileo's time, the norms of the educated groups, namely the Church and universities, was to publish in the language of the educated—Latin. Galileo realized that the Copernican theory which he supported was unacceptable to these groups, and even if published, its message would fall on deaf ears.

In fact, Galileo, himself, was fully aware of this insider's attitude. One of his opponents in a dialogue argued similarly to Storer that Galileo did not follow the proper scientific norms, "since he had the poor taste to write in common Italian and not philosophical Latin . . ."²⁸¹ Therefore, instead of writing for the "insiders," the

²⁷⁷C. Ginenthal, "A Perspective" on Einstein's relationship with Velikovsky, *The Velikovskian*, Vol. 1, No. 4, (1993), p. 96.

²⁷⁸*Stargazers, loc. cit.*

²⁷⁹*Ibid.*

²⁸⁰Storer, *op. cit.*, p. 37.

²⁸¹Stillman Drake, *Cause Experiment & Science*, English translation of Galileo's "bodies that stay atop water or move in it." (Chicago, 1981), p. 17.

university professors and churchmen conversant in Latin, he addressed his book in Italian to the educated public in order to bridge the gulf between what was acceptable science, at that time, and what was not acceptable science. Galileo merely addressed the world of his day outside the conventional, normative channels so that his views would become known to both the educated public and the professors and churchmen. What is wrong with that? Apparently nothing! As Stillman Drake explains,

"Galileo's *Dialogue* ranks high among the classics of science, and is deservedly even more famous as a chapter in the struggle for *freedom of thought*. It was not Galileo's greatest contribution to the body of scientific knowledge, and yet in a sense it was his most significant service to science itself, for it effectively made clear to scientists and non-scientists alike As Professor Einstein has remarked, this [authoritarian view] would have been done anyway Yet the fact remains that his book which historically did the most toward breaking down the religious and academic barriers against free scientific thought. Moreover, unlike most scientific classics, *it is a book which was capable of interesting the layman and which still is today*."²⁸² (Emphasis added)

This is made explicit by Paul Feyerabend.

". . . the experts declared the doctrine [of the motion of the Earth that Galileo upheld] to be 'foolish and absurd in philosophy' or, to use the modern term, they declared it to be unscientific. This judgment was made without reference to the faith, or to church doctrine, but was based exclusively on the scientific situation of the time. **IT WAS SHARED BY MANY OUTSTANDING SCIENTISTS. TYCHO BRAHE HAVING BEEN ONE OF THEM** Compared with those facts, theories, and standards, the idea of the motion of the earth was as absurd as were Velikovsky's ideas when compared with the facts, theories and standards of the fifties. A modern scientist . . . cannot cling to his own very strict standards [NORMS] and at the same time praise Galileo for defending Copernicus."²⁸³ (Capitalization added)

If Storer is correct, then "the judgment of the church experts was scientifically correct and had the right social intention, *viz.* to protect the people It wanted to protect the people from being corrupted" ²⁸⁴ The scientists and organizers at the AAAS felt they, too, were protecting the public for the very same reasons. But modern scientists and science historians do not condemn Galileo for disobeying Storer's injunction. They do just the opposite. As historian of science Stephen Jay Gould states, "I honor Galileo for writing his two major works as dialogues in Italian, and therefore addressed to all thinking people in his orbit, and not in the formal Latin of Churches and universities."²⁸⁵

Someone at the time could just as easily have said, as Storer does, "since Galileo does not follow the norms of writing in Latin and addressed his book to the lay public, this puts him outside the sphere of individuals who play by the rules or norms governing science." If Galileo had played by Storer's norms, his ideas would not have reached such a broad audience and it may have taken far longer for these scientific ideas to spread and grow. But, by the same token, Storer condemns Velikovsky's approach of seeking "vindication from the lay public through the popular press." Not only did Galileo pursue this method of first writing for the educated, non-establishment public, but so did Charles Lyell, the great uniformitarian, in his volumes, *Principles of Geology*, about which historian of science Claude C. Albritton, Jr. writes. "The *Principles* proved to be an eminently successful and influential work

²⁸²Stillman Drake, "The Translators Preface," *Galileo Dialogue Concerning the Two Chief World Systems*, 2 ed., (Berkeley, Calif., 1967), p. XXI.

²⁸³Paul Feyerabend, *Against Method*, rev. ed., (London, 1988), pp. 132-133.

²⁸⁴*Ibid.*, p. 137.

²⁸⁵Stephen Jay Gould, *Dinosaur in a Haystack*, (New York, 1995), p. 10.

Lyell had . . . synthesized the geological knowledge of his day around a coherent theory His literary style combined grace and clarity so that readers without technical training could understand his message. The fact that the book excited controversy only added to its popularity."²⁸⁶ What made it so right for Galileo and Lyell to seek vindication from the lay public by writing a book aimed at the public, and so wrong for Velikovsky to seek vindication from the public by doing the same thing (especially since he would not have been published in their journals!)?

Where an idea is promulgated or to whom it is addressed has nothing to do, whatsoever, with the scientific validity of the work. A piece of scientific work dressed up in all the trappings of science, published in the most prestigious organs of science, if it is wrong, changes its scientific nature not one wit. It would still be wrong.

But just how scientific are papers in the journals which Storer claims are the proper channels for publication? Is their system of peer review anything like what Storer's analysis of Velikovsky seems to suggest? Is it open and just and fair, or is it really a system set up to drown new ideas? According to Broad and Wade,

"The peer review system is sometimes accused of operating like an old-boy network because its members are drawn from the same elite groups and institutions that end up receiving the bulk of the grants. According to one critic, former representative John B. Conlan of Arizona, the peer review system . . . is an 'old boys system' where program managers rely on trusted friends in the academic community to review proposals. These friends recommend their friends as reviewers It is an incestuous 'buddy system' that FREQUENTLY STIFLES NEW IDEAS AND SCIENTIFIC BREAKTHROUGHS" (Capitalization added)

"Even journal peer review is vulnerable to abuse. In several documented cases, journal editors have refused to send out for peer review articles that criticize other scientists' work or indicate possible fraud."²⁸⁷

[Robert Bell also explains:]

". . . journal peer review, and experiment replication are supposed to make science self-correcting. But [my book] *Impure Science* illustrates that preeminent people involved in science are repeatedly defeating these mechanisms. If science is at all self-correcting in the United States, it is despite the efforts of some of the powerful individuals, not because of their efforts."²⁸⁸

Finally, Alexander Kohn tells us,

"An interesting study on the reliability of published scientific papers was carried out by Dr. Richard R. Roberts of the National Bureau of Standards. He estimated that at least half of all published papers were unusable or unreliable"²⁸⁹

But Storer suggests that Velikovsky should have submitted his concepts, which he suggests would not be accepted, to just such types of scientific institutions and be subject to just such norms. Robert K. Merton and Harriet Zuckerman, in an article edited by Storer one year prior to the AAAS symposium, stated of one particular journal

²⁸⁶Claude Albritton, Jr., *Abyss of Time* (New York, 1984), p. 139.

²⁸⁷Broad and Wade, *op. cit.*, pp. 100-101.

²⁸⁸Robert Bell, *Impure Science*, (New York, 1992), p. XIV.

²⁸⁹Alexander Kohn, *False Prophets*, (New York, 1986), p. 8.

that "at least it had no perceptible influence on patterns of evaluation."²⁹⁰ Thus, Storer clearly understands just how political the peer review system is but still attacks Velikovsky for not using it. However, when one looks deeper into the process at most journals, a totally different picture than Storer presented emerges. According to Broad and Wade,

"When ten high-quality published articles in psychology were resubmitted with the author's names and affiliations changed to the very same journals that had published them two years earlier, only three of the pseudomanuscripts were detected as masquerades. The other seven went out for review before twenty-two editors and reviewers, only four of whom (18 percent) recommended publication. 'A rather massive lack of reliability exists in the editorial practices' conclude the authors of the study.

"A more elaborate investigation was designed to scrutinize referees' theoretical biases in assessing papers. Michael J. Mahoney had a journal send out fictitious manuscripts on a hotly debated aspect [of science] . . . to seventy-five referees whose personal positions on the problem were on record. The manuscripts all described the same experimental procedure, but the purported results were different, some favoring the reviewer's perspective, some refuting it. The result: 'Identical manuscripts suffered very different fates depending on the direction of their data. When they were positive (*i.e.*, in accord with the referee's particular bias), the usual recommendation was to accept with moderate revisions. Negative results earned a significantly lower evaluation'

"By accident, a glaring error was introduced in the manuscripts sent out for review. The error was not spotted equally by all reviewers: in manuscripts with 'positive' results, only 25 percent of the reviews noted it; but the error was obvious to 71 percent of the reviewers when the results were 'negative' *i.e.*, inconsistent with the reviewer's theoretical perspective."²⁹¹

One of the norms of science is testing! And these findings were true tests of how scientific journals operate in reality, as opposed to the utopian ideas proposed by Merton or Storer. They show that this norm of scientific evaluation promoted by Storer is almost non-existent and, therefore, in no way is a proper, just and appropriate norm by which to determine if a work of science is acceptable.

The real norm of science that Storer refuses to face and understand is that of preconception by the insiders. As Kohn suggests,

"The attractiveness of any theory . . . depends on how relevant it is and how well it fits everyone's preconceptions. If the fit is good then even if the proofs are shaky, there would be a tendency to accept the theory."²⁹²

Likewise, the unattractiveness of any theory depends not on how relevant it is but on how poorly it fits everyone's preconceptions. If the fit requires change from that preconception, even if the proofs are strong, there would be a tendency to reject the theory.

I suggest that it is hypocritical for Storer to claim,

". . . [O]ne corollary of my earlier statement about the importance to the scientific community of an organized universe of discourse is that a threat to its integrity is far more than

²⁹⁰Robert K. Merton, Harriet Zuckerman, "Institutionalized Patterns of Evaluation in Science," *The Sociology of Science*, Norman W. Storer ed., (Chicago, 1973), pp. 460-496.

²⁹¹Broad and Wade, *op. cit.*, pp. 102-103.

²⁹²Kohn, *op. cit.*, p. 7.

simply a threat to a few scientists' reputations or positions. It is a threat to the very foundations to the very meaning, of most scientists' professional careers."²⁹³

Here Storer makes the psychological aspect of the scientific establishment's behavior clear. They were threatened by Velikovsky's ideas and perverted all the norms to suppress him. What is the most interesting aspect of Storer's presentation is that at the very same symposium at which he presented this idealized picture of science, the very norms to which he alludes had also been perverted.

While Velikovsky actually dealt in and with myths to analyze the catastrophic nature of mankind's recent past, Storer has turned into myth the reality of the scientific establishment's actions toward Velikovsky. While Velikovsky was accused of perverting science by incorporating the study of myths to rewrite it, Storer has perverted the study of science by incorporating the myths of how science operates for the realities of how it actually operates. His judgment is influenced by his sociologically rose colored glasses just as the bigots described above were not color blind, believing they were right in their actions.

As was pointed out in *Pensée*, by Professor Lynn E. Rose:

"All this culminated in the following contention [by Storer]: . . . 'it is far better that most claims about scientific truth coming from non-scientific sources be rejected out of hand . . . than that each and every one be accepted seriously and patiently be subjected to detailed testing.'

"Clearly it is not an either-or situation. As Rose remarked following the symposium. [The scientists in the Velikovsky affair had not read his work while at the same time they condemned it.]

"It is not the case that any new theory must either be 'rejected out of hand' or else 'accepted seriously and patiently subjected to detailed testing.' Any scientist who is unfamiliar with the arguments and evidences advanced in favor of a new theory, and who does not have the time or wish to take the time to acquire such familiarity, should not choose *either* of Storer's alternatives; he should simply tell the truth, namely that he does not know . . .

"Storer suggests that the Velikovsky affair was 'inevitable.' But if the 'scholars' had behaved with propriety and had said 'I don't know' when they, in fact, did not know, [because they had neither read Velikovsky's books nor familiarized themselves deeply with the evidence as Albert Einstein did in the last years of his life] instead of saying not only that Velikovsky was wrong [which they did not know] but also that his work should be suppressed, then there would have been no Velikovsky affair; that is, there would have been no book burning, no boycott, no censorship, no slander, no libel, no firings, or any of the rest. But they did not behave with propriety."²⁹⁴

The sociological attitude of Storer makes it clear he believes it was all Velikovsky's fault, as were the conditions of the cold war that precipitated this negative response. I draw the parallel here likening Storer's attitude toward Velikovsky to the bigots described earlier who, when they were dragged before the courts for murder, cried, "Don't blame us . . ."

The very same rationalizations used by certain racist bigots to explain to themselves, and those who would listen, are the same kinds of rationalizations offered by Storer to explain the behavior of the scientific society to which he belongs and from whom he receives recognition. All that Storer has done is blame the victim for the behavior of his scientific colleagues who, themselves, perpetrated criminal actions against Velikovsky. The criminals were not guilty; the victim was! In essence, his norms are no better than those used by white racist bigots. He could no more call Harlow Shapley's actions, and the actions of the others in the Velikovsky affair, vicious and dishonest, than he could call Velikovsky the innocent victim of the degradation heaped upon him by those scientists

²⁹³Storer, *op. cit.*, p. 37.

²⁹⁴"San Francisco, February 25, 1974," *Pensée*, Vol. 4, No. 2, (Spring 1974), p. 32.

who are really his associates. It was Velikovsky who was the victim of a campaign of defamation by scientists, journalists and science writers, who presented a host of misrepresentations repeatedly. This is the sad truth that has been exposed above and will be exposed with those others who followed in the same footsteps at the AAAS Symposium on Velikovsky.

* * * * *

PETER HUBER

MATHEMATICAL MANIPULATIONS

"There are three kinds of lies—lies, downright lies and statistics."

Peter Huber, whose hobby is cuneiform writing, presented a paper at the Velikovsky symposium titled "Early Cuneiform Evidence for the Planet Venus," in which he claimed that he could prove that Venus was always observed just as it is today long before Velikovsky's catastrophe. Therefore, if ancient records show that Venus has always been on its present orbit, it could never have come close enough to the Earth to create the catastrophes Velikovsky described. Huber is so sure of what this evidence shows that he claims ". . . the primary and early cuneiform sources from the second and third millennia B.C. do not support his [Velikovsky's] contentions. On the contrary, they flatly contradict them."²⁹⁵

In order to undermine Velikovsky's position on what the ancient texts say, Huber states,

"Velikovsky draws on historical and archeological evidence to support his hypothesis, but unfortunately his arguments are mainly based on late and secondary sources, in part on obsolete and erroneous translations, and therefore lack force. Moreover he sometimes makes a complete muddle of texts, insights, periods and places."²⁹⁶

Huber claims that the sources Velikovsky relied upon are "obsolete and erroneous," as compared with more modern translations and how they were handled. What Huber maintains is that the modern translations contain the truth and the older ones do not. How do we know this? This is based only on Huber's say so.

According to Rose, in time the present translations will also be erroneous based on Huber's logic. According to Edward Harrison, "[u]nfailingly, humans pity their ancestors for being so ignorant and forget that their descendants will pity them for the same reason."²⁹⁷ Based on Huber's logic, future translations will make his acceptable translation obsolete. But does Huber really avoid older translations, analyses, etc.? Of course not!

What Huber has done is use a book published in 1928, *The Venus Tablets of Ammizaduga*, by Langdon, Fotheringham and Schoch, for analyzing the ancient motions of Venus as depicted on cuneiform tablets. And why does Huber use this old source as *his* guide? Because the book rejects evidence that will contradict Huber's assumptions. As Rose pointed out, one of the *cuneiform readings* was for a period of "five months and sixteen days." But in this older source, this is changed to "two months and six days." One of the Babylonian months and days, Nisan 9 is changed in the book to the month Ayar 29, while the month and day, Ulul 29, is changed to Ab 5. Each of these changes made by these investigators supports Huber's preconceptions and thus this old translation is not obsolete or erroneous.

What Huber has chosen to do is use not only an old source, but an old source that has culled and manipulated the data to make it fit Huber's assumptions. Thus, Velikovsky is accused of using "obsolete and erroneous translations" while Huber uses a truly "obsolete and erroneous translation." If the obsolete and erroneous translation agrees with Huber's uniformitarian assumptions, it is an acceptable source. If it disagrees with his dogma, it is rejected. Huber then claims he will employ science and evidence in the proper scientific manner. So begins his hypocrisy.

²⁹⁵Peter J. Huber, "Early Cuneiform Evidence for the Existence of the Planet Venus," *Scientists Confront Velikovsky*, (Ithaca, N.Y., 1977), p. 117.

²⁹⁶*Ibid.*

²⁹⁷Michael Pellegrino, *Return to Sodom and Gomorrah*, (New York, 1994), p. 96.

But this is completely contradicted by a researcher who works in the field of archaeoastronomy who has been examining just how modern researchers handle these kinds of evidence. What he claims, in complete contradiction to Huber, is that modern researchers also manipulate the data to make it fit the view that there have been no changes in the orbits of bodies in the solar system. This scientist, Robert R. Newton, has not made this charge once, but has done so again and again! He states specifically,

"... *all uses of solar eclipses* that I have seen, were based upon the logical fallacy of reasoning in a circle, specifically, most reports used could not be dated on the basis of their texts or their historical contexts. The workers [scientists] thereupon assigned dates by finding which ones led to accelerations [of the celestial bodies] that agree most closely with assumed values. [*i.e.*, with what the astronomers expect to find] It is not surprising that the resulting 'data' were self-consistent."²⁹⁸ (Emphasis added)

Newton elsewhere writes:

"*Virtually all studies* of ancient [solar] eclipses that I know have used the following procedure in handling doubtful or ambiguous cases. The author [scientist] has assumed values of the accelerations [of the celestial bodies] in advance and has calculated the circumstances of the possible observations using them. He has then rejected as invalid all observations or interpretations thereof that do not agree well with the assumed values. He finally used the remaining set of observations to calculate the accelerations. He necessarily found good agreement with his initial assumptions. (Emphasis added)

"This, of course, is reasoning in a circle."²⁹⁹

Newton specifically claims that *all the observations*, such as cuneiform texts, taken from the ancient documents by the researchers, are made obsolete by the way they are handled and not at all by the translations, as Huber suggests. Newton said this in the 1970's at the time Huber was presenting the view that these documents, with better translations, are more accurate and thus will fit the modern concept of the motions of celestial bodies in the solar system. Newton maintained that the data was *culled*, that is, manipulated so that it would fit the "assumed values" of the scientists. He further tells us that these scientists "rejected as invalid all observations or interpretations thereof, that do not agree with the assumed values." What Newton is also telling us is that these new translations, when found to contradict the assumptions of the scientists, are "rejected." In essence, Newton, who is in no way a Velikovskian but is a researcher into ancient astronomy, is saying that ALL translations and applications of them are fixed so that they are made to agree with what is assumed, *à priori*, to make it fit the scientific viewpoint and thus become self-consistent.

Huber, of course, never mentions this approach, and for very good reason, because he will employ this very same approach. He will use only data that fits his assumptions or reject it if it fails to do so. He will only employ data that fits his preconceptions of times and places. He will reason in a circle and in the end will make the data found appear to be in good agreement with his initial assumptions. And it will be fully demonstrated that he does this again and again! We will also show that in his desire to make the evidence fit his assumptions he will make a mess and a mockery of the evidence and scientific procedures. Huber's first point is:

"(1) That Venus was known as the morning and evening star certainly by 1900 B.C., and that in archaic texts, shortly after 3000 B.C., it is mentioned as a star in connection with the rising and setting sun. Hence it was already in an orbit between the sun and the Earth."³⁰⁰

²⁹⁸Robert R. Newton, *Medieval Chronicles and the Rotation of the Earth*, (Baltimore, 1972), pp. 2-3. (Emphasis added)

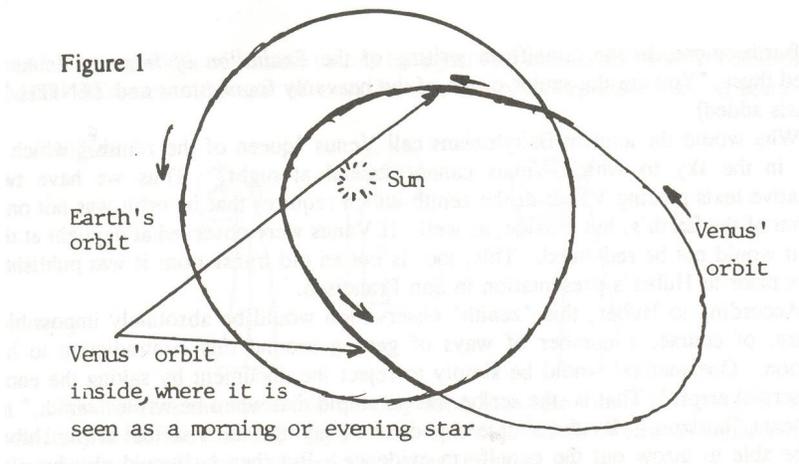
²⁹⁹Robert R. Newton, *Ancient Astronomical Observations*, (Baltimore, 1970), p. XIV.

Huber dutifully then presents cuneiform data that make it undeniably clear that before Velikovsky's catastrophe around 1500 B.C., that Venus is called the morning and evening star. And he concludes that Venus' orbit had to be "between the sun and the Earth." Now as the reader will recall, both Professor Lynn Rose and scientist C. J. Ransom were excluded from taking a direct part in the debate because Rose was not a scientist and Ransom was not attached to an institution of higher learning. Huber, on the other hand, was a hobby cuneiformist and a mathematician from an institution of higher learning. But this did not stop him from making a whopping error on this very point, which Ransom pointed out to him during the question and answer period. If Huber had been an astronomer, he would understand that his first point is inane and absurd.

To wit, once a body in an orbit around the Sun is inside the orbit of the Earth, it will be seen, for a period of time, as the morning star or as the evening star, based on whether it passes the Earth on the east or west side. (Figure 1, following page)

³⁰⁰Huber, *op. cit.*, pp. 117-118.

Figure 1



As any astronomer will well understand, during the time Venus' path is inside the orbit of the Earth, that is, between the Earth's orbit and the sun, Venus must be observed as either a morning or evening star. What is extraordinary is that Huber failed to understand this very simple astronomical concept. He has, as was pointed out earlier, simply taken the evidence and rejected the part of it which fails to agree with his assumption. As long as a celestial body is in orbit around the sun, while its path is inside the Earth's orbit, it is either a morning or evening star. To not know this very simple fact is either great ignorance or willful distortion.

Now, one point that must be understood and stressed is that if Venus' orbit was always inside the orbit of the Earth and never outside, it could never be observed at night at the zenith (directly overhead) at Babylonia. If its orbit was always its present one, Venus could only rise at night only part way above the horizon; it could never rise at night to the Babylonian zenith unless, and that is a very big "unless," its orbit was not only inside that of the Earth, but also outside the orbit of the Earth, as depicted in Figure 1.

What do the latest translations of the cuneiform texts say in this regard? Do they say, as Huber would have us believe, that Venus was never seen at night at the zenith? The *Chicago Assyrian Dictionary*, Vol. 16, page 75, analyzes the word *Sallummu* as it relates to Venus; when it is altogether red-hued, this authoritative dictionary claims Venus "moves across [its] variant: at its ZENITH (?)" (Capitalization added) The cuneiform translators of this passage could hardly believe their eyes when they read that Venus was observed at the zenith of Babylon and placed a question mark in parentheses to emphasize their dismay. That is, in a cuneiform text discussed in 1962, twelve years prior to Huber's discussion on this point, one of the worlds most authoritative cuneiform dictionaries and its translators were forced to admit that a cuneiform document claims that Venus was seen at night, red-hued at the zenith and, swallowing hard after doing so, inserted a question mark in the text to express their shock!

Furthermore, in the cuneiform writing of the *Exaltation of Inanna*, Venus is described thus: "You are the senior queen of the heavenly foundations and ZENITH."³⁰¹ (Emphasis added)

Why would the ancient Babylonians call Venus "queen of the zenith," which is a point in the sky to which Venus cannot ascend at night? Thus we have two authoritative texts placing Venus at the zenith which requires that its orbit was not only inside that of the Earth's, but outside, as well. If Venus were observed at daylight at the zenith, it would not be red-hued. This, too, is not an old translation; it was published six years prior to Huber's presentation in San Francisco.

According to Huber, this "zenith" observation would be absolutely impossible. There are, of course, a number of ways of getting around this contradiction to his assumption. One method would be simply to reject the

³⁰¹W. W. Hallo, A. J. A. Van Dijk, *The Exaltation of Inanna*, (New Haven, 1968), p. 29.

document by saying the entry was a "scribal error." That is, the scribe was so stupid that when he wrote "zenith," he really meant "horizon." By the simple expedient of saying this is scribal error, Huber would be able to throw out the cuneiform evidence. But then he would also have to explain why the lexicographers, "the real experts" who compiled the dictionary, didn't reject this translation. I would suggest that they have no uniformitarian axes to grind and left it in. They are not, I hastily add, Velikovskians.

In order to make his point about Venus being a morning and evening star, Huber used a primary source, A. Falkenstein, *Archaische Texte aus Uruk*, (*Ancient Texts of Uruk*), (Leipzig, 1936). Falkenstein has transcribed these in table "No. 606 . . . 'star, setting sun, Inanna' . . ." ³⁰² Now how did the Babylonians write that Venus, Inanna, was inside the Earth's orbit? They wrote,

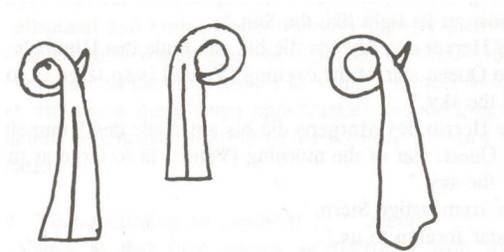
"Star, setting sun, Inanna," or Venus. And what does the reader think is the symbol for Venus or Inanna? Lo and behold, it is: (Figure 2)

Figure 2



Does this symbol look like Venus or does it look like a comet? It clearly looks like a comet. And Huber was forced to admit it at the symposium. "The Inanna symbol sometimes looks like a comet," although he immediately assures us that "the similarity is not borne out by the more elaborate representations." ³⁰³ But the fact of the matter is that ALL the symbols of Venus, Inanna look just like comets and not as Venus presently appears. In the cylinder seal of Uruk Venus, Inanna is depicted thus. (Figure 3)

Figure 3



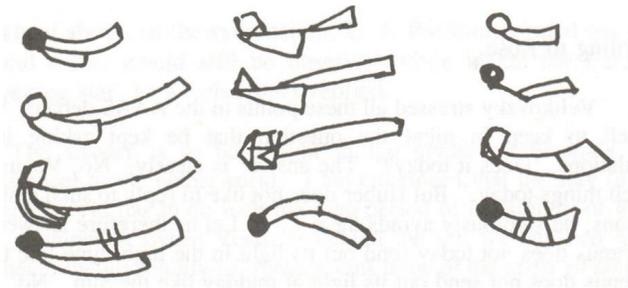
Again, I ask the reader, do these symbols look like Venus as it presently appears, or do they look like comets? Lynn Rose collected *all* the Venus, Inanna symbols in Falkenstein's book ³⁰⁴ and presented them thus: (Figure 4)

³⁰²Huber, *op. cit.*, p. 121, *figure 2*.

³⁰³Lynn E. Rose, (A), "Just Plainly Wrong": A Critique of Peter Huber, *KRONOS*, Vol. III, No. 3, (November 1977), p. 108.

³⁰⁴*Ibid.*, p. 111.

Figure 4



As a statistician Huber has enormous difficulty in distinguishing between Venus "sometimes" looks like a comet and the fact that ALL the Venus, Inanna symbols "always" look like comets. In order to evade this difficulty, what Huber did was withdraw this part of his paper at the AAAS symposium and simply omit this aspect of Venus' appearance as a comet from his paper in *Scientists Confront Velikovsky*. Little boys, when confronted with naughty behavior, become quiet and put their heads down in denial to handle their embarrassment. Scholars like Huber, when confronted with evidence that contradicts their assumptions, also stop talking about the evidence and act as if it does not exist. I personally fail to be able to distinguish between the childish denial behavior of either. As I mentioned earlier, when Huber finds evidence that goes against his assumptions he rejects it. Does Storer or Huber really believe that this behavior is an example of the proper norms of science? Apparently Huber does. Rather than coming to grips with clear depictions of Venus as a comet, Huber simply stops dealing with it!

But Huber also made the claim that Venus was never any brighter in the past than it is presently observed today. However, Velikovsky presented direct cuneiform citations from the Babylonian Hymn to Inanna used by Huber as a supporting document of his evidence, as did Rose.

"Zur Nachtzeit sendet sie Licht aus wie der Mond am Mittag sendet sie Licht aus wie die sonne."

"At night time [Venus] sends out its light like the Moon at midday [Venus] sends out its light like the Sun."

"Die Herrin des Abends die bis ans Ende des Himmels gross ist."

"The Queen star of the evening [Venus] is so large as to reach to the very end of the sky."

"Die Herrin des Morgens die bis ans Ende des Himmels gross ist."

The Queen star of the morning [Venus] is so large as to reach to the very end of the sky."

"Der fremdartige Stern."

"A star foreign to us."

"Inanna die weithin wie die Sonne leuchtet." "Inanna [Venus] shines as greatly as the Sun."

"Das heilige Licht, das den Himmel erfüllt." "Her [Venus'] heavenly light fills the sky."

According to Rose,

"Velikovsky stressed all these points in the AAAS debate. We would do well to keep in mind the question that he kept asking in various formulations, 'Does it today?' The answer is clearly, 'No, Venus does not do such things today.' But Huber does not like to reply to such embarrassing questions; he studiously avoids them Let us therefore answer for him. No, Venus does not today send out its light in the night time like the Moon. No Venus does not send out its light at midday like the Sun. No, neither as Evening Star nor as Morning Star is Venus today so large as to reach to the very end of the sky. No, Venus does not appear in Velikovsky's translation from the hymn, as 'a star foreign to us,' 'not from this family' of planets. No, Venus does not shine today as greatly as the Sun. And no, Venus does not today fill the sky with its light"³⁰⁵

Babylonian documents show that Huber is wrong on both counts. Venus is clearly pictured as a comet and also described as a brilliant light that spans the heavens. Huber's denial to himself and to everyone of this evidence is sheer bluff. As David Talbott and Dwardu Cardona elsewhere in this volume show, the evidence supporting Venus' cometary nature is overwhelming. It is only through denial that one can account for any suggestion that all this evidence does not fully support Velikovsky's thesis. It is only through denial that one can account for Huber steadfastly avoiding this evidence. It is only through denial of this evidence that Huber states that Venus has always appeared as it does today.

In order to prove that Venus was always in its present orbit, Huber directs his discussion to the ancient cuneiform tablets of Ammizaduga which describe the appearances and disappearances of Venus. As Venus presently orbits the sun, it appears for about 260 days as the evening star, then it disappears behind the sun for about 63 days, but is generally closer to 70 days, coming out on the other side of the sun to reappear as the morning star for about another 260 and then disappearing in front of the sun for about 8 days. All these numbers vary slightly because the Earth's and Venus' orbits are slightly elliptical and both planets travel at slightly different speeds along the different parts of their orbits which average together to about 584 days. Venus, of course, because it is closer to the sun, travels at a much greater velocity than the Earth.

Thus Huber maintains these same appearances and disappearance of Venus are recorded on the ancient tablets and, therefore, Velikovsky's theory is without a shred of support. Huber states,

"(2) That according to [ancient] . . . records . . . the observed motion of Venus at that time agrees satisfactorily with its motions as calculated from the currently accepted orbital elements—which show that Venus' orbit . . . was almost, if not completely identical with the planet's orbit today. The same is true with regard to its brightness."³⁰⁶

As explained above, at the symposium, C. J. Ransom pointed out that Venus, on a highly elliptical orbit, would still be observed while inside the Earth's orbit as a morning and evening star, to which Huber replied,

"The answer is to be sought in the myths on Inanna's descent [disappearance] to the netherworld. I am convinced that the stay of Inanna in the netherworld has to do with the long period of invisibility of Venus at superior conjunction [when Venus is behind the sun for between 63 to 68 days] I mean, the invisibility of Venus seems to be of the order of sixty days, but—The corpse of Inanna is hung on a stake, and it has to be sprinkled with the water of life sixty times. And I am personally convinced that this refers to sixty days. And if the period of invisibility is that long, it [Venus] cannot be a comet."³⁰⁷

³⁰⁵Rose, (A), *op. cit.*, pp. 110-112.

³⁰⁶Huber, *op. cit.*, p. 118.

³⁰⁷Lynn Rose, (A), *op. cit.*, p. 104.

In the first place, "personal assumptions" do not qualify as solid evidence. Huber, himself may be "convinced" that "sixty sprinklings" refers to "sixty days" but that is still only his assumption. But is it a reasonable assumption? The answer is: No, it is not!

As Rose made it quite clear, "the present invisibility amounts to somewhat *more* than sixty days. Indeed, it is, on average, a little closer to *seventy* days than to sixty days."³⁰⁸

Now let us accept Huber's personal assumption that Venus disappeared for 60 days. Does that support his conclusion that Venus was then in its present orbit? Again the answer is no! If Venus' disappearance behind the sun was for only 60 days instead of 63 to about 68 on average, it could in no way be in its present orbit which required these larger numerical values. The 60 day value is not a reasonable one. The sprinklings should be 64 or 65 times to be a ballpark figure, *i.e.*, somewhere between the smallest and largest numerical values.

If Huber's analysis is truly valid then the ancient tablets should clearly support that evidence. Here he employs the appearances and disappearances of Venus as written in the cuneiform tablets of the Babylonian king Ammizaduga. These tablets trace the appearances and disappearances of Venus over 21 years. Based upon what Huber believes is a very careful statistical analysis, he claims these tablets prove Venus was in ancient times in its present orbit. In order to do this, Huber kept stressing at the symposium that of the 50 events described in the cuneiform tablets, 15 were to be discarded as "scribal errors."

Can anyone imagine a scribe being so incompetent that he got thirty percent of the data he was inscribing on a document wrong? Scribes were very carefully trained to do such work and it is quite obvious that no scribe would have lasted in his job who got thirty percent of his work wrong.

Gerald S. Hawkins, the British astronomer, informs us:

"The Babylonians . . . kept track of Venus in a very straightforward way. They took a starting date, added the length of the manifestation to it, and came up with a new date . . . For the Babylonians, line by line, tablet by tablet, planet Venus was pressed into clay. **ONLY THE EXPERTS DID THIS. THEY WERE THE CALCULATORS, THE KEEPERS OF THE RECORD.**"³⁰⁹ (Capitalization added)

The scribe would have to have been incompetent. Now, how does Huber determine which materials are scribal errors and which materials are not in error? The answer is very simple; as R. R. Newton showed, the data in the material, which generally agrees with Huber's assumptions about the orbit of Venus, is correct; and data in the material which disagrees strongly with Huber's assumptions about the orbit of Venus are "scribal errors." Now, there may have been one or two or even five errors in this document, but an error rate of thirty percent is madness. Can anyone imagine a scientist saying to his colleagues at a real science conference, "I believed I proved my case from the data, but I changed thirty percent of the data to make the analysis fit." He would have become a laughing stock after suggesting the person who wrote the data was so incompetent that he got thirty percent of everything wrong. One would have to be more incompetent than the scribe who wrote such a document to swallow such carefully culled data.

Ultimately, what errors were made by the scribe and what were the changes that Huber made in the data to correct these errors? Velikovsky explains,

"As to whether Venus had been seen before –1450, Huber refers to the so-called Ammizaduga tablets . . . But Huber needed to announce that in about thirty percent of readings the text has to be changed: east must be changed to west, and west to east; the names of the

³⁰⁸ *Ibid.*, p. 105.

³⁰⁹ Gerald S. Hawkins, *Mindsteps to the Cosmos*, (New York, 1983) p. 47.

months must be changed; the intervals between disappearances and reappearances of Venus must be lengthened or shortened—all in order to prove Venus moved then as it does now."³¹⁰

As one can see, this was an extraordinary scribe. He wrote that Venus was in the east when it was in the west, and that Venus was in the west when it was in the east; that is, he couldn't tell or read east from west. If he were alive today he would not know one month from another; for example, he might have mixed up April with August, because both begin with the letter A. He also couldn't get the numbers between the appearances and disappearance correct. He somehow made them too long or too short. What were the errors that the scribe made? They are ones that disagree with Huber's preconceptions. Whatever fit or seemed to fit Huber's preconceptions the scribe evidently wrote correctly. Whatever does not fit Huber's preconceptions the scribe evidently wrote incorrectly. But once Huber changed all these scribal errors he claims he has proven his case.

Apparently unhappy over the fact that Velikovsky exposed his methodology, Huber wrote a letter to *The Humanist* and accused Velikovsky of misrepresenting him. "I am not aware that I was 'forced to change west to east, names of months . . . ' in order to 'make the ancient observations conform to the present day motions.' But I certainly did make some emendations" ³¹¹ Huber having been caught with his pants down denied what he had done. But Rose, who has published research on ancient calendars in such prestigious journals as the *Journal of Near Eastern Studies*, published by the University of Chicago, was in no way fooled by this subterfuge. He carefully examined Huber's paper and then wrote:

"There are five cases in which he *does* have to change east to west, and there are six cases in which he *does* have to change west to east Huber has to reject reported month names in at least ten cases."³¹²

In spite of all this Huber make the bold claim. "I have tried to avoid doctoring the evidence."³¹³ Evidently he did not try very hard.

Giorgio de Santillana and Hertha von Dechend, who have also spent much of their lives researching ancient astronomy, make it quite clear that Huber's behavior is unscholarly, to say the least. They state,

". . . it is an unsound approach to [ancient] . . . astronomy to start from preconceived convictions about . . . what could have been known and what . . . could not have been known: one should, instead, draw conclusions only from the data as given. That this has to be stressed explicitly reveals the steady decline of scientific ethics."³¹⁴

The doctoring of ancient data that Huber employed, I believe, would be properly described by Santillana and von Dechend as a "cultivated . . . pristine ignorance of astronomical thought."³¹⁵

To support his claim regarding Venus' orbit being always what it is presently, and that the Earth's and Moon's orbits were also unaffected, Huber discusses "intercalary months." These are months added to the year based on lunar months of 29½ days. Twelve lunar months thus equal 354 days, while the present year is 365.24

³¹⁰Immanuel Velikovsky, "Afterword," *KRONOS*, Vol. 3, No. 2, p. 22.

³¹¹Lynn E. Rose (B), "Just Plainly Wrong": A Critique of Peter Huber," (Second installment), *KRONOS*, Vol. IV, No. 2, p. 48.

³¹²Lynn Rose (B), *op. cit.*, p. 49.

³¹³Huber, *op. cit.*, p. 136.

³¹⁴Giorgio de Santillana, Hertha von Dechend, *Hamlet's Mill*, (Boston, 1969), p. 67.

³¹⁵Peter Tompkins, *Secrets of the Great Pyramid*, (paperback ed.), (New York, 1971), p. 175.

days. He claims that in order to explain the Ammizaduga tablets with respect to Venus, "we have seven or eight intercalary months that are attested to by the tablets."³¹⁶ In a private communique to Dr. Robert Bass, Huber emphatically made this point: "Intercalary months were a reality throughout Babylonian history from before Hammurabi to the latest times, and you cannot afford to ignore them when discussing calendaric [sic] matters."³¹⁷

Huber has based his analysis on the view that ancient Near East chronology is basically correct and that he can place his analysis into that chronology. But this is now in question.

How is one to explain this? The problem becomes solved if the established view of ancient historical chronology is wrong. Presently, Dr. Gunnar Heinsohn, in a series of books and papers, has made the claim that Hammurabi is the alter ego of the Persian king Darius the great. He claims that all of Mesopotamian history was created on the basis of false assumptions and is not as long as historians or Huber tell us it is. He claims that there were four or five civilizations in Mesopotamia, not eight, as the historians claim. He has challenged the historians and archaeologists to test his theory on the basis of archeological, stratigraphical, evidence, and that is just what they did!

At a mound in Syria called Tell–Munbaqa, archaeologists and historians claimed that the age of two civilizations are separated from one another by a 700 to 800 year break between their times of living there. Heinsohn said that no such break exists or has ever been proven to exist except in the minds and assumptive historical writing of these historians. Of course, it would be easy to prove Heinsohn wrong with a simple excavation which was carried out in 1988. Well, lo and behold, the excavators discovered no such break existed. They came back the next year with other experts to prove Heinsohn wrong, and again the evidence showed no break; no 700 to 800 years of ancient history ever existed in Mesopotamia except in the minds and assumptions of the archaeologists and historians! This was published in Vol. II of the *Sixth International Congress on Egyptology* in 1993 in Torino, Italy, by Dr. Heinsohn, p. 211.

Heinsohn further challenges the historians of Mesopotamia that there is another long hiatus invented by them and archaeologists and claims that the excavator at this other site has also tentatively claimed that no such break in the history exists. Whether or not Huber understands this, the established history of Mesopotamia has received a death blow. Based on Heinsohn's chronology, Hammurabi's reign falls *after* the calendrical changes in which intercalary months are added to the calendar of Babylonia. Huber, of course, could not have known this when he raised this argument; but Heinsohn's chronology has vanquished much of Babylonian history and if the historians and archaeologists carry out the other digs he challenges them to perform, all of the rest of the established chronology may topple.

Elsewhere in this volume is Professor Lynn E. Rose's paper on Egypt based on calendars and chronology. What Rose has shown was that a supposedly ancient Egyptian dynasty, the twelfth, must be placed over a thousand years closer to our time. Again if this is confirmed by archeological digs in Egypt, which Heinsohn wants to have carried out, the Egyptian chronology may also topple.

This does not prove that Huber's history is wrong, but it is presented to show that what he assumes is correct and settled with regard to history, calendars and chronology, is far from settled or correct.

In Chapter IV of *Science Awakening II*, a book by Bartel L. van der Waerden and co-authored, in part, by Peter Huber, we find,

". . . accurate period relations [such as months] are not found in the earlier texts. For example, the Mul APIN compendium does not give a single period for the sun, moon or planets, apart from the schematic year of 12 months of 30 days."³¹⁸

Here it is admitted by van der Waerden and Huber that there is no "accurate" data "in the earlier texts." The year is given as "12 months made up of 30 days." The word "schematic," meaning "generalized plan," as

³¹⁶Huber, *op. cit.*, p. 125.

³¹⁷Rose (B), *op. cit.*, p. 52.

³¹⁸Bartel L. van der Waerden with Peter Huber, *Science Awakening II*, p. 102.

opposed to "precise measurements" by the ancient Babylonians, is what van der Waerden and Huber have inserted and invented derived strictly from their uniformitarian thinking and assumptions.

That is why Rose charged Huber could not present full documentation of intercalation of months in the calendars between 1400 B.C. to 700 B.C.

But the real proof that Venus' orbit was always stable topples once we examine Huber's *opus magnum* in his statistical analysis of the Ammizaduga tablets.

Remember that for Huber to make his claim, he was forced to change west to east and east to west, to change the names of some of the months and the spacings of the appearances and disappearances of Venus. He also added seven intercalary months to the evidence. Now, with all this moving about of data, did he finally prove, within reasonable limits, that Venus' ancient orbit is almost precisely the same one it follows today? The answer is emphatically No!

How do we know this? Professor Rose compared the present motions of Venus in the chart presented by Huber. He found they do not fit. That is, Rose expected to find within reasonable limits, (a few days or so) that the disappearances and appearances of Venus would correspond to Huber's statistical analysis. He cited another authority, David Pingree, who maintains that if Venus' appearances and disappearances are off by "more than a few days,"³¹⁹ one cannot reliably accept this as confirmation of the tablets. Further, Rose used Huber's own uniformitarian principles to evaluate the analysis. If a period of Venus' invisibility ends before Huber says it should start, that would constitute a statistical miss in the analysis and vice-versa. As Rose points out, "an invisibility cannot end before it begins, or begin after it ends!"³²⁰

Now just how bad were the fits, or more aptly, how far off the mark were Huber's calculations that he claims fit fairly well? Here, then, are Rose's results.

"Huber's graph contains 50 recorded events (disappearances or reappearances [of Venus]) and the 50 'corresponding' retrocalculated events [that should fit his analysis]: Of the 50, only 20 might be considered fits in terms of the criteria . . . stated [above]. *The remaining 30—or 60%—are definitely misses, AND A NUMBER OF THEM ARE MISSES BY MORE THAN A MONTH! ONE EVEN MISSES BY NINE MONTHS . . .* In the printed version of his paper (page 142) **HUBER DETECTED THIS ERROR, BUT DID NOT BOTHER TO CORRECT THE GRAPH** [in which it appeared]."³²¹ (Emphasis, capitalization and bold added)

The utter disdain with which Huber unwittingly admitted that his statistical analysis was worthless is shown in what follows. In order to determine when and where Venus can be seen at the horizon or above it with the sun is the value of the *arcus visionis*. When the sun is at the horizon above it or a few degrees below the horizon, its light is still so bright that only extremely bright objects can be seen in its glare. That is, there is a small arc of vision, *arcus visionis* when Venus cannot be seen in the glare of the rising or setting sun, only when the sun is at or near the horizon.

According to Gerald S. Hawkins:

"The glare of the sun blots out a whole sign of the zodiac . . . the doorposts are the morning and evening twilight zones

"The sun-sign was a mysterious part of the zodiac for the ancients. They could not see what took place in this sign Astrologers call it conjunction with the sun. The Sumerians knew all the planets had to pass through this zone of conjunction. There were no exceptions

³¹⁹Rose, (B), *op. cit.*, p. 45.

³²⁰*Ibid.*

³²¹*Ibid.*, p. 46.

One by one the planets disappeared in the light of the morning dawn and reappeared a few weeks later

"Venus is called the morning star when it is on the right-hand side of the sun. From there it moves into the dawn light as it travels around the zodiac. From morning to morning Venus becomes more difficult to see" ³²²

Ultimately, Venus moves into the full glare of the sun when the sun is above the horizon and cannot be seen. The sun must set to a greater degree of arc *below* the horizon for it to be dark enough to be able to see Venus at or near the horizon. One must have positive values for this *arcus visionis* to conform with the ability of ancient astronomers to explain the observations of Venus when it is on or near the horizon. The positive values mean the sun is below the horizon.

But Rose, in examining this aspect of Huber's work, found that Huber was using *negative arcus visionis* values when Venus would be at or near the horizon. The sun would have been above the horizon and Venus would have to be seen at the same time in its full glare. Under these circumstances, Venus would have to be at the opposite horizon in order to be seen at a horizon. Yet, in terms of Huber's chart, when Venus should be rising it would be setting and, conversely in other cases, when Venus should be setting it would be rising. These sloppy, disjointed and inaccurate data which Huber allowed to remain in his statistical findings, again as with the nine month miss he left in his chart, show his utter disdain for the evidence.

The crass indifference with which Huber allowed such incredibly large errors to remain in his work is testament to the callowness of his research. There is nothing in it to suggest that he used even a modicum of precision or care in handling his data. It was thrown together slap-dash, pell-mell, jerry-rigged and wrong and presented as a finished, refined product when, in fact, it was a hulking mess.

But let us say that Venus was observed exactly where Huber's analysis places it at those times of *arcus visionis*. What does this mean? There are only two ways or a combination of both that will allow Venus to be seen on the horizon at the same time with the sun slightly above the horizon. One way to accomplish this would be for Venus to be very much nearer to the Earth than its present orbit allows. That is, Venus would have to be well out of its present orbit to come near enough to the Earth to be observed at or near the horizon in the glare of the sun with the sun above the horizon. In essence, if Huber's statistical analysis is accepted, Venus would have had to be on a totally different orbit, a much more elliptical orbit than the highly circular one it presently follows. Huber's statistical analysis, simply proves Velikovsky's concept correct regarding Venus having a very different orbit in ancient times. On its present orbit Venus would never have been observed at the same horizon with the sun.

The second way in which Venus could be seen at or near the horizon at the same time with the sun is for Venus to give off a great deal more light and thus be much brighter than it presently appears. That is, Venus would have to be a great deal brighter than it now appears in its present orbit to have been observed near the sun above the horizon. In essence, if Huber's statistical analysis is accepted, Venus would have had to be much brighter than is seen presently. Huber's statistical analysis also proves Velikovsky's concept correct regarding Venus being much brighter in ancient times.

However, these are the phenomena that Huber has set out to disprove. He claimed he would prove Venus was in an orbit quite like the one it follows today, and was no brighter than it is today. But what he has proven in terms of *arcus visionis* is that Velikovsky is correct!

The point to stress is that one cannot expect to have Venus a great deal brighter thousands of years ago and expect it to follow its present orbit or to have Venus in a highly elliptical orbit being just as bright as it is presently. What Huber's intriguing statistical analysis proves is that Venus had to be much brighter than it is today and on a totally different orbit than it is today. Based solely on his statistical analysis, he has disproved what he set out to prove and shows that Velikovsky is correct. Of course he never even gave this clear implication of his analysis a second thought. That is why I call such an analysis a hulking mess. He disproves what he set out to prove.

This is how Huber's statistical study proves Venus has always been on its present orbit. One has to accept Huber changing east to west and west to east, changing also the names of the months, changing the dates of the

³²²Gerald S. Hawkins, *op. cit.*, pp. 37-38

months, changing the intervals between the appearances and disappearances, and adding seven intercalary months where needed. And with all this, Huber got 60 percent of the data wrong; some of the measurements are wrong by more than a month and one by over nine months. Realizing how dismal his findings were, Huber did not even bother to remove the stupendous nine month error from his chart while admitting elsewhere in the text that the error existed and, completely ignoring it; he also gave no attention to *arcus visionis* figures.

Must one believe that such a dismal statistical analysis supports Huber's conclusion? I simply will not accept these kinds of statistics as evidence of anything except Huber's uniformitarian belief system and desire to force feed it to everyone. What he has proven is that Velikovsky is correct.

And that is precisely what one gets at an AAAS symposium when a hobby cuneiformist and statistician sets out to take a set of cuneiform texts of Ammizaduga which,

". . . [s]ince the first effort at explanation . . . [by] Archibald Henry Sayce in 1874, . . . challenged the wit of a score of experts of astronomy and cuneiform philology."³²³

What Huber suggests is that he alone, of all the many great astronomers, cuneiform philologists, and mathematicians, has solved a problem that taxed the best minds who tackled it and failed for a hundred years; and that he just happened to solve this difficult problem in time to present his proof at the AAAS symposium on Velikovsky before the press! This is nothing but grandstanding.

Literally, some of the finest astronomers, mathematicians and cuneiform experts prior to Huber's presentation tried to make these tablets fit Venus' present orbit and failed. For example, Arthur Ungnad in 1940 wrote a special monograph trying to prove the tablets were written with enormous numbers of errors, just as Huber has. But even doing so, he arrived at the same conclusion, that Venus could not have been moving in its orbit as it presently does. He was thus driven to argue that while the Babylonians were careful observers of Venus, they had not yet invented a regular yearly calendar. In other words, Ungnad claimed the Babylonians were expert observers of Venus' motions, appearances and disappearances while at the same time they lacked the simple ability to determine the yearly calendar.³²⁴ This, too, simply is benighted fantasy.

In 1951 Antonie Pannekoek cited an astrologer from Babylonia who supposedly used Venus' appearances and disappearance only as omens to explain why it would appear and disappear in each of the successive months of the year. To make this strategy work, Pannekoek claims this astrologer had Venus and Earth line up on the same side of the sun about every 587 days instead of about 584. Venus then would be invisible for 90 days instead of about 63 to 68 days, when it disappeared behind the sun, and then could be seen to appear for two equal periods of 245 days. Therefore, Venus (if we use our present calendar) would appear in January of one year and reappeared in February of the second year, then reappeared in March of the third year, and so on.³²⁵

But this scheme does not require a single observation to make it work; all it requires is changing and inventing numbers for Venus' orbit. Pannekoek created a compulsive astrologer who, like so many others, was so disturbed by the irregular appearances and disappearances of Venus that he invented an astrological concept detached from reality for Venus' motions. This ancient astrologer was, in reality, a closet uniformitarian astrologer. As Stecchini remarks,

"Pannekoek tries to use the existence of this product of a bizarre mind to discredit, 'observational data.' All that the astrological texts prove is that in Mesopotamia too there were numerological cranks and that in Mesopotamia too there were people who were disturbed by the irregularities documented in the observational reports

³²³Livio C. Stecchini, "Astronomical Theory and Historical Data," *The Velikovsky Affair*, Alfred de Grazia, Ralph E. Juergens, Livio C. Stecchini, eds., (New Hyde Park, N.Y., 1966), p. 148.

³²⁴*Ibid.*

³²⁵*Ibid.*, p. 149.

"As for what concerns the body of the tablets that contain 'observational data,' [of Venus' motions] Pannekoek limits herself to this statement: 'Nevertheless among them there is a large portion that is often most erroneous; hence, there are wrongly copied numbers that at times by incorrect phenomena come out right.' This is what is said in the Dutch text. (p. 250) The reasoning is so contorted that the translator could not cope with [translate] it [properly]" ³²⁶

What Pannekoek was saying was that many of the numbers, dates and other data in the tablets were erroneous, wrongly copied, and are incorrect phenomena but they somehow come out right. Sound absurd?

Franz Xavier Kugler, the greatest authority on Babylonian astronomy, on the other hand, concluded the tablets were responsible reports of careful observations. Therefore, in order to explain them, he had to invent special meteorological factors as, for example, long periods of cloudy weather. But, naturally, this could only suggest that the sightings of Venus were off by, at most, a few days or a week but not by a month or several months.

This brings us to Huber's old source: S. Langdon, J. K. Fotheringham, *The Venus Tablets of Ammizaduga*, computed by Carl Schoch, (Langdon 1928). They showed that the Babylonian astronomers made very precise and careful observations in compiling these tablets. But after manipulating the data as far as they felt rationally justified, they concluded on (page 57), "some of these dates are impossible and some others are highly improbable." According to Stecchini,

"Furthermore, one of the major problems in accounting for these documents is that of explaining why the scribes kept copying through the centuries . . . this particular set of observations." ³²⁷

That is, the Babylonians apparently had one of the most incompetent scribes create a completely fraudulent astronomical document and then many others, as incompetent as he, over the ensuing centuries could not discover for themselves that Venus' appearances and disappearances, which they so slavishly copied, were wrong. This is carrying absurdity to its limits, because the documents any society preserves, as these were, are the ones of greatest significance and meaning in terms of data. And, in particular, this is what the Babylonians were primarily involved in analyzing.

O. Neugebauer states this specifically, "The Babylonian astronomers 'were primarily interested in the appearance and disappearance of the planets.' That is, the Babylonians were concerned with fluctuations, with first and last sightings of planets and stars with 'periodic recurrence' . . ." ³²⁸ With this preoccupation, Huber asks us to believe they allowed an enormous set of errors to be repeatedly copied over long eras. One wonders at what point Huber is willing to deal with reality.

Huber's contribution to the literature on these documents falls into the very same category as all the other failures that preceded him. This would not be so sad if the press hadn't completely accepted Huber's opus as good scientific analysis!

In 1972, John D. Weir also wrote an analysis of this material titled, *The Venus Tablets of Ammizaduga*. He too realized the daunting problems they presented to uniformitarian theory of solar system stability. On page 78, Weir stated that a possible solution to what was recorded in the tablets was that "a relatively large body may have entered the solar system and passed close enough to Venus and to the Earth and moon to cause temporary [sic] perturbations of their orbits." What Weir was offering was that sometime ago, a body large enough, meaning large as a major planet, entered the solar system came close to the Earth and Moon changing their orbits and then did the same for Venus. Weir suggested that the Babylonians had documented this in these tablets, that is, that the orbit of Venus was different enough to be noted by the ancient observers. In essence, Weir suggested that in recent times, the solar system had been unstable! And this he based on the Venus tablets of Ammizaduga. Indirectly, Weir was confirming Velikovsky's claim that in ancient times, mankind observed Venus following a different orbit. All this

³²⁶ *Ibid.*, pp. 149-150.

³²⁷ *Ibid.*, p. 151.

³²⁸ Derek Gjertsen, *Science and Philosophy*, (London, Eng., 1989), p. 63.

was written only two years prior to the AAAS symposium on Velikovsky. It was not an old source based on old interpretations. It was based on modern translations and supported in small measure Velikovsky's views regarding Venus' orbit.

Weir used cuneiform texts that say the tablets of Ammizaduga show Venus was not on its present orbit. Therefore, the text Velikovsky employed and the arguments he put forth, which Huber castigated as "late and secondary sources, [were based,] in part, on absolute and erroneous translations and, therefore, lacked force. Moreover, he sometimes makes a complete muddle of texts, insights, periods and places . . ." Huber's accusations are simply nonsense. Weir, using all the modern texts etc., came to exactly the same conclusion as Velikovsky, that Venus' orbit was different from what it is today. Apparently, when modern texts indicate that Venus' orbit was different from what it presently is, they are not obsolete, erroneous, lack force or are a complete muddle of texts, insights, periods and places . . . When Velikovsky draws the same conclusion about Venus' orbit, as does Weir, he is "flatly contradict[ed]"! Huber's logic is absolutely stunning, to say the least!

Nevertheless, one year later, in 1973, Huber wrote a review of Weir's book in the *Bibliotheca Orientalis*, Vol. XXXI, No. 1/2 page 86. What did Huber himself conclude regarding the possibility of Venus' orbit being different in the past as compared to now? He suggested that this idea was "highly improbable, but not to be excluded," ("ausserst unwahrscheinliche, aber nicht auszuschliessende"). Huber made it clear that one cannot exclude the possibility that Venus, in ancient times, followed a different orbit than it does today. That was only a few months before he came to the AAAS symposium to denounce Velikovsky for saying just that. The idea of solar system instability, *vis à vis* Weir's contention, was not so improbable to Huber that it could be excluded. But at the gathering, when Velikovsky confronted Huber with this material, he replied that, "if something has a very low probability, then you do not believe it."³²⁹

But, if Velikovsky's and Weir's concepts are both of such low probability, why then did Huber say *only* Weir's concept could "not be excluded," but that Velikovsky's could? The statistical evidence must have clearly suggested to Huber that Weir's thesis could "not be excluded." Thus, if one cannot statistically exclude Venus' errant orbital behavior, the question of the cause of that errant, orbital behavior of Venus is thrown open to other explanations, as well-meaning Velikovsky's concept. And that was what Huber would not countenance based on the statistical analysis which he applied to Weir's explanation. What Huber wants is to have it both ways, not to exclude Weir's conclusions on the basis of high improbability, but at the same time, to exclude Velikovsky's conclusions on the basis of high improbability. The logic underpinning Huber's beliefs on this matter is illogical. His belief system is simply based on a double standard. That double standard allows him to exclude Velikovsky's highly improbable scenario, but not exclude Weir's highly improbable scenario. I hope Huber's intentions are becoming clear to the reader.

To make matters worse, Huber neither gave nor gives any statistical evidence to back up his claim regarding Weir's scenario. As Rose explains:

"The fact is that no one has the slightest idea of how many a 'dark star' of Jovian [Jupiter type planets] or smaller size lies in interstellar space: such bodies are not yet within our capacity to detect let alone count. Nor does anyone have the slightest idea how to 'estimate the mean time between two encounters' [of the Earth-Moon system and Venus] with such bodies. Huber was simply offering the audience some pseudo-statistics that he thought might sound impressive; he had nothing with which to back it up."³³⁰

The final argument Huber presented is one that has been used repeatedly in the Velikovsky affair. This is the argument from eclipse data that Newton claimed was based on circular reasoning by the scientists. That is, if Velikovsky's scenario is correct and the length of the year changed in about 687 B.C., then it should be impossible to find high precision evidence of solar or lunar eclipses, based on the present orbits of the Earth and Moon retrocalculated before that time. Therefore, if even one eclipse is authenticated to have occurred before 687 B.C., then the Earth's calendar and year had not changed and Velikovsky would be proven wrong.

³²⁹Rose (B), *op. cit.*, p. 50.

³³⁰Rose (B), *op. cit.*, pp. 50-51.

John Q. Stewart, an astronomer from Princeton University, earlier carried on a debate with Velikovsky which appeared in the June 1951 issue of *Harper's* magazine wherein he said:

"Several modern scholars (notably Fotheringham) have examined Grecian, Babylonian, and Chinese records and listed passages which seem to describe solar eclipses. A brief survey of astronomical publications reveals at least three recorded total eclipses of the sun before -687 (the supposed date of Velikovsky's last catastrophe) which have been considered by computers to fit the present motions. This evidence . . . strongly suggests that no unaccountable disturbance of the motion of the Earth or Moon occurred in that year."³³¹

According to Juergens:

"But Velikovsky in his rejoinder, printed in the same issue of *Harper's* showed that the alleged eclipses, in the original sources, are accompanied neither by dates nor by locality specifications. Moreover, [in] the three mentioned records, the text of one (Chinese) referred to a disturbance of celestial motions which had prevented the occurrence of a predicted eclipse, and commentary about a second (Babylonian) by Kugler, the greatest authority on Babylonian astronomy, called attention to the fact that an eclipse would not be possible at all on the indicated day of a lunar month."³³²

As for the third eclipse, Velikovsky showed,

"A chronicle relates 'insurrection in the city of Ashur. In the [Assyrian] month Siwan the sun was obscured.' The place of observation is not given. Nor the day of the month. The year is named in honor of a magistrate. By retrograde calculation an eclipse should have occurred on July [read June] 15, 763 B.C., if there were no changes. Placing the eclipse in 763 B.C. . . . and assigning the same year to the magistrate, an Assyrian chronology was built [but only] by reconstructing the list of magistrates. However, it required a change of 44 years in Biblical Chronology."³³³

None of Stewart's solar eclipses could be solidly authenticated. He had, as Newton pointed out, made the celestial motion of the Earth, Moon and Sun follow his preconceptions to argue that these eclipses were known, when experts who examined them rejected these dates because the place, date and time of day were not specified, or the texts could not be made to show that an eclipse had actually occurred. Yet he like Huber maintains such evidence is "one of the most imposing demonstrations of the validity of celestial mechanics"³³⁴ and proof against Velikovsky. Having read the Velikovsky-Stewart debate, Julius S. Miller, Professor of Physics and Mathematics at Dillard University, wrote a letter published in the August 1951 issue of *Harper's* saying Stewart's material was characterized by a "glaring paucity and barren weakness of explicit criticism."³³⁵

³³¹J. Q. Stewart, in Immanuel Velikovsky, *Stargazers & Gravediggers*, (New York, 1983), p. 215.

³³²Ralph E. Juergens, "Minds in Chaos," *The Velikovsky Affair*, Juergens, de Grazia, Stecchini eds., (New Hyde Park, N.Y., 1966), pp. 32-33.

³³³*Gravediggers, op. cit.*, pp. 216-217.

³³⁴*Ibid.*, p. 217.

³³⁵Juergens, *op. cit.*, p. 34.

Now Huber follows in Stewart's footsteps based on yet another solar eclipse to disprove Velikovsky. The reader is asked to recall Robert Newton's point about how astronomers reason in a circle. They did not know the place or time of day where the solar eclipse occurred; still they massaged the data to make their assumption fit, and they naturally got the results they were seeking. With all this in the literature for Huber to read, did he present a solar eclipse which could be solidly confirmed in terms of date, place and time of day? No, absolutely not!

Huber was well aware of the debate between Stewart and Velikovsky. In a letter to Velikovsky, Huber claims that Velikovsky was right in his reply to Stewart in *Harper's* to "not accept Fotheringham's eclipses as valid evidence."³³⁶ What did Huber then do? He suggested that an alleged solar eclipse was reported to be observed in China on July 8, -708 (astronomical). But as Rose points out, "Neither the time of day nor the place of observation is given in the report"³³⁷

This eclipse is found in so-called Chinese *Spring and Autumn Annals*, which was translated by James Legge into English. Now how far back do these annals actually go? Do they go back to 800 B.C.? According to Rose, "the *Spring and Autumn Annals*, in its present form, can be definitely traced no farther back than about twenty-one centuries"³³⁸ Therefore, Huber is arguing that an alleged eclipse that occurred in -708 which has no documentation for it until 500 or 600 years later is documented proof that it occurred. Thus, the next question is, was this eclipse actually observed, or did the Chinese astronomers 500 or 600 years later retrocalculate its occurrence, since they could never have observed it? Robert R. Newton admits that this is precisely the problem involved in using Chinese *Annals* for eclipse information. Newton says it was not unusual for Chinese astronomers in the first and second centuries B.C. to retrocalculate eclipses.³³⁹

Why, then, did Huber try to pass off this kind of deeply flawed data as proof against Velikovsky's theory? Was Huber ignorant that this is one of the major problems facing astronomical reconstructions of the ancient past? Again, the answer is no, he was fully conscious of this question and problem. As Rose exposes another of Huber's canards:

"Huber himself should have been well aware that the ancients were just as inclined as are the moderns to rely on computation instead of actual observations. This can be seen from *Science Awakening II: The Birth of Astronomy*, written according to the title page, 'by Bartel L. van der Waerden with contributions by Peter Huber.' In his Preface, van der Waerden says that 'Huber has written considerable parts of Chapters 3 and 4.' And Chapter IV opens with the words: 'A considerable part of this chapter is due to Peter Huber (Federal School of Technology, Zurich).' But in that chapter we find such statements as the following (page 101):

"Very often it is difficult to decide whether text data were observed or calculated. We know from the diaries of later times that missing observations were filled in by calculation sometimes without explicit indication of the fact, sometimes with the note 'not observed,' sometimes with a note that the observation gave a different result. In the case of Sirius phenomena an investigation by A. Sach . . . has shown that calculation was the rule, even when the statement 'not observed' is missing.

"This is also confirmed by Otto Neugebauer. In his *The Exact Sciences in Antiquity*, second edition, speaking of late Egyptian astronomy, he says (page 90):

". . . the dates when a planet enters a zodiacal sign are recorded. These texts are based on computation, not on observations, as is evident from the fact that entrances into a zodiacal sign are also noted when the planet is in conjunction with the sun, thus being invisible" [Arcus Visionis]

³³⁶Rose (B), *op. cit.*, p. 63.

³³⁷*Ibid.*, p. 64.

³³⁸*Ibid.*

³³⁹Robert R. Newton, *Ancient Astronomical Observations and the Accelerations of the Earth and Moon*, (1970), pp. 67-68.

"Indeed, the practice of including materials that were 'not observed' is acknowledged by Huber himself on page 131 . . . and also in his review of Weir. (*Bibliotheca Orientalis*, XXXI (1974), page 88), where he quotes one of these calculated entries that bears the notation 'not observed.'³⁴⁰

R. R. Newton has told us the Chinese in their *Annals* often calculated their solar eclipses; that is, these eclipses were not observed. But Huber tells us without specifying the place or time of day that there was an "observed" solar eclipse in China on July 8, -708 which may very well have been calculated and not observed. Again his criticism fails to be valid. His evidence on this point is pseudo-evidence based on his assumptions and circular reasoning. In fact, his entire paper is based on circular reasoning and ignoring critical evidence. And I do not hesitate to suggest that Huber could not be so ignorant as not to know what he was doing. After all, he was dealing with Velikovsky to educate the public and the press. As Ezra Pound remarked, "Real education must ultimately be limited to men who insist on knowing, the rest is mere sheep-herding."³⁴¹

Indeed, the public and the press followed Huber like sheep down the crooked path he led them. As Rose pointed out,

"After Huber's paper was presented at the AAAS Symposium one of the symposium organizers, Professor Owen Gingerich of Harvard University, who apparently swallowed it whole, was interviewed by Robert Gillette for *Science*. Gillette's ensuing article quoted Gingerich as saying that 'He [Huber] demolished Velikovsky' and that 'There was really no point in continuing after that.' (*Science*, March 15, 1974, Volume 183, page 1061) In response to my inquiry (Rose to Gingerich, March 31, 1978), Gingerich said that, as he recalled, there was an '[i]f Huber was right' in front of the statement that there was no point in continuing, but he also indicated that the report of his position by Gillette was accurate." (Gingerich to Rose, April 26, 1978)³⁴²

Rose will have more to say about Gingerich's commitment to the truth elsewhere in this volume. And as one will see, that commitment is not very deep. It is a great pity that the press did not seek out scholars like Rose instead of being led like sheep by Gingerich and Huber, a hobby-cuneiformist whose rhetoric they believed was supported by real facts when it was all manufactured data! Professor Rose deserves much thanks for exposing the imposture of Huber's analysis. Had the press done its job, Velikovsky's views would not have been distorted and the truth would not have been violated. Joseph Alsop once said,

"All government handouts lie; some lie more than others." A science writer for *Time* feels that they are 'useless, fit for nothing more than the garbage pail.' Another suggests that 'every good reporter needs a good sh__ detector and this holds true especially for science writers.'³⁴³

There can be little doubt that Huber's handout or presentation, like all the others, was directed at the press and through it the public. None of the reporters who must have been aware of the deep felt anger the scientific community had exhibited for thirty years against Velikovsky, were skeptical of establishment science turning over a new leaf. They were led by the AAAS down the crooked path and they too must accept blame for the ugly furtherance of the continuing Velikovsky affair which, by their own consent to follow, they helped to prolong. They did not think to ask whether these handouts by Huber were "lies," "useless," "fit only for the garbage pail." Their

³⁴⁰Rose (B), *op. cit.*, pp. 65-66.

³⁴¹Ezra Pound, *A.B.C. of Reading*, (1934), p. 70.

³⁴²Rose (B), *op. cit.*, p. 67.

³⁴³Dorothy Nelkin, *Selling Science*, (New York, 1987), pp. 128-129.

"sh__ detectors" were either forgotten or ignored. And what ensued helped to contribute, by their lack of skepticism, to promulgating disinformation. That is how Huber helped to educate the public about Velikovsky!

* * * * *

J. DERRAL MULHOLLAND

J. Derral Mulholland is a specialist in celestial mechanics and Professor of Astronomy at the University of Texas at Austin. He was introduced by Ivan King as "a celestial mechanic whose name is almost synonymous with high precision." When we compare this glowing endorsement of Mulholland with King's put-down of Velikovsky, "Most scientists would say that this picture [Velikovsky's scenario] is totally impossible No one who is involved in the organization of this symposium believes that Dr. Velikovsky's ideas are correct"; one can see how determined the scientists were to tilt even the introductions for the press and the public. What he was saying, in essence, was, "We insiders know how ignorant Velikovsky's ideas are and now here's a brilliant specialist who will prove to you that Velikovsky doesn't know what he's talking about."

Now that might have been acceptable if Mulholland didn't prove that he had utter indifference to Velikovsky's theory when he made the following admission:

"Before I am asked the question, I would like to point out that I first read Dr. Velikovsky's work in 1950 in *Collier's* magazine, when I was sixteen years old, and I have read that same work [sic] three times since, the most recent yet this year.

"I found it entertaining when I was sixteen, incidentally, and I still do."³⁴⁴

The *Collier's* article was six large magazine sized pages long, as compared with Velikovsky's almost four hundred page book. What it contained were some excerpts and adaptations from *Worlds in Collision*. It was so bad, in fact, that Velikovsky was forced to protest the manner in which *Collier's* handled his theory and the series was stopped. Could you imagine a pupil saying to his teacher that instead of reading, say Shakespeare's *Macbeth*, he had read a *Classic Comic* of it and found Shakespeare "incidentally entertaining"? The put-down and insulting nature of such a remark could hardly have gone unnoticed except by the press at the AAAS Symposium where Mulholland made this admission.

Velikovsky's scenario suggests that perhaps about 10,000 years ago Jupiter fissioned, giving birth to the planet Venus from its core. Venus eventually had, late in its history, two near collisions with the Earth and then a very near collision with the planet Mars, causing it to move out of its orbit and also eventually having a near collision with the Earth before settling into its now distant orbit. Velikovsky suggests that Venus was born from Jupiter as a comet. Then, thousands of years later, Venus dislodges Mars from its orbit. Even, if Mulholland had only read *Collier's* adaptation of Velikovsky's scenario, he would have known these basic facts about Velikovsky's scenario. Mulholland begins his paper with the following:

"Within the folk memory of man, Venus and Mars erupted into the sky and rushed close to Earth and each other several times, creating unimaginable destruction. The Earth stopped turning, the poles shifted, the year and month changed Finally, the two giant comets settled down into their present harmless orbits and became peaceable planets"³⁴⁵

The title of Mulholland's paper contains the phrase "Velikovsky's Fatal Flaw." Mulholland did not even know the simple fact that Mars did not erupt into the sky as a giant comet to then become a planet. This is not what is found in Velikovsky's scenario. This would be similar to that pupil suggesting one of three weird sisters was Lady Macbeth who came to Macbeth's castle to murder King Duncan. What Mulholland has done is not logical and is based on no evidence. It is *his* scenario, not Velikovsky's, that he presented with that opening remark. But when one is so indifferent to evidence, one reads *Collier's* and does not hesitate to present distortions to the public about

³⁴⁴Immanuel Velikovsky, Lynn E. Rose, (C), "Mulholland: . . ." *KRONOS*, Vol. X, No. 1, (Fall 1984), p. 71.

³⁴⁵J. Derral Mulholland, "Movements of Celestial Bodies—Velikovsky's Fatal Flaw," *Scientists Confront Velikovsky*, (Ithaca, N.Y., 1977), p. 105.

Velikovsky's theory. Mulholland apparently has never had any doubts whatsoever about such things. He imagines that Mars was a comet and this comet becomes, "Hocus Pocus," a planet. He then suggests that:

"If it is the function of science to explain man's relation to his universe, then these are questions of serious significance and should be dealt with seriously."³⁴⁶

By "serious" Mulholland means, cite the source in a magazine and then misrepresent the scenario one is dealing with. This is the "serious" nature of Mulholland's research. The serious reader can understand the nature of Mulholland's approach to Velikovsky. But then Mulholland, having gotten this parody out to the press, does indeed get serious.

"Velikovsky's challenge is not one to be decided on a basis of belief or unbelief. He does not say 'Trust me'; he says 'This conclusion is suggested by the observations.' He strives, it seems to me, to build physically plausible solutions that involve testable ideas. He is not a mystic. He doesn't use little green men with three ears; he uses real planets. [But earlier Mulholland called them comets.] It is not sufficient to reply that his ideas are absurd: there are too many examples of absurd ideas come true.

"Are the explanations plausible? From at least one vantage point, yes indeed. *If* a planet-sized object were to pass close by the Earth, then giant tides would be raised; there would be global earthquakes; the north pole would change direction; the day, the month, the seasons, the year would all change. Faith is not involved here; these are unavoidable consequences of the laws of motion as we presently know them. We must accept that the dynamical aspects of Velikovsky's visions of hell on Earth are largely acceptable. This is not to admit that the events he described ever happened."³⁴⁷

Here Mulholland has put his finger on the one problem the scientists have in dealing with Velikovsky's theory. Mulholland admits all the physical phenomena Velikovsky presented were a natural and realistic outcome of his theory but then he suggests that gravitational "dynamics offers perhaps the most clear-cut contradiction to the evil influence of Venus and Mars."³⁴⁸

Mulholland claims that gravitational theory clearly contradicts Velikovsky's scenario. Before beginning our analysis of the evidence, it should be noted that other scientists examined Velikovsky's dynamical concepts and came to a very different conclusion. Who, one might naturally ask, did so? Well, Albert Einstein, for one, that's who! At Velikovsky's last meeting with him, Einstein said,

"I have again read *Worlds in Collision*. It is a book of immeasurable importance and scientists should read it.' [Mulholland, of course, prefers *Collier's*.] He wondered why Velikovsky needed to challenge . . . celestial mechanics, saying he could explain everything described in Velikovsky's book 'on the basis of accepted celestial mechanics of gravitation and inertia . . . [e]ven the circular orbit of Venus, though this would require a very unusual degree of coincidences."³⁴⁹

³⁴⁶*Ibid.*

³⁴⁷*Ibid.*, pp. 105-106.

³⁴⁸*Ibid.*, p. 106.

³⁴⁹Charles Ginenthal, "Before the Day Breaks—A Perspective," *The Velikovskian*, Vol. I, No. 4, (New York, 1993), p. 102.

Here Einstein, in complete contradiction to Mulholland, claims that Velikovsky's dynamical scenario can be well explained and fit in with gravitational theory. Why, one may again ask, did Albert Einstein conclude that the gravitational aspects of Velikovsky's theory were explicable, while Mulholland concludes just the opposite? Was Einstein so inferior a scientist compared to Mulholland that he could not understand the dynamics of Velikovsky's scenario? If Mulholland is right, then Einstein was not as competent as one would believe him to be, or Mulholland is a more competent scientist than Einstein!

In the early 1950's, Professor Lloyd Motz, astronomer at Columbia University, also claimed, "he had gone carefully through the pages of the Epilogue [of *Worlds in Collision*] that dealt with celestial mechanics and that he could not tell . . . of anything methodologically wrong with . . . [Velikovsky's] hypothesis."³⁵⁰ Here again an authority claims that Velikovsky's scenario in terms of celestial mechanics is possible. What I think is the difference between Albert Einstein, Lloyd Motz and Mulholland is their openness on this question and their attitudes toward solar system instability. Like Martin Gardner, Professor Mario Bunge, and others we will encounter later in this volume, Mulholland as a scientist already knew the answer to this question before he arrived to confront Velikovsky. We will see, over and over again, that evidence plays no role to an individual such as Huber, as it does to an Einstein. Mulholland, on this question, has shut his mind, while Einstein has not. Einstein apparently employed the theory of gravity and came to the conclusion that Velikovsky's theory is not inconsistent with celestial mechanics. Mulholland is going to prove that Velikovsky was wrong and that Einstein was incompetent. Therefore, let us hasten to examine Mulholland's evidence.

To begin, Mulholland argues,

"The occurrence of supernaturally long days and nights pervades folklore all over the globe, and Velikovsky's recitation of numerous passages [of this information] is very impressive, but perhaps sufficiently repetitive to deaden one's skepticism. That the length of time consumed by these events varies from three to ten days is dismissed as due to the inability of the storyteller to determine time accurately. Indeed we are told that the difference between seven and nine days is negligible among a people who are elsewhere said to have clocks of high accuracy."³⁵¹

Mulholland thinks that at the time of a blackout lasting days during a violent catastrophe in which hurricanes were roaring and blowing, earthquakes were shaking the Earth, thunder and lightning were booming and flickering, that those caught in this event expecting death at any minute had nothing more on their minds than keeping accurate time for the length of the catastrophe. Just how would they do that? Well, of course, Mulholland suggests they had very accurate sundials, but in a blackout lasting days such highly accurate timepieces are worthless. Then what about their accurate water clocks? There also has to be enough light to read these as well as sundials, and it was unfortunately quite dark. The people probably had lamps of some kind, but then water clocks needed to be refilled with water constantly because water runs out of these mechanisms. Again can anyone imagine someone who believes that at any moment he is going to die during such a catastrophe if he should somehow light a lamp feel one of the imperative needs at the moment is to get water to fill the water clock, rather than to get water to drink. Mulholland has no understanding of human psychology during times of immense horror and emotional stress if he can suggest that someone involved in such a cataclysm would shout out to anyone in the house who happened to light a lamp, "Quick, fill the water clock!" This is the scientific understanding of human psychology that Mulholland has presented.

Mulholland then states,

"Perhaps more damaging to his case is his report on the geographic distribution of day and night which appears to contain some geometric inconsistencies. No matter where the poles lie,

³⁵⁰ *Stargazers, op. cit.*, p. 121.

³⁵¹ *Mulholland, op. cit.*, p. 107.

no matter what the orientation of the globe, light and dark must each occupy a complete hemisphere. The myths do not seem to satisfy this simple requirement."³⁵²

How do we know that, say, during the long day of Joshua on one side of the Earth, Eurasia had a much longer day, and on the other side of the Earth, the Americas had a much longer night? Mulholland says the myths do not "satisfy this simple requirement." The simple requirement that Mulholland did not satisfy regarding the myths is by not reading the chapter in Velikovsky's book, *Worlds in Collision*, which deals with this hemispheric dichotomy of a long day in Eurasia and a long night in the Americas, titled "On the Other Side of the Ocean."

"In the Mexican *Annals of Cuauhtitlan*—the history of the empire of Culhuacan and Mexico, written in Nahuatl—Indian in the sixteenth century—it is related that during a cosmic catastrophe that occurred in the remote past, the night did not end for a long time.

"The biblical narrative describes the sun as remaining in the sky for an additional day ('about a whole day'). The Midrashim, the books of ancient traditions not embodied in the Scriptures, relate that the sun and the moon stood still for thirty-six *itim*, or eighteen hours, and thus from sunrise to sunset the day lasted about thirty hours."³⁵³

A Spanish scholar, Fernando Montesinos, who came to Peru in 1628 and studied its ancient history for the rest of his life, summed up his research in *Memorias Antiguas Historiales del Peru*, left in the convent library of San Jose de Sevilla. In 1920, P. A. Means translated this into English which analyzed the history. Zecharia Sitchin, in *The Lost Realms*, carefully studied this material, and in his chapter "The Day the Sun Stood Still," dated the "long day of Joshua" and the "long night in the Americas" historically. That is, in Peru, and in Teotihuacan in Mexico, and in the time of Joshua, the long day and night all occur in the same year. Mulholland never mentions this evidence that was clearly written in *Worlds in Collision*. But that's what Mulholland achieves when he reads *Collier's* and entertains his readers without knowing what Velikovsky actually wrote about this hemispheric dichotomy regarding the long day and the long night in Eurasia and the Americas.

To prove that there is no evidence of a disturbance in the direction of rotation of the Earth which Velikovsky posited, Mulholland argues,

"The objectionable feature of the chart in the tomb of Senmut [in Egypt] seems to be that it shows the southern sky as seen from the Southern Hemisphere. It is not necessary to suppose (as Velikovsky does) that this indicates the hemispheres have been physically reversed in space. Indeed this switch would require either that the description of the *northern* sky be anomalous or that the murals represent different epochs. It is enough to know that the Egyptians had already made sea voyages around the Cape of Good Hope and had seen the southern sky pivoting about the other pole."³⁵⁴

Here is how Mulholland explains the sky depicted in the tomb of Senmut located in the Northern Hemisphere. A group of Egyptian sailors circumnavigated Africa and saw the southern sky in the Southern Hemisphere. When they returned to Egypt they told of their discovery to the priests who then had this painted on the tomb of a pharaoh. How do we know that this is what happened? Does Mulholland give a citation that the Egyptian sailors told this to the priests? No. He invents a "just-so" story, claiming indirectly this is just what probably happened.

The simple fact Mulholland could not face is that in the tomb, the constellation Orion and the star Sirius, *both of which can be seen arcing across the sky* in the NORTHERN HEMISPHERE for much of the year, are

³⁵² *Ibid.*

³⁵³ Immanuel Velikovsky, *Worlds in Collision*, (New York, 1950), pp. 45-46.

³⁵⁴ Mulholland, *op. cit.*, p. 107.

depicted. What the Egyptians could see clearly during much of the year is that Sirius would always be following the constellation Orion across the sky. Instead of portraying this arrangement on the tomb, the Egyptians reversed this arrangement to have Orion following Sirius across the sky. This would be a complete reversal of the nature of how the Earth should rotate, but Mulholland cannot deal with this. The Egyptians did not have to circumnavigate Africa to see this celestial arrangement; even in the Southern Hemisphere they would observe this. Their sailors did not have to tell their priests their story, because the priests during the year could see both the star Sirius and constellation Orion in the sky from Egypt. In order for this reversal of position of these celestial apparitions to have occurred, either the Earth's direction of rotation had to be reversed, or the Earth flipped over; the Northern and Southern Hemispheres and poles changed positions while the rotational direction of the Earth remained the same, that is, clockwise.

The problem that Mulholland has studiously avoided in his discussion of Senmut's tomb is that the priests would have always known that Sirius follows Orion across the sky and that Orion does not follow Sirius. With this knowledge, even if the southern sky were seen by sailors and reported, the tomb would still have shown Sirius following Orion across the heavens, not the other way around, as pictured in the tomb. This is an extremely simple piece of evidence that Velikovsky presented. Is it possible that a professional astronomer could not understand the fundamental nature of this evidence?

Mulholland continues,

"Velikovsky's discussion of latitudes suffers two assumptions, either of which may be doubted: first, that observations and clocks were completely accurate twenty-seven centuries ago, and second, that they were made and used at the sites where they were found."³⁵⁵

Mulholland says Velikovsky's assumption about ancient water clocks, which he "doubts," is that they were accurate twenty-seven centuries ago. Following, an historian of astronomy, A. Pannekoek, discusses water clocks that date back to ancient Babylonia:

". . . the Babylonians used waterclocks, in which the passage of time was measured by the outflow of water. Hence it seems safe to consider the numbers as differences in time of transit through the meridian; and they are indeed more or less concordant with the differences in right ascension [the distance measured eastward along the celestial equator meaning the celestial hours of the day] between the stars."³⁵⁶

Pannekoek tells us that ancient water clocks were so accurate that they could be used astronomically to measure right ascension for the passage of stars and the distance in arc degrees between stars. She further suggests that the ancients used these clocks to measure just these relationships.³⁵⁷ But Mulholland says this concept of the accuracy of ancient clocks is something to be "doubted." What is to be doubted is Mulholland's research. He never presented a single piece of evidence or citation about the nature or accuracy of ancient timepieces.

Let us examine this from another viewpoint. Both sundials during the day and water clocks at night could be used to tell the time in ancient times. The question arises: Did ancient civilizations build sundials and water clocks that were so inaccurate that they could not tell the proper time? What Mulholland is indirectly suggesting is that ancient civilizations used completely inaccurate timepieces and that they were too incompetent to build an accurate sundial or make an accurate water clock. While Pannekoek tells us the ancient water clocks could be used to make very precise astronomical measurements of time, Mulholland suggests these timepieces could not even tell the proper time. Like the incompetent scribe, one must accept for Huber's recasting of the Ammizaduga tablets, Mulholland's "doubts" about the accuracy of ancient sundials and water clocks also requires incompetent artisans

³⁵⁵Mulholland, *loc. cit.*

³⁵⁶A. Pannekoek, *A History of Astronomy*, (Dover ed.), (New York, 1989), p. 51.

³⁵⁷*Ibid.*

who constructed these timepieces. Again, if the evidence contradicts Mulholland's preconceptions it is doubtful, while if it corresponds to his preconceptions it is not doubtful.

The second part of Mulholland's argument is that the sundials and water clocks were probably made far from where they were being used. Now, this may apply to a water clock that only tells one the hours, but it would hardly make sense if the water clock had different lengths for the division of the hours during winter and summer; that is, some water clocks divided the day into twelve hours and the night into twelve hours; but since the length of the daylight varies from latitude to latitude over the seasons, one would have to know precisely these differences in time in order to construct a water clock that showed longer hours during the day and shorter hours at night in summer, and shorter hours during the day and longer hours at night in winter. And all this precision Mulholland claims was "doubtful" and that these instruments were built elsewhere from where they were used, causing them to be inaccurate.

What Mulholland cannot face is the fact that Velikovsky showed that clocks and sundials found in Egypt and elsewhere could not tell time correctly based on the present position of the Earth in space. Mulholland had various ways to explain the evidence away so that it would fit his uniformitarian philosophy. Since these timepieces imply that the length of the day was different in the past, the only recourse is to deny their accuracy. On the other hand, if these ancient sundials and water clocks were shown to fit Mulholland's uniformitarian philosophy, one can be very sure that they would be hailed as extremely accurate, and would be called upon as support for the belief that the length of the day and the tilt of the Earth's axis had never changed. Thus, the only evidence Mulholland will accept is that which supports his uniformitarianism; all the rest must be explained away or doubted. As Stephen J. Gould stated, "Theories are built upon the interpretation of numbers and interpreters are often trapped by their own rhetoric. They believe in their own objectivity, and fail to discern the prejudice that leads them to one interpretation among many consistent with their numbers We can . . . show that . . . [one] use[s] numbers not to generate new theories but to illustrate *à priori* conclusions."³⁵⁸ Because it flatly contradicts his uniformitarian philosophy that Orion would follow Sirius across the sky, Mulholland invents a just-so story to evade the evidence and mislead the readers about what this material actually suggests. Since Mulholland cannot even make the numbers on the sundials and water clocks fit his prejudices of a uniformitarian length of day, he casts "doubt" on their accuracy.

However, when it comes to where they are found and what these timepieces imply geologically, Mulholland makes very sure that he is prepared to say anything, no matter how bizarre, to try to destroy this evidence, as well.

"On this basis [that is, where these timepieces were made and that they were used at the sites where they were found] Velikovsky's statements imply that Babylon has moved southward by some 250 kilometers. Further, Velikovsky gives no estimate for the distance covered in his sift in [latitude of the] location of Faijum in Egypt, but states that the sundial found at Faijum does not read true there or at any other place in Egypt. While he does not say that it would read true anywhere else, we can understand that it would only be at a higher latitude. Indeed, Velikovsky implies that Faijum has shifted southward. Finally, the waterclock at Thebes is cited with the emphasis on the clock error rather than on what the error implies. This seems understandable, since Velikovsky's interpretation of the clock error requires Thebes to have moved 1,000 kilometers northward while the other Near Eastern cities moved southward. It is difficult to reconcile these requirements."³⁵⁹

In order to dispute the catastrophic scenarios Velikovsky posited for the Near East that occurred at *different periods of time*, (when the Earth moved north or at another time when the Earth moved south), Mulholland has decided that all of the different motions were simultaneous. This requires that the historians' chronologies for the Near East are all incorrect, except *one*, so that these clocks were being used at the same time. However, the shadow clock of Faijum, described by Velikovsky, has been assigned by historians to the Libyan Dynasty of Egypt. The water clock at Thebes, they say, belongs to an entirely different dynasty or time period, that is, the Theban or

³⁵⁸Broad and Wade, *op. cit.*, p. 197.

³⁵⁹Mulholland, *op. cit.*, pp. 107-108.

Eighteenth Dynasty of Egypt. Only one researcher of this region has ever claimed that both the Libyan and Theban Dynasties both ruled Egypt at the same time. This researcher also claims that these timepieces were employed at the same time in Egypt, and he is neither an historian nor conversant with the information upon which to formulate a chronology. In fact, he knows practically nothing about such research and the chronology of the Near East. The only person who has ever claimed that the Libyan and Theban Dynasties ruled in Egypt at the very same time is an astronomer cum historian named J. Derral Mulholland.

The "Mulholland Chronology" of the Near East is based on an entirely new foundation of research located only in the mind of the originator of that new chronology, J. Derral Mulholland. In order to have these timepieces operating concurrently, Mulholland has rewritten a major part of ancient history.

What Velikovsky claimed is that, based on the established chronologies, none of these timepieces would work where they are located. Neither will they tell the time accurately based on the new chronological fantasy conveniently created by Mulholland for the AAAS symposium on Velikovsky. Not one single reporter ever noticed that J. Derral Mulholland had just rewritten an entirely new chronology for the Near East!

Mulholland ultimately claims that the lengths of the day, the month and the year have not changed in the manner in which Velikovsky suggests. But he can only make such a claim by suggesting that all the measurements made by ancient man that conflict with his uniformitarian preconception are either wrong or to be ignored or doubted.

What Mulholland has done to claim that the length of the day, month and year have remained very nearly the same today as they were in ancient times, is ignore all the measurements made by ancient men, as outlined by Velikovsky in *Worlds in Collision*, or imply that they are wrong. To accept Mulholland's concept one must believe the Chinese were so incompetent that they could not measure the length of the shadow cast by the Sun on the longest and shortest days of the year. The same incompetence must be ascribed to the Babylonians and Egyptians. All the measurements analyzed by Velikovsky and found in ancient records that contradict Mulholland's assumptions must all be in error because they support Velikovsky's views on the changes of the length of the day, month and year.

Ultimately if Mulholland is correct with respect to solar system stability and Velikovsky is incorrect regarding this point, this will be reflected in the gravitational energy or angular momentum of the bodies involved in Velikovsky's scenario. Angular momentum is a measurement of the relationship of the mass and the distribution of mass in a body and spin rate. Angular momentum, therefore, includes both the speed of rotation or revolution of the body and the distance of its mass from the axis or center around which it turns. If, as Mulholland maintains, the planets involved in Velikovsky's catastrophic scenario never changed either their rotation rates or places in the solar system, this will be exhibited by "a smooth sequence of angular momentum . . . by nearly all the planets."³⁶⁰ If, on the other hand, these bodies were disturbed in their rotation periods and revolution, this will also be reflected in their angular momenta that they do not exhibit a smooth sequence, but fall outside the logarithmic graph of this relationship. C. J. Ransom examined this question and pointed out,

"A review of current literature reveals that the smooth function is only smooth if certain information is thrown out in order to make it smooth. Even Mulholland admitted that Mercury, Venus, the Moon and Mars were not considerate enough to conform to the theory. Columbo observes that although Mercury, the Moon and several satellites of Jupiter have odd angular momenta, the behavior of Venus and Mars are much more difficult to explain." (See G. Columbo, *ESRO Planetary Space Missions*, Vol. 1: Basic Data on Planets and Satellites, p. 29, November 1970)³⁶¹

Thus, Mars and Venus, the principal bodies involved in Velikovsky's scenario, have angular momenta that are so different from expectations that they are not explicable in terms of solar system stability touted by Mulholland. Venus has no moon to slow its rotation. Mars' tiny satellites can play a very small role regarding its angular momentum. Rose shows that the linear graph of angular momentum has "one proposed line [that] misses

³⁶⁰Mulholland, *op. cit.*, p. 110.

³⁶¹C. J. Ransom, *The Age of Velikovsky*, (Glassboro, N.J., 1976), p. 129.

Mercury, Venus, Mars, Earth, Neptune, and the Sun, and another proposed line misses all the asteroids, Mercury, Venus, Earth, Neptune and the Sun."³⁶² Can anyone imagine that the solar system could have one analysis which leads to the function of smooth angular momentum missing all four of the terrestrial planets, one of four gas-giant planets and the Sun to boot, considered as a smooth function that Mulholland claims is real; or that the other line should miss three out of the four terrestrial planets, one of the four gas giants, the entire asteroid belt and the Sun?! As Rose commented, "It is difficult to see how a function with so much arbitrariness and so much uncertainty and so many exceptions can be a 'law.'"³⁶³ Based on this evidence, in each case most of the planetary bodies and the Sun contradict Mulholland's contention regarding the smoothness he claims for the angular momentum of a stable solar system. This recalls to mind the boast of a boxer who he had fought and won a hundred bouts. When cornered by a reporter who said he had lost more than half the bouts, the boxer replied: "Well, to be truthful I fought over two hundred bouts and won a hundred so I really didn't lie." But what is Mulholland's excuse for making a claim that is over 50 percent wrong?

If Mulholland's contention is correct, that the Earth, Moon, and Mars never experienced sudden and catastrophic changes in their rotations, then one could hardly expect these bodies to exhibit clear linear features such as cracks, linear rilles, linear ridges, linear albedo boundaries and linear scarps all trending in a few preferential directions. If Velikovsky's view is correct and the rotations of these bodies has undergone sudden changes, then it is only to be expected that they will all exhibit these linear phenomena trending in a few preferential directions. As Ransom explains,

"Surface cracks which tend to be straight for extended distances are called lineaments. Analysis of Mariner 4 photographs of Mars reveals a well-defined system of lineaments. In only eight frames sent back by the early Mars probe about 160 lineaments are apparent. [Alan] Binder remarked 'The presence of these lineaments may indicate that Mars has lost appreciable angular momentum during its history.'³⁶⁴ Later, Fish noted 'The means by which Mars, could have decelerated presents a problem.'³⁶⁵

"Mariner 6 and 7 supplied additional evidence of lineaments. In contrast to the Mariner 4 data these later photographs contained great numbers of readily discernible linear features. Binder and McCarthy say that these data 'demonstrate that the lineaments are expressions of real elements of surface structure that have systematic preferential trends.'³⁶⁶ *These structures are also found on the Moon and Earth. AND ON ALL THREE BODIES THEY ARE SIMILARLY ORIENTED WITH RESPECT TO THE AXES OF ROTATION.* Binder suggests that *all of them may be due to loss of angular momentum.*³⁶⁷ (Emphasis and capitalization added)

Mars has only two tiny moons which could not slow down its rotation by any appreciable amount as the Moon over time does to the Earth. But the extremely gradual slowing of the Earth by the gravitational pull of the Moon would have also acted so gradually that no such lineament system would be expected. Why would the Earth, Mars and the Moon, the bodies involved in Velikovsky's catastrophic scenario, all show not only lineaments but lineaments that are similarly oriented with respect to their axes of rotation? Mulholland, an astronomer must be quite familiar with this well-known data but like the evidence of the ancient clocks has avoided facing this data squarely, as well!

To further support his claim that the Earth's rotation has been gradually slowing and that there has never been a sudden or catastrophic alteration of our planets spin, Mulholland discusses: "Various studies of growth rings

³⁶²Velikovsky, Rose, (C), *op. cit.*, p. 76.

³⁶³*Ibid.*

³⁶⁴A. B. Binder, *Science*, Vol. 152, (1966), p. 1053.

³⁶⁵F. F. Fish, *Icarus*, Vol. 7, (1967), p. 25.

³⁶⁶A. Binder, D. W. McCarthy, Jr., *Science*, Vol. 176, (1972), p. 279.

³⁶⁷Ransom, *loc. cit.*

in fossil materials which show an average increase of 2 milliseconds over the past 360 million years."³⁶⁸ According to C. T. Scrutton,

"Various organisms with accretionary skeletons record the influence of environmental rhythms on their metabolic processes as fine growth increments grouped into a hierarchy of patterns in their skeletons. Based on an identification of the basic increment with circadian rhythm and the patterns with tidal, equinoctial and annual rhythms, increment counts give values for the number of days in the month and year in the past. Contributors **HAVE ASSUMED** that all the available data reflect the solar day and the synodical month."³⁶⁹ (Capitalization and bold added)

What Scrutton has explained is that the lines in shells, corals and stromatalites have also been shown to be reflections of light, pressure, tidal motions and other phenomena, not only of the length of the days per month or months per year. And he tells us that "[m]ost data from invertebrates have been recorded as means."³⁷⁰ This means that these invertebrates' line counts, like those found in the annual rings of trees, are not measured or counted precisely. What the experimenters find instead is a scatter of different numbers in many samples. These are then averaged to get *what is considered* a precise number of days per month or per year. Therefore, such counts should never, when averaged, show a count of less than 365 days per year on fairly modern corals, if Mulholland is correct. As Mulholland claims, "about 60 million years ago . . . the year had 380 day."³⁷¹ The point to stress is that under no condition would one expect corals to show a 360 day year. Based on Mulholland's uniformitarian concept, there has never been a 360 day year in the entire history of the Earth. If the rotation of the Earth has only been slowing down, as Mulholland claims, there would have been only more days per year in the past, never fewer than 365 day years. If Velikovsky is correct, then there may be counts that reflect on average years of 360 days on fairly modern corals.

Professor John Wells, of Cornell University, who did the original studies on corals, "found on average of 360 lines per year on modern corals . . ."³⁷² Modern is the term to reflect the last few thousand years. Here, once again, the evidence Mulholland points to shows Velikovsky's theory is correct! In essence, every piece of evidence Mulholland has turned to as support, supports Velikovsky and contradicts Mulholland's assertions. Mulholland, I suggest, had his mind made up that Velikovsky was wrong long before he ever came to the San Francisco meeting. How am I to back up this accusation? I will let a non-Velikovskian, Irving Michelson, do this for me. According to Rose,

"Mulholland, repeatedly claimed both in his paper and in the discussions, that contemporary celestial mechanics takes into account electromagnetic factors. But when it came down to specifics, he was overly eager to reject such factors, and was not even aware of relevant developments within his own field.

"When Velikovsky mentioned the discovery by Danjon that Earth's rotation rate was temporarily changed following a solar flare, Mulholland denied it: 'it needs to be said that Danjon was wrong about that, that the data do not show any such effect.' Mulholland was also totally

³⁶⁸Mulholland, *op. cit.*, p. 109.

³⁶⁹P. C. T. Scrutton, "Periodic Growth Features in Fossil Organisms and the Length of the Day and Month," *Tidal Friction and the Earth's Rotation*, P. Brosche, J. Sunderman, eds., (New York, 1978),

p. 190.

³⁷⁰*Ibid.*

³⁷¹Mulholland, *op. cit.*, p.110.

³⁷²Robert H. Dott, Jr., Roger L. Batten, *Evolution of the Earth*, (New York, 1981), p. 288; see also

J. W. Wells, "Coral growth and geochronometry," *Nature*, Vol. 197, (1963), pp. 948-950.

hostile towards [Irving] Michelson's willingness to allow electrical effects in celestial mechanics. Both examples will be examined.

"The evening session had begun with Michelson's paper, 'Mechanics Bear Witness.' Although Michelson [who was a direct participant at the symposium] approached the symposium as a neutral and did not speak even a single word in Velikovsky's defense against the charges that Storer, Huber, Mulholland and Sagan had hurled at him, there were nevertheless a number of points in Michelson's paper that had a bearing on some of the subjects under debate

"Michelson's aim was to examine some question of ELECTRO- mechanics, particularly as these relate to Earth rotation." He began by noting that:

'The orthodox assumption in astronomy that stars and planets cannot carry appreciable net electric charges has been widely abandoned during the past twenty years or more.'

"In particular, the Sun could have quite a large electrical charge, and the planets could also have charges.

'If we assign negative electric charges both to Sun and Earth, the resulting electrostatic repulsion force effectively reduces the magnitude of the Sun's gravitational influence. Required adjustment of the solar gravitational constant may be accommodated as a modification of the solar mass value alone, leaving the Newtonian constant unchanged.'

"In other words, the orbital changes that would result from a charged Sun and charged planets could be accommodated by making a slight change that would still fall within the range of uncertainty for that estimate anyway. The orbital formulas in use would not have to be revised at all.

"Michelson's overall conclusion was that *Velikovsky's 'contentions are certainly not at variance with classical mechanics.'*" (Emphasis added) It is no wonder that Mulholland was eager to try to discredit Michelson.

"Toward the end of his paper, Michelson had presented his 'curious and tantalizing' finding that the energy required to turn the rotational axis of the Earth through 180° 'happens to correspond closely to modern estimates of the energy of a single moderately strong geomagnetic storm.' As soon as Michelson finished, Mulholland was on the attack

"Mulholland [said], 'I would like to point out, with respect to this last calculation here, which produced such remarkable results in a correspondence between the energy required to flip the Earth over and the energy expended in a solar flare of great magnitude [Michelson had of course spoken of a geomagnetic storm, NOT a solar flare], (Capitalization added) it falls a little short when one realizes that the Earth as seen from the Sun, represents rather less than ten to the minus eighth power of the total space into which the energy of that flare is expelled there the 10^{23} ergs [at the Sun] results in less than 10^{15} ergs at the Earth. Thank you.'

"Michelson [said]: 'I'll let *that* go. I'll let that go.'

"In *Science*, March 15, 1974, page 1062, Robert Gillette implied that Michelson dismissed Mulholland's question because he had no answer."³⁷³

³⁷³Rose (C) *op. cit.*, pp. 76-78.

At this point Michelson became fed up with what was going on and wrote a reply which was damning both to the AAAS and to Mulholland, as well, and sent it to Gillette for rebuttal in *Science*. The Editor of *Science*, anticipating the damage and understanding the embarrassment the publication of Michelson's letter would create, decided to suppress it, claiming it had already been published in *Pensée*, a pro-Velikovsky journal. In order to open things up, a petition signed by thirty-five concerned scientists, was sent to *Science*, demanding that they refute Michelson's complaint regarding their behavior or else play by the rules and publish his letter. Afraid that this could open up a can of worms, namely publicity about the realities that were operating at the symposium on Velikovsky and having the press pick up the fact that they were suppressing evidence favorable to Velikovsky, they promptly changed their minds and Karlick, on July 2, 1974, called Michelson on the telephone to say they would publish his letter, which they did. Here then is what the journal *Science* wanted to suppress, and one can well understand their reasons for so doing.

He stated that Gillette had,

". . . omitted mention of the irrelevance of the outburst from the floor to which I responded 'I'll let that go.' Those who heard my presentation as symposium panelist were aware that it deserved no reply; your readers are entitled to know a bit more, having been given what Gillette told them."³⁷⁴

Michelson went on to show "that the energy required to turn the Earth's magnetic dipole through 180° (interchanging positions of north and south poles) happened to be equal to that of a moderately strong *geomagnetic storm*."³⁷⁵ Mulholland's argument about the attenuated strength of a *solar flare* that triggers such a geomagnetic storm was, therefore, baseless. As Michelson stated in his letter:

"In the discussion period, someone who wanted to voice an 'objection' talked about the energy of a *solar flare* and the spacial attenuation at Earth's distance from the Sun—declaring that one of my numbers was therefore very wrong. The relevance of solar flare energy to the geomagnetic storm energy confined to the geomagnetic cavity surrounding the Earth is about as small as the Sun's distance from the Earth is large. At most we can say that the sudden influx of charged particles from the Sun triggers geomagnetic storms—their energy is to the energy of the storm as the detonator energy is to the energy released by the bomb it activates."³⁷⁶

What Mulholland had done was try to equate the trigger energy of a solar flare to the explosive energy that it actually releases in a geomagnetic storm. The equation that Mulholland suggested was totally wrong and totally irrelevant like that of his other evidence as described above. In his letter, Michelson, a non-Velikovskian, then gave his evaluation of the symposium and Mulholland though he never mentions him by name:

"There had already been all too much acrimony, backbiting, and anger expressed in the Symposium—and too many long-winded replies to comments from the floor. For me to launch into a lecture explaining the difference between the Sun's solar flare and the Earth's geomagnetic storms to one who either knew it already, or would never know it, while all other's present wanted to get onto more meaningful discussion of real questions raised by my presentation seemed inappropriate. I hoped that most others present knew this was my meaning in arguing to enter into heated or lengthy dialogue with an individual whose zealous opposition to Velikovsky outran his reason."³⁷⁷

³⁷⁴ *Ibid.*, p. 78.

³⁷⁵ *Ibid.*

³⁷⁶ *Ibid.*

³⁷⁷ *Ibid.*

This is, I believe, the main reason that *Science* wanted to suppress Michelson's letter. He had put his finger on the nub of the issue at the debate over Mulholland's behavior. Michelson, in reality, charged that Mulholland's zealotness of opposition had been so great that he didn't care what the evidence was or implied, he was going to get Velikovsky and nothing, not even evidence, was going to stand in his way. He was willing to say anything, no matter how far-fetched, to disprove Velikovsky's concepts because he had his mind made up that Velikovsky was wrong long before he ever came to the San Francisco meeting. And this too I will now make clear.

Once Mulholland had committed himself to the task of destroying Velikovsky, he had to construct, at infinite cost to his intelligence, his dignity and to himself, new barriers against this enemy that he had created. During the discussion period, Velikovsky went on to discuss the findings of Andre Danjon at the Paris Observatory who claimed solar flare energy affected the Earth's magnetic field, whereby it slowed the rotation of the Earth by milliseconds, that thereafter, the Earth's rotation, over a few days, sped up by microseconds and that this had occurred more than once. When Menzel from Harvard University suggested the flare's heat had caused the atmosphere and Earth to expand therefore slowing the rotation, Professor Schatzmann, Danjon's associate, calculated the thermal effect and found it much too small to cause the slowing of the Earth's rotation; he concluded that the cause was electromagnetic. Velikovsky also mentioned that Mulholland came from the Observatory of Paris, where he spends most of his time, and where Velikovsky had communicated with Mulholland.

There was no accusation aimed at Mulholland in Velikovsky's remarks, but Mulholland, from some emotional level, was driven to defend what he perceived as an accusation. He replied that he was not at the Paris Observatory but at Meudon. Now why did he deny that he was at Paris Observatory and stayed, instead, at Meudon? I cannot understand, for the life of me, except that Mulholland felt in some way vulnerable. If he had only given a moment's thought he would have realized the "Observatoire de Paris" is at "Meudon." In fact, the Observatory letterhead read just that, with "Meudon" in smaller print below "Observatoire de Paris."

The simple fact that Mulholland felt that he had to deny his being at the Paris Observatory when this meant nothing, except in his own mind, is proof that at some deeper level he felt threatened and was ready to deny a fact that was inconsequential to the debate. That is, at this point, he was fighting not only Velikovsky's evidence, but the shadow of his own fears.

Apparently feeling that in some way he was losing his battle, Mulholland began to make quite elementary mistakes. In one of the verbal interchanges with Velikovsky, Mulholland argued about the temperature of the cloud tops of Venus which is about

-25°C, but was printed in error in *Pensée IV* as -250° Kelvin. He determined to correct it stating, "[t]hat's not true. C means Centigrade. What they found was -250° Kelvin, which works out to -23°C."³⁷⁸ A few kind speakers had to explain to Mulholland that Kelvin is an absolute scale; there is no minus in the Kelvin scale. When Mulholland said minus two hundred fifty degrees Kelvin he had lost all consciousness of what he was talking about.

Mulholland made a mess of things and proved as Michelson said that his zealous desire to bury Velikovsky had outrun his rational side, the side he should have brought to the symposium. His aim, as with Storer and Huber, was to educate the press and the public, and that he most certainly did. It is pathetic to observe such a display of zealousness gone amuck to destroy an idea.

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³⁷⁸ *Ibid.*, p. 84.

CARL SAGAN

Probably the most influential speaker at the symposium to criticize Velikovsky was Dr. Carl Sagan of Cornell University. He is one of the world's most recognized scientists and science spokespersons. At the symposium he raised so many points critical of Velikovsky's theory that it took this writer eight years to research nearly all of them. These criticisms of Sagan have been in the public domain since my book, *Carl Sagan & Immanuel Velikovsky*, was published by New Falcon Publications in 1995.

This article will concentrate mostly on Sagan's self-contradictions.

About a year ago (1994), I received a telephone call from a Mr. Leo Goldberg, a staff writer for an English publication in Israel. He was doing an article on Velikovsky (which I still have not seen) and wished to talk with me about the issues involved. As soon as he learned from me that I was not a Ph.D. in the sciences, being instead a teacher of the handicapped, his tone and discussion completely changed. I had told him that in my book on Sagan there was fundamental evidence that contradicted what Sagan had said. This he passed off as of no importance coming from an amateur researcher. To make my point, I cited the following from my book, roughly paraphrased: Carl Sagan ridiculed Velikovsky for saying that comets can take on the shapes of different animals through distortion during interactions with other celestial bodies or electromagnetic phenomenon.³⁷⁹ Although Sagan ridiculed Velikovsky for suggesting this, he nevertheless, has a chapter in his own book, *Comet*, titled "A Cometary Bestiary," in which he claims that comets do take on the shapes of animals.³⁸⁰

At the other end of the phone line there came a sudden "What!" from Mr. Goldberg. I assured him that if he wished I would gladly tell him where these citations by Sagan can be located, and that I had many, many other contradictions in my book as damaging as this one. I further pointed out that my being a teacher of the handicapped did not change the facts that had just been disclosed to him. There followed a long silence and Goldberg finally replied, "I don't think my editor would be interested in this material." I sensed he was very uncomfortable with the information he had been given and did not know how to deal with it. This is certainly strange, since here was an indication that Sagan's work on Velikovsky was so scandalous that any journalist might wish to investigate further. This was not the case with this reporter or with the journal he represented. Nor have I found this to be the case with other journals, such as the New York *Village Voice*, whose editor I telephoned and requested that one of his reporters have a look at my book. While he accepted my invitation and I sent the book to him, I never received a reply from him or his reporters. Apparently, some journalists feel that with respect to Velikovsky, Sagan is above criticism.

As Robert Anton Wilson pointed out with respect to Sagan,

"If anybody possesses all the qualifications for a fully ordained Expert in America today, Carl Sagan certainly has that dizzying eminence. Through frequent appearances on TV and in *Parade* (a news magazine circulated through hundreds of newspapers in their jumbo Sunday editions), Dr. Sagan has issued Expert verdicts on every possible controversial issue in science and in politics, and even in theology, for three decades now . . .

"You may wonder how a man who only has qualifications in astronomy can also function as an **Expert** on everything in general. Well, I think it requires Sagan to have a lot of raw courage, in the first place, and a strong well-founded confidence that those who don't believe his dogmas have much less access to the media than he does; if they answer him back, however effective their arguments, very few of his large gullible audience will ever hear about it."³⁸¹

³⁷⁹Carl Sagan, *Broca's Brain*, (New York, 1979), p. 88.

³⁸⁰Carl Sagan, Ann Druyan, *Comet*, (New York, 1985), p. 174.

³⁸¹Robert Anton Wilson, "The Astronomer Who Abolished Gravity," *Cosmic Trigger III, Mask*, (Tempe, Ariz., 1995), pp. 193-194.

Taken together, the reticence of the press to expose Sagan's errors and misrepresentations to the public, and Sagan's ability to reach millions because of his access to the press and media compared to that of his critics, creates an environment where one side, Sagan's, in the Velikovsky debate is, to all intents and purposes, the only side which is propagated widely to the public. Under such conditions, the side with the access to the media determines the entire nature of the public discussion, both its direction and content. Because the other side is barely heard and, in fact, not even listened to, their case is squashed. The spreading of propaganda and misrepresentations over and over again through the press and media by highly respected and influential public figures such as Sagan, creates a witch hunt atmosphere for the person or the ideas under attack.

For example, on October 5, 1993 Sagan spoke to a mass audience of about 5,000 at the University of Florida at Tallahassee. In that audience were two students, Adam Stuart and John Godowski, who wanted to raise the issue of Sagan's statements about Velikovsky. After Sagan's talk there were questions from the floor and Adam Stuart held up my book, *Carl Sagan and Immanuel Velikovsky*, and began to discuss the evidence in it. Sagan would have none of this; the microphone into which Stuart was speaking was killed, and Sagan delivered a statement that scientists do not take Velikovsky seriously. The microphone then miraculously came to life for the next speaker from whom Sagan expected perhaps a new direction of discussion to emerge. That speaker was John Godowski who held up my book and again raised the question in it on Sagan and Velikovsky and, lo and behold, the microphone again went dead so that only Sagan, speaking from the stage, could be heard. He indignantly dismissed Velikovsky's theory and went into a lecture on "critical thinking," which he implied these young men were not employing. Sagan then claimed he had written a 50-page paper in response to the book. However, these students wrote to Sagan requesting this paper and were told in a letter from Sagan that his paper was only about Velikovsky. While he claimed publicly he had answered my book, he admitted privately he had not. How's that for critical thinking?! These students then found, as they went back to their seats, that they were being jeered at by the audience with anger. Critical thinking, in Sagan's terms, could only be accompanied by stopping these young men from completing their statements to the audience so that Sagan could lambast Velikovsky's work.

As Robert Ball explains, "[i]t smacks too much of McCarthyism where if someone calls you something [unscientific] often enough, no matter how often you deny it, there remains a cold doubt. This is unfair" ³⁸²

Sagan did his job on these two young men well enough to cause some of the audience to jeer them creating a social setting which appeared to label these students as heretics, misfits or worse. In Nazi Germany this method was used over and over again in the media. It was purported as truth that Jews were international bankers and in control of the media and intellectual institutions and were working to destroy Germany. No matter how often this lie was denied with evidence, it was not heard above the roar of the Nazi propaganda apparatus. When these methods are employed by a scientist, then reason has been jettisoned.

When Leo Goldberg refused to follow through on the information that had been presented to him about Sagan's contradictory statements about comets and their gases taking on the shapes of animals, he was helping not only to suppress evidence against Sagan, but was helping Sagan to propagate his misrepresentations, which I add is simply a dirty trick. As Wilson states, ". . . Sagan never fails to use every dirty trick he knows," ³⁸³ just as he did with these young men. What are these dirty tricks? What follows is a very abbreviated expose of deception and deceit that Sagan has promulgated against Velikovsky and his work over the last twenty or so years.

For example, Sagan suggests that scientific papers should be submitted to scientific journals for proper peer review "because in it there are adequate standards of validity, which can be agreed upon by competent practitioners the world over." ³⁸⁴ This is the same point raised by Norman Storer.

But Sagan, himself, did not follow this procedure with his theory of Nuclear Winter, which suggests that a large comet or asteroid striking Earth would create so much atmospheric dust and smoke across the world that it would literally bring about a global freeze. When the National Center for Atmospheric Research suggested most of the world would experience a mild nuclear autumn instead of a deep freeze, Sagan simply refused to acknowledge

³⁸²Fred Wendorf in Robert Ball, *Impure Science*, (New York, 1992), p. 16.

³⁸³Wilson, *op. cit.*, p. 194.

³⁸⁴Carl Sagan, "An Analysis of *Worlds in Collision*," *Scientists Confront Velikovsky*, (paperback ed.), Donald Goldsmith ed., (New York, 1977), p. 45, henceforth, *SCV*; Carl Sagan, "Venus and Dr. Velikovsky," *Broca's Brain*, (New York, 1978), p. 83, henceforth *BB*.

merit in their analysis titled "nuclear autumn." Sagan felt he did not have to answer this attack by defending his case in the scientific journals in a scientific manner. In fact, he presented his case in *Parade* magazine a month prior to its appearance in *Science*. *Parade* is not a peer reviewed scientific journal, nor is it a scientific journal of any kind.³⁸⁵ In essence, the advice Sagan gives to Velikovsky, the press, and the public he categorically ignores when it comes to playing by the same norms of science.

Typical of Sagan's dirty tricks is his discussion of an anonymous "distinguished professor of Semitics at a leading university" who called Velikovsky's "Assyriology, Egyptology and Biblical scholarship . . . nonsense," similarly as he did on the stage at the University of Florida.³⁸⁶ Wilson explains what is underhanded about such a criticism.

"Sagan likes to quote a 'distinguished professor of Semitics' who told him no Semitic scholars take Dr. Velikovsky very seriously. Like the 'intelligence officer' who told Newt Gingerich about dope in the White House, this 'distinguished professor' remains anonymous, and thus Sagan's hearsay about him would get thrown out of any civilized court. Three distinguished professors of Semitic studies, however, have all shown cordial support for Dr. Velikovsky: Prof. Claude F. A. Schaeffer, Prof. Etienne Droiton, and Prof. Robert Pfeiffer. Look them up in any *Who's Who* of Semitic studies, archeology and Egyptology. They have a lot more prestige in those fields than Sagan's Prof. Anonymous, who doesn't have a single entry under his name anywhere in the scholarly journals (although elsewhere he receives credit for many olde ballads and almost all bawdy limericks)."³⁸⁷

What Sagan presented was only gossip and he let it loose to do its work.

Too often in his criticisms of Velikovsky, Sagan, like a politician, talks out of both sides of his mouth, saying one thing about Velikovsky in one place, and exactly the opposite in another, when not discussing Velikovsky, as he did regarding comets taking on the shapes of beasts.

Robert Anton Wilson describes one of these peccadillos thus:

"Consider next the high temperature of Venus (480C). As Dr. Roger Wescott and others have pointed out, Dr. Velikovsky predicted a temperature in this range for Venus when astronomical orthodoxy believed that planet much, much colder. Sagan tries to avoid giving Dr. Velikovsky credit for this confirmation of his model by claiming 'many' had predicted a high temperature before the Venus flyby. Actually, he only names *one* other who made such a prediction, Dr. Rupert Wildt, and Wildt's work did not win general acceptance (others try to get around Dr. Velikovsky's correct estimate in this and other instances by describing him as a 'lucky guesser.' That seems mere cage rattling to me. One Could as well call any scientist who made many correct predictions a 'lucky guesser' . . .)

"But the final joker came on page 153 of *Broca's Brain* where Sagan writes (and this really deserves caps):

'ONE NOW FASHIONABLE SUGGESTION I FIRST PROPOSED IN 1960 IS THAT THE HIGH TEMPERATURES ON THE SURFACE OF VENUS ARE DUE TO A RUNAWAY GREENHOUSE EFFECT.' (All emphasis added and deserved)

"First, Sagan claims that Dr. Velikovsky does not deserve credit for predicting high temperatures on Venus because everybody knew it, although historical fact shows that *only* Dr. Wildt had made the same prediction before Velikovsky. Then Sagan either tells a double lie or else suffers an alarming memory lapse that may require neurological consultation claiming that neither Dr. Wildt nor Dr. Velikovsky had made this prediction (which they had, and he had noted

³⁸⁵*Science*, (January 16, 1987), pp. 271-273.

³⁸⁶*SCV*, *op. cit.*, p. 48, *BB*, *op. cit.*, p. 86.

³⁸⁷*Wilson*, *op. cit.*, p. 196.

earlier)—and then he brazenly claims he had originated it himself. Quite a performance wouldn't you say?"³⁸⁸ [What Wilson decries in Sagan is not that Sagan's *runaway* greenhouse theory is sufficiently different than Wildt's simple greenhouse, but that Sagan never mentions Wildt when he discusses his own version of the greenhouse theory.]

To this I can only add the following comment by Charles Kingsley which seems apropos:

"Only one fault he had, which cock-robins have likewise . . . that when any one else found a curious worm, he would hop around them, and peck them, and set up his tail, and bristle up his feathers, just as a cock-robin would; and declare that he found the worm first; and that it was his worm: and if not, that then it was not a worm at all."³⁸⁹

Therefore, the following performance of a one act drama is presented between Sagan, the critic of Velikovsky, and Sagan the scientist when he is not criticizing Velikovsky. This performance stars two dramatic personae: Carl Sagan, Critic of Velikovsky, and his alter ego, Carl Sagan the Scientist who is not criticizing Velikovsky. The drama is titled "Sagan Confronts Sagan." Act I, scene one opens with Sagan the Critic and Sagan the Scientist standing on stage several feet away from one another. Both hold microphones in their hands which neither of them can turn off. In the audience are reporters from all the news media that attended the symposium on Velikovsky. Sagan the Critic will have his errors corrected by Sagan the Scientist. In this production Sagan the Critic of Velikovsky plays the straight man. Whether the drama is a tragedy or comedy is anyone's guess. The quoted material is all by, or reported about, Sagan's work while I have added unquoted observations for both actors.

SAGAN CONFRONTS SAGAN

Sagan the Critic: "Velikovsky[s] . . . statement (page 283) that 'meteorites, when entering the Earth's atmosphere, make a frightful din,' when they are generally observed to be silent";³⁹⁰

Sagan the Scientist: [Nonsense] "Meteorites . . . *can* be heard; they and the fireball produce on occasion a sonic boom or a deep rumbling roar . . ."³⁹¹ "The Herero call them 'buzzing stones' which doubtless reflects some direct experience with meteorite falls."³⁹²

Sagan the Critic: "If, therefore, Velikovsky's proto-Venus comet were a member of some family of objects, like the Apollo Objects or the comets, the chance of finding one Velikovskian comet 6,000 km in radius would be far less than of finding one some tens of km in radius."³⁹³

Sagan the Scientist: [That's not true at all.] "It is entirely plausible that much bigger comets than those several kilometers across were ejected into the Oort Cloud."³⁹⁴

³⁸⁸Wilson, *op. cit.*, pp. 197-198.

³⁸⁹Charles Kingsley, *The Water-Babies*, in Bernard Heuvelmans' *On the Track of Unknown Animals*, abridged ed., (New York, 1965), p. 149.

³⁹⁰*SCV, op. cit.*, p. 55; *BB, op. cit.*, p. 92.

³⁹¹Sagan and Druyan, *Comet, op. cit.*, p. 225.

³⁹²*Ibid.*

³⁹³*SCV, op. cit.*, p. 96; *BB, op. cit.*, p. 322.

³⁹⁴Sagan and Druyan, *Comet, op. cit.*, p. 217.

[As one perfect example] "Chiron is three or four hundred kilometers across, bigger than any known comet, although it is no larger than the bigger asteroids. Could it be the most visible member of a previously unknown population of massive comets that live mainly beyond Pluto."³⁹⁵

"Alan Stern of the southwest Research Institute suggests that they [Titan, the natural satellite of Saturn, 1570 miles in diameter, and Triton, the natural satellite of Neptune, 825 miles in diameter] are two members of a vast collection of small worlds rich in nitrogen and methane . . . Pluto yet to be visited by a spacecraft, appears to be another member of this group."³⁹⁶

"Larger worlds may, for all we know, also be hiding in the dark beyond Pluto, worlds that can properly be called planets."³⁹⁷

Sagan the Critic: "He [Velikovsky] points to certain concordant stories, directly or vaguely connected with celestial [cometary] events that refer to a witch . . . clearly interpretable . . . to culturally isolated peoples of very different backgrounds. No attempt is made to show that a clear form—for example, a woman riding a broom and topped with a pointed hat, could have been produced in this way, . . ."³⁹⁸

Sagan the Scientist: "When we see a picture of a comet some of us are immediately reminded of a woman with long straight hair being blown back behind her, . . ."³⁹⁹ "Pliny noted the appearance of a comet 'too brilliant to be looked at directly; it was white with silver hair and resembled a god in human form' . . . The configuration of a coma can be complicated, and can suggest a human form."⁴⁰⁰ [And you know Dr. Sagan, that witches are women who have a human form.]

Sagan the Critic: "From this it is easy to calculate backwards from simple tidal theory . . . that Velikovsky is talking about a grazing collision: the surfaces of Earth and Venus scrape!"⁴⁰¹

Sagan the Scientist: [Then why did you say just the opposite, in that] "Velikovsky believes that the close passage of Venus or Mars to the Earth would have produced tides at least miles high . . . in fact, if these planets were ever tens of thousands of kilometers away, as he [Velikovsky] seems to think, the tides . . . would be hundreds of miles high."⁴⁰² [You can't have planets like Earth and Venus "scrape" and at the same time be "tens of thousands of kilometers" apart, as Velikovsky maintained they were!]

Sagan the Critic: "Velikovsky's thesis has some peculiar biological and chemical consequences, which are compounded by some straightforward confusion on simple matters. He seems not to know (page 16) [in the early history of the Earth] that oxygen is produced by green-plant photosynthesis on the Earth."⁴⁰³

Sagan the Scientist: "[Sagan's] thesis has some peculiar biological and chemical consequences, which are compounded by some straightforward confusion on simple matters he seems not to know⁴⁰⁴ [in the early history of the Earth that . . . ultraviolet light is lethal to green plants. You see?] "oxygen generated by green plants must have been in short supply before the Earth was covered by vegetation. But ozone is generated from oxygen. No oxygen, no ozone. If there's no ozone, the searing ultraviolet (UV) from the Sun will penetrate to the ground. The intensity

³⁹⁵*Ibid.*, p. 177.

³⁹⁶Carl Sagan, *Pale Blue Dot*, (New York, 1994), pp. 141-142.

³⁹⁷*Ibid.*, p, 143.

³⁹⁸*SCV, op. cit.*, p. 50; *BB, op. cit.*, p. 88.

³⁹⁹Sagan and Druyan, *Comet, op. cit.*, p. 122.

⁴⁰⁰*Ibid.*, p. 180.

⁴⁰¹*SCV, op. cit.*, p. 98; *BB, op. cit.*, p. 324.

⁴⁰²*SCV, op. cit.*, p. 67; *BB, op. cit.*, p. 103.

⁴⁰³*SCV, op. cit.*, p. 68; *BB, op. cit.*, p. 104.

⁴⁰⁴*Ibid.*; *Ibid.*

of UV at the surface of the Earth in those early days may have reached lethal levels"⁴⁰⁵ [and killed the green plants before they could even develop and generate oxygen which would form ozone to protect them]!

Sagan the Critic: ". . . most geologists have concluded, petroleum arises from decaying vegetation of the Carboniferous and other early geological epochs, and not from comets."⁴⁰⁶

Sagan the Scientist: "[Decaying vegetation of the Carboniferous and other geological epochs is organic and, as you stated] "'Organic' only refers to molecules based on carbon. And organic chemicals would be produced and destroyed even if there were no life anywhere in the universe."⁴⁰⁷ ". . . if the Earth never outgassed at all, comets may still have brought an atmosphere, an ocean and huge quantities of organic matter. Thus, in seeking the source of organic molecules from which we [and green plants] come, we are in the embarrassing position of having two different, and apparently equally successful hypotheses."⁴⁰⁸ [So as you can see, petroleum can arise equally from both comets and plants.]

Sagan the Critic: "Reading the text [of *Worlds in Collision*, by Velikovsky] is made still more difficult by the apparent conclusion (page 366) of Martian polar caps made of manna which are described ambiguously as 'probably in the nature of carbon.' Carbohydrates have a strong 3.5 micron infrared absorption feature due to the stretching vibration of the carbon-hydrogen bond. No trace of this feature was observed in infrared spectra of the Martian polar caps taken by the Mariner 6 and 7 spacecrafts in 1969."⁴⁰⁹

Sagan the Scientist: [Don't you remember what you wrote only months before you raised this issue, Carl?] "Mars has in its winter hemisphere a large polar cap which, at various times, has been ascribed to frozen water or frozen carbon dioxide. Even at the present time its composition is unsettled."⁴¹⁰

Sagan the Critic: "Finally, there is a curious reference to intelligent extraterrestrial life [on Mars] in *Worlds in Collision*. On page 364, Velikovsky argues that the near collisions of Mars with the Earth and Venus 'make it highly improbable that any higher forms of life, if they previously existed there survived on Mars.'⁴¹¹

Sagan the Scientist: [Carl, don't you remember suggesting that the low density of Phobos, Mars' satellite, leaves] "only one possibility? . . . Could Phobos be indeed rigid, on the *outside*—but hollow on the inside? A natural satellite cannot be a hollow object. Therefore, we are led to the possibility that Phobos—and possibly Deimos [Mars' other small satellite] as well—may be artificial satellites of Mars.

"They would be artificial satellites on a scale surpassing the fondest dreams of contemporary rocket engineers

"The idea that the moons of Mars are artificial satellites may seem fantastic, at first glance. In my opinion, however, it merits serious consideration. A technical civilization substantially in advance of our own would certainly be capable of constructing and launching massive satellites. Since Mars does not have a large natural satellite such as our moon, the construction of large natural satellites would be of relatively greater importance to an advanced Martian civilization in its expansion into space. The launching of massive satellites from Mars would be a somewhat easier task than from Earth, because of the lower Martian gravity

"Let us imagine that through the next several centuries massive artificial Earth satellites are launched and maintained Perhaps mankind will destroy itself We cannot reasonably assess these possibilities, but it does seem conceivable that the lifetime of our artificial satellites may exceed the lifetime of our civilization. These satellites would then remain as unique and striking monuments to a vanished species which had once flourished on the planet Earth.

⁴⁰⁵Carl Sagan, Ann Druyan, *Shadows of Forgotten Ancestors*, (New York, 1992), p. 26.

⁴⁰⁶*SCV, op. cit.*, p. 69; *BB, op. cit.*, p. 105.

⁴⁰⁷Sagan and Druyan, *Comet, op. cit.*, p. 153.

⁴⁰⁸*Ibid.*, p. 319.

⁴⁰⁹*SCV, op. cit.*, p. 68; *BB, op. cit.*, p. 104.

⁴¹⁰Carl Sagan, *The Cosmic Connection*, (New York, 1973), p. 118.

⁴¹¹*SCV, op. cit.*, p. 71. *BB, op. cit.*, p. 106.

"Perhaps we are observing an analogous situation on Mars."⁴¹²

Patrick Moore Critic and Scientist: (Enters) "Not so long ago considerable interest was aroused by the suggestion . . . that Phobos and Deimos might be artificial and were probably hollow. Amazingly enough, *some newspapers treated this weird idea seriously*, and a paper on the subject was solemnly read before the British Interplanetary Society. At any rate, it is an attractive theory, even if it is about as likely as the age-old hypothesis that our Moon is made of green cheese."⁴¹³ (Emphasis added) (Exit)

Sagan the Critic: "But when we examine Mars as seen by Mariner 9 and Vikings 1 and 2, we find that a bit more than one-third of the planet has a modified cratered terrain . . . and that it shows no sign of spectacular catastrophes other than ancient impacts."⁴¹⁴

Sagan the Scientist: [I wish you'd thought about that before you said it. Don't you remember saying about Mars that] "The resulting geological maps reveal an enormous array of linear ridges and grooves that surround the Tarsis Plateau [on Mars]—as if a third or a quarter of the whole surface of Mars were cracked in some colossal recent event that lifted Tarsis."⁴¹⁵ [How can a colossal recent event that cracked Mars open and lifted the Tarsis Plateau be reconciled with your statement that Mars] "shows no sign of spectacular catastrophes other than ancient impacts?" [And with respect to ancient impacts on Mars, you also did a calculation] "using Mariner 9 wind data, Dr. Carl Sagan of Cornell University calculated erosion rates [on Mars] assuming a dust storm peak of 100 mph blowing 10 percent of the time. This would mean erosion of 10 km [6.2 miles] of surface [erosion] in 100 million years."⁴¹⁶ [On the basis of this kind of erosion rate you claimed] "the rate of sand blasting by wind—transported grains on Mars is perhaps 10,000 times greater than the rate on the Earth . . ." (Carl Sagan, "The Solar System," (San Francisco, 1975), p. 9) [You, therefore, concluded] "Because of the efficiency of Martian crater erosion—regardless of the mechanism—"the surface we see is not that of very ancient Mars."⁴¹⁷ [You can't say Martian craters are ancient and also claim they are not ancient based on erosion.]

Sagan the Critic: [With respect to Velikovskian Manna from heaven] ". . . it is now known that comets contain large quantities of simple nitriles—in particular, hydrogen cyanide and methyl cyanide. These are poisons, and it is not immediately obvious that comets are good to eat."⁴¹⁸

Sagan the Scientist: "In any case, the tails of comets are extraordinarily thin, a wisp of smoke in a vacuum. The cyanogen [poison] is in turn a minor constituent in the tails of comets. Even if the Earth *had* passed through the tail [of Halley's Comet] in 1910 and the molecules in the tail had been thoroughly mixed down to the surface of the Earth, there would have been only one molecule of cyanogen in every trillion molecules of air—a good deal less than the pollution caused even far from cities by industrial and automobile exhaust (and much less than what would happen in the burning of cities in a nuclear war) . . ."⁴¹⁹

Sagan the Critic: [Velikovsky is in error suggesting L. D. Kaplan really meant that there are hydrocarbon clouds on Venus] "He said something like 'I'll tell you what I think' . . . He proposed that hydrocarbons would be splendid greenhouse molecules.

"Kaplan's cautions were not noted by the press, and the next day headlines could be found in many American newspapers saying, 'Hydrocarbon Clouds Found on Venus by Mariner 2.' Meanwhile, back at the Jet Propulsion Laboratory, a group of Laboratory publicists . . . picking up the morning newspaper and saying, 'Hey! I didn't know we found hydrocarbon clouds on Venus.' And indeed, that [Jet Propulsion Laboratory] publication . . .

⁴¹²I. S. Shklovskii, Carl Sagan, *Intelligent Life in the Universe*, (Delta ed.), (New York, 1966), pp. 373-374.

⁴¹³Patrick Moore, *The Planets*, (New York, 1962), p. 96

⁴¹⁴*SCV, loc. cit., BB, op. cit.*, pp. 106-107.

⁴¹⁵Carl Sagan, *The Cosmic Connection*, (New York, 1973), p. 127.

⁴¹⁶*Aviation Week and Space Technology*, (January 29, 1973), p. 61.

⁴¹⁷Carl Sagan, *Intelligent Life in the Universe, op. cit.*, p. 289.

⁴¹⁸*SCV, op. cit.*, pp. 71-72; *BB, op. cit.*, p. 107.

⁴¹⁹Carl Sagan and Ann Druyan, *Comet, op. cit.*, p. 144.

lists hydrocarbon clouds as one of the principal discoveries of Mariner 2: . . . (Velikovsky has chosen to believe only a part of what was printed.)"⁴²⁰

Sagan the Scientist: [But on April 8 and 9, 1963, at a scientific meeting of the Goddard Institute for Space Studies, an office of the National Aeronautics and Space Administration was host to a group of physicists, astronomers and earth scientists who met to discuss the origin and evolution of planetary atmospheres and oceans. Carl, you were at that meeting and spoke about the scientific work of L. D. Kaplan thus] "Kaplan (*Planet and Space Sci.*, 8, 23 (1961)) has looked at a plot of line intensity vs. rotational quantum number in the 7820 Å [angstrom] band and finds two intensity peaks, as if there were a superposition of two Boltzmann distributions. One peak is at a temperature of about 325° K and that the surface temperature [of Venus] approaches 700° K . . . Kaplan points out that these clouds would not be water clouds, since the pressure required for their stability would be too high. He suggests they are clouds of hydrocarbons."⁴²¹ [Kaplan's paper is not related to a press conference but is a scientific paper published in a peer reviewed journal. Are you, Carl, trying to say that L. D. Kaplan not only fooled himself, but also that peer reviewed journal and you as well, so that you cited his work at a scientific meeting? What part of this do you expect us to believe?]

Sagan the Critic: "[Velikovsky] believes that Mars . . . should have a high temperature . . . in the same section he badly states 'Mars emits more heat than it receives from the Sun' . . . This statement is dead wrong."⁴²²

Sagan the Scientist: "It has long been known that the observed temperature of Mars is 30 degrees centigrade higher than would result from the Sun shining on an airless planet at its distance. [As reported in *Sky & Telescope* about Sagan's view on this matter.]"⁴²³

Sagan the Critic: [The surface of Venus is old because] "the planet [Venus] is . . . cratered abundantly; perhaps like parts of the Moon, saturation cratered . . . that is, so packed with craters that one crater overlaps the other."⁴²⁴

Sagan the Scientist: "There is still debate about whether such craters are of impact or volcanic origin, but the sparseness of craters on Venus shows that the surface is continually being modified—probably by vulcanism as on Io."⁴²⁵

[Look at all the contradictions you've made and all the doubletalk. How will all of this look to the public and the press when they read this?]

Sagan the Critic: [Don't worry; it means absolutely nothing. I'm a very, and I mean very, influential scientist and media personality. No one in the press or media will pick this up or pay the slightest attention to Ginenthal's research. He's not a highly respected scientist like I am. And don't forget all my relationships with newspaper people and media people who will take my side as truth in denial of Ginenthal's research. Didn't you see how Leo Goldberg and his editor and also the *Village Voice* handled his work on me? It will be absolutely no different in this case, as well.] (Both exit)

The End

The tragedy is that I believe the closing invented remark of *Sagan the Critic* of Velikovsky to *Sagan the Scientist* has the ring of truth, and that the press and media will do exactly as he says it will. But now let us deal

⁴²⁰SCV, *op. cit.*, pp. 76-77; BB, *op. cit.*, p. 112.

⁴²¹Carl Sagan, "The Atmosphere of Venus," *The Origin and Evolution of Atmosphere and Oceans*, (New York, 1964), p. 282.

⁴²²SCV, *op. cit.*, p. 78; BB, *op. cit.*, pp. 113-114.

⁴²³Anonymous, "American Astronomy Report," *Sky & Telescope*, (March 1961), p. 149.

⁴²⁴SCV, *op. cit.*, p. 84; BB, *op. cit.*, p. 118.

⁴²⁵Carl Sagan and Ann Druyan, *Comet*, *op. cit.*, p. 258.

with Sagan's Ten Plagues which he has put upon Velikovsky. These ten plagues are in the form of ten problems. As C. J. Ransom remarks:

"At the AAAS symposium, Sagan presented ten 'problems' with Velikovsky's ideas. Actually, if you include all the subcategories, Sagan presented many more than ten problems. However, if he referred to twenty or thirty problems, it would detract from likening these to ten plagues of Egypt. These problems (plagues) were supposed to destroy Velikovsky's thesis. Ginenthal [in his book on Sagan] demonstrates that the only thing destroyed was any appearance that Sagan attempted honest research."⁴²⁶

Sagan's ten problems are really ten non-problems because much of the scientific literature is replete with evidence that thoroughly contradicts Sagan's assertions. He has merely invented problems that have been dealt with in the scientific literature, and to those involved in these areas of research, his statements are meaningless attempts to becloud concepts that are well-known and well explored by his scientific colleagues. Like the statements of so many other critics of Velikovsky in this volume, he is in error from beginning to end, and like his self-contradictions, it is impossible to believe he is ignorant of such fundamental evidence, concepts and theories. Let us examine each of these:

Problem I: The Ejection of Venus By Jupiter

Sagan states, "Velikovsky's hypothesis begins with an event which has never been observed by astronomers and which is inconsistent with much that we know about planetary and cometary physics, namely the ejection of an object of planetary dimensions from Jupiter."⁴²⁷

The process Sagan is discussing is called the fission process and is so well-known and well documented in the scientific literature that an astronomer suggesting this is "inconsistent . . . with planetary and cometary physics" is simply pulling the legs of the gullible and incompetent. Sagan uses the authoritarian "we know," when in fact scientists admit they do not know how planets were formed. As Fritz Kahn stated long ago,

"The dust [nebula] hypothesis is as highly speculative as all previous cosmogonies. A cosmogony [theory of the origin of stars and planets] can be nothing but speculative and cosmogonic endeavor will always have 'the smell of the lamp'"⁴²⁸

Alan Nourse states,

"Precisely how this planetary system formed is still a matter of conjecture and debate. A number of theories have been proposed to account for the formation of our solar system as it is.

⁴²⁶Charles Ginenthal, "Scientists, Journalists and Editors as Suppressors (Part II)" *The Velikovskian*, Vol. II, No. 2, (1994), p. 94.

⁴²⁷*SCV, op. cit.*, p. 60; *BB, op. cit.*, p. 96-97.

⁴²⁸Fritz Kahn, *Design of the Universe, The Heavens and the Earth*, (New York, 1954), p. 212.

So far none of them adequately answers all the questions that must be answered to explain fully how the planets came to be."⁴²⁹

Even Carl Sagan admits as much:

"From earliest times the question and evolution of the Earth and the other planets in our solar system has challenged the keenest minds. Philosophers and scientists of the caliber of Kant and LaPlace have wrestled with this problem; yet it remains largely unresolved."⁴³⁰

Although the nebular hypothesis is the more fashionable theory presently, proves absolutely nothing. Scientist do not really know how the solar system or the planets formed. Oh, they have ideas all right, but these are all theory with plenty of assumptions taken as facts, to be pointed out in our discussion of Henry Bauer's work below.

The real question is: Do scientists offer the theory that planets can fission from other planets such as Jupiter? Of course they do, and have been doing so for a long time. Patrick Moore, in discussing the origin of the Moon as first delineated by G. H. Darwin, the son of the great naturalist, Charles Darwin, as long ago as 1879, suggested just this concept:

"Darwin started by assuming that the Earth and the Moon originally formed from one body, and that the Moon was thrown off as a fluid-mass. In a modified version of this idea, the Earth had cooled down sufficiently to form a thin crust before the separation took place, and the sequence of events was worked out in considerable detail. The Earth, rotating rapidly on its axis, was in the state known as 'unstable equilibrium' so that it became egg-shaped, spinning about its shorter axis. Two main forces were acting upon it—the tides raised by the Sun, and its own natural period of vibration. When these two forces were in resonance (that is to say, acting together) the tides increased to such an extent that the whole body became first pear-shaped, and then dumbbell-shaped with one 'bell' (the Earth) much larger than the other (the future Moon). Eventually the neck of the dumbbell broke altogether and the Moon moved away, settling into a stable orbit

"A strong supporter of the fission idea was W. H. Pickering, an American astronomer [from Harvard] whose main interest was lunar work

"For some time the fission theory was regarded as dead, but it has been revived in recent years, though not in Pickering's original form. One variant involves Mars, whose diameter is just over 4,000 miles It has been suggested that Mars was thrown off the Earth and moved away independently, while the Moon is merely a droplet which was formed between the two bodies during the process of separation. But the main support for a fission theory has come from H. C. Urey and John O'Keefe in America whose ideas are based upon studies of the Moon's composition

"There is no doubt that Urey and O'Keefe have made many interesting points. The fission theory cannot be dismissed; but it is fair to say that on the [fashionable] majority view, the Earth and the Moon have always been separate bodies."⁴³¹

⁴²⁹ Alan E. Nourse, *Nine Planets*, (New York, 1960), p. 39.

⁴³⁰ I. S. Shklovskii, Carl Sagan, *Intelligent Life in the Universe*, *op. cit.*, p. 161.

⁴³¹ Patrick Moore, *New Guide to the Moon*, (New York, 1976), pp. 33-36.

Moore tells us, two years after Sagan introduced this criticism, scientists other than Sagan suggested the fission theory of planets being born from planets, "which has never been observed by astronomers [but] is not inconsistent with much that 'we know' about planetary and cometary physics, namely the ejection of an object of planetary dimensions from a planet." Apparently William Pickering, the great American astronomer, was ignorant not to understand this. And so, too, were Harold C. Urey, a Nobel prize laureate, and John O'Keefe not to understand that what they were suggesting was contrary to what WE KNOW (meaning what Sagan says we know).

Ernst J. Opik, the internationally known astronomer from Estonia and researcher of Armagh Observatory in Northern Ireland, states of the Moon's origin from the Earth by fission:

"This remarkable system must have come into being in an equally remarkable manner.
[Opik then reviewed Darwin's theory and adds:]

"Nevertheless, the possibility of the Moon's having broken off from the earth cannot be entirely ruled out If this did happen, the moon itself must have broken up into smaller fragments which collected into the present body on reaching a safe distance from the disturbing earth. It is possible that the separation took place not from the present solid earth, but from a body of larger size and lower density which only later became the present earth and moon

"Therefore, despite the clear outcome of the mathematical theory, it is still possible that the moon broke away from the earth" ⁴³²

Sagan obviously thinks that a world renowned astronomer like Opik didn't understand what "we know." He also suggests, in complete contradiction to Sagan, that planets can be born from planets in spite of the fact that astronomers have never observed this. And Opik, too, says this concept is not inconsistent with planetary and cometary physics. Apparently, Opik was ignorant not to understand that what he was saying was contrary to what "WE KNOW." Each of these two books cited on the fission theory were written in 1976 and 1960, or sixteen years prior to and ten years after Velikovsky wrote *Worlds in Collision*, and fourteen years before and two years after Sagan presented this attack on Velikovsky. Have things really changed since then? In 1970, four years prior to Sagan's statement, T. F. Gaskell stated, ". . . an increasing body of well-informed opinion is swinging toward the old nineteenth-century idea of [Leonard] Darwin that the moon is a cast-off fragment of the earth itself." ⁴³³ He then goes on to suggest that Mars formed from the Earth leaving the Moon as a droplet behind, and further adds "The Venus-Mercury system could be similar to the Earth and Mars" ⁴³⁴ meaning Mercury fissioned from Venus!

Was Gaskell also out of the loop of those in the "we know" group regarding the fissioning of planets from planets? He should have had a long chat with Carl Sagan about his apparent ignorance.

In 1993, almost twenty years after Sagan said "he knows" planets cannot be born from planets, Tom Van Flandern, head of the U.S. Naval Observatory, still didn't get Sagan's "we know" planets cannot be born from planets, and wrote,

"So did the Moon originate from the Earth? Certainly no consensus exists among authorities in this field of research My opinion is that the preponderance of evidence now favors the pure fission theory over the other possibilities. Most significant to me is that the fission theory is the only one which can adopt a starting point and derive a Moon closely like the real one." ⁴³⁵

⁴³²Ernst J. Opik, *The Oscillating Universe*, (soft covered ed.), (New York, 1960), pp. 29-31.

⁴³³T. F. Gaskell, *Physics of the Earth*, (New York, 1970), p. 22.

⁴³⁴*Ibid.*, p. 27.

⁴³⁵Tom Van Flandern, *Dark Matter Missing Planets & New Comets*, (Berkeley, Calif., 1993), p. 270.

How can Van Flandern so flagrantly say that the bulk of the evidence means "we know" planets can be born from planets? And he also has the temerity to state "that Mercury originated by fission [from Venus], as has been proposed for the Earth's Moon" ⁴³⁶

To compound Van Flandern's postulation, "the [fission] theory has recently been revived . . . by Alan Binder and others . . ." ⁴³⁷ Apparently, Binder, a well respected scientist, and his colleagues in 1986 were not paying attention to Sagan's "we know" planets are not born from planets. While Richard S. Lewis points out:

"One of the curious consequences of lunar exploration so far is that in spite of the mass of physical evidence accumulated about the Moon since 1964, there is not enough to exclude any of the major theories of its origin . . . the comment of Don L. Anderson, of the California Institute of Technology, summarizes this circumstance nicely: 'All the classical theories of lunar origin are still with us—capture, FISSION and dual planet accretion' he said." ⁴³⁸ (Capitalization added)

Two years earlier, Sagan "knew" this simply could not be true. But what is this all based on? On Sagan bluffing the entire press corps at the AAAS symposium? What then of fissioning from Jupiter? After all, we have been discussing Mars and the Moon fissioning from Earth and Mercury from Venus; so let us turn to Jupiter fissioning off planets. Do "we" really "know" that this, too, is not a credible hypothesis, as Sagan suggests? Again, Sagan was airily leading the press corps down the garden path. Who then had the audacity to say planets could be born from Jupiter? One of England's foremost cosmologists, Raymond A. Lyttleton, that's who. Here is what I believe is his most terse and accurate description of the Jupiter fission concept. Speaking of the nebular (dust cloud) hypothesis occurring first, Lyttleton states:

"This [nebula] material would initially be at very high temperature, and therefore gaseous in form, and it cannot be supposed that it would immediately condense into planets. What it would do is to spread out into a huge flat disc of material surrounding the sun, rather like a gigantic Saturnian ring, but necessarily much further out from the central body (the sun) in comparison. This ring would cool into separate small solid particles, and these would begin to collect together gradually. It can be shown that small aggregates of varying sizes would form to begin with, but that once formed the largest of them would continue to grow faster, swallowing up some of the smaller concerns too, after the manner of financial corporations under laissez-faire.

"Accordingly what would result to begin with would be that a few large planets would form moving round the sun at distances comparable with those of the great outer planets [Jupiter and Saturn] at the present time. But as such bodies pulled in more and more material from the disc, they must inevitably come to rotate faster and faster, because the disc has indestructible vorticity in it as a consequence of its motion round the sun. The incoming material drawn from further and further away as a mass of the growing planet increases will not at first be concerned about any internal rotational difficulties that it is going to produce for the planet. But when this [accretion] proceeds far enough, the primitive planets, as we may term them, will no longer be able to exist as single bodies, and their rapid rotations will tear them apart . . . Each planet will divide into two separate main pieces, that escape completely from each other, but not necessarily from the sun, and as these recede from one another a stream of much smaller bodies will be drawn out between them, like droplets falling from something drawn out of a treacly liquid, but here the stickiness is produced by gravitational attractive forces. The nearest of them, at the two ends of the stream, will be retained by the attraction of the adjacent main bodies, and remain as satellites moving around them. But the droplets near the middle of the stream will escape from both the main separating pieces and become independent bodies.

⁴³⁶*Ibid.*, p. 251.

⁴³⁷Thomas A. Hockey, *The Book of the Moon*, (New York, 1986), p. 204.

⁴³⁸Richard S. Lewis, *From Vinland to Mars*, (New York, 1976), p. 306.

"Suppose we think of Jupiter and Saturn as two main pieces into which a single large primitive planet originally divided. The four terrestrial planets—Mercury, Venus, the Earth (and moon), and Mars—taken together do not amount to as much as one percent of the mass of Jupiter. So these four comparatively tiny bodies, plus a great deal more debris too, that would go to constitute asteroids, could easily represent the 'droplets' in between two separating large planets. If this explanation is correct in other respects it explains at once why the terrestrial planets are so much smaller than the great outer ones, and it could even explain why our moon is so very similar in both size and mass to each of the four great moons of Jupiter. They may all have been formed adjacently in the self same stream of material."⁴³⁹

It was eleven years after Velikovsky wrote *Worlds in Collision* that one of England's foremost cosmologists, in complete contradiction to Sagan's "we know" planets cannot be born from Jupiter, claimed that planets can be so born. Finally, here is what W. H. McCrea, England's Astronomer Royal, said about Lyttleton's fission theory. "Lyttleton [Lyttleton] has suggested that the Earth, Moon and Mars may originally have formed (from) a single rotationally unstable planet . . . He has shown that this is possible in accordance with the theory of rotating fluid masses and with the dynamics of the solar system."⁴⁴⁰ Here, a world renowned astronomer tells us fissioning of planets from Jupiter is contrary to Sagan's "we know," that it is inconsistent with planetary and cometary physics, but is, instead, consistent with the behavior of such large planets and also consistent with the nature of the solar system. He wrote this nineteen years after Velikovsky stated this view and five years prior to Sagan's first announcement that this cannot occur.

What Sagan probably counted upon to carry out this assault on the truth, was his hope that no one in the press corps would examine his statements by comparing them to what other scientists wrote and published in books and peer-reviewed journals all through that era. He pulled a fast one and largely got away with it!

Problem II: Repeated Collisions AMONG THE EARTH, VENUS AND MARS

Sagan states, "[t]hat a comet may strike our planet is not very probable, but the idea is not absurd" This is precisely correct and it remains to calculate the probabilities"⁴⁴¹ Robert Anton Wilson explains what is essentially wrong with Sagan's calculation.

"In several places, Sagan has published a mathematical proof that several near collisions between a comet and a planet have odds against them of a 'trillion quadrillion to one' (1,000,000,000,000,000,000,000,000,000 to 1)

"Sounds pretty damned improbable doesn't it?"

"The problem here lies in the fact that Sagan considers each collision as an isolated or haphazard event, thereby ignoring gravity. In fact, any two celestial bodies, once attracted to each other, *will tend to continue to approach each other periodically*, according to Newtonian laws unmodified by Einstein. This periodicity will continue until some other gravitational force pulls one of the bodies away from the gravitational attraction of the other. Ask any physics or astronomy professor about this, if you think I'm pushing too hard here. As Robert Jastrow of NASA's Goddard Institute of Space Studies wrote (*New York Times*, 22 December 1979), 'Professor Sagan's calculations in effect ignore the law of gravity. Here, Dr. Velikovsky was the better astronomer.'

⁴³⁹Raymond A. Lyttleton, *The Modern Universe*, 3 ed. revised, (London, Eng., 1961), pp. 170-172.

⁴⁴⁰W. H. McCrea, *Nature*, Vol. 224, (1969), p. 28.

⁴⁴¹SCV, *op. cit.*, p. 62; BB, *op. cit.*, pp. 98-99.

"[Professor] Robert Bass [at the time from Brigham Young University] wrote, even more harshly, '[t]his Sagan assumption [ignoring gravity] is so disingenuous that I do not hesitate to label it a deliberate fraud on the public or else a manifestation of unbelievable incompetence or hastiness combined with desperation.'"⁴⁴² . . .

"Well, I always had doubts about Sagan's ability to pronounce verdicts outside astronomy. When he does calculations *inside* astronomy and then ignores or forgets gravity, I begin to wonder about his competence in general. Perhaps the misfortunate man needs a guide or keeper to lead him about and insure that he doesn't bump into buildings.

"As far as I can see, Sagan's greatest area of ability lies in one truly well-proven and absolutely undeniable talent—for getting himself promoted in the mass media as an authority on everything in general, even though he seems to have no competence at anything in particular."⁴⁴³

Wilson titled his chapter on Sagan, "The Astronomer Who Abolished Gravity."⁴⁴⁴

Sagan has suggested that five or so near collisions altogether between the Earth and Mars and the Earth and Venus are so highly improbable that it is simply ridiculous and cannot be taken seriously. But then Sagan states that for comets to be captured from either the Oort Cloud or Kuiper Belt of comets by the solar system and be funnelled into the inner part of the solar system, "most short period comets may have achieved their orbits by multiple gravitational encounters with Jupiter, or even by multiple encounters with more distant planets and eventually with Jupiter itself."⁴⁴⁵ R. D. Chapman and

J. C. Brandt, authorities on comets, tell how many encounters are necessary to bring a comet into the inner regions of the solar system.

"Originally it was thought that a single close encounter with Jupiter would suffice [to capture a comet] . . . In fact, single encounters can capture comets into short-period orbits. However, such encounters are very rare and cannot account for the observed number of short-period comets. Instead it now seems that the capture of long-period comets into the inner solar system *results from accumulated perturbations of HUNDREDS of not-so-close interactions with Jupiter* and to some extent, with the other giant planets."⁴⁴⁶ (Emphasis and Capitalization added)

Sagan tells us that a halo of "trillions of comets are now widely accepted by astronomers all over the world and called properly the Oort clouds."⁴⁴⁷

Hence, Sagan and his colleagues accept that long-period comets come into the solar system from the Oort Cloud and then have *hundreds of near encounters* with Jupiter and the other giant planets to become short period comets. Velikovsky, on the other hand, has a theory that is too improbable to accept because it suggests *five or six near encounters with the Earth* by Venus and Mars. The logic underlying this is as follows: It is acceptable and probable to Sagan and to his astronomer colleagues that a body on an elliptical cometary orbit can have hundreds of near encounters with planets, but it is unacceptable and improbable to Sagan and to his astronomer colleagues for a body on an elliptical cometary orbit to have five or six near encounters with planets. The logic underlying such a conclusion is so bereft of reason that it boggles the mind. And this is what Sagan passed off to the public and the press corps as rational scientific analysis.

⁴⁴²C. J. Ransom, *Age of Velikovsky*, (Glassboro, N.J., 1976), pp. 225-226.

⁴⁴³*Wilson, op. cit.*, pp. 198-199.

⁴⁴⁴*Ibid.*, P. 193.

⁴⁴⁵Carl Sagan and Ann Druyan, *Comet, op. cit.*, pp. 95-96.

⁴⁴⁶R. D. Chapman, J. C. Brandt, *The Comet Book*, (Boston, 1984), p. 88.

⁴⁴⁷Carl Sagan and Ann Druyan, *Comet, op. cit.*, p. 201.

Problem III: The Earth's Rotation

In this problem, Sagan argues that if the Sun stood still in the sky during the long day of Joshua, "the most serious objection is rather at the other end. How does the Earth get started up again rotating at approximately the same rate of spin? The Earth cannot do it by itself because of the law of the conservation of angular momentum."⁴⁴⁸ But does the Earth have to cease rotating to see the Sun slow as it crosses the sky? The legends tell of a long day in Eurasia and a long night in the Americas, not that the Sun stopped its motion; the Sun moved across the sky much more slowly!

According to Velikovsky's hypothesis, Venus would be approaching the Earth from the sunward side. This would cause the Earth to be pulled gravitationally inward toward the Sun. With the Earth in a closer orbit to the Sun and rotating at the same speed, the Sun would appear to move along a path across the sky more slowly. Once Venus passed outside the Earth's orbit, it would gravitationally pull the Earth away from the Sun to very near its original distance from the Sun. In this circumstance, the Earth's rotation would have changed extremely little and the problem Sagan envisions evaporates.

If one believes that such a process is not possible, I refer that skeptic to that well-known astronomer, Carl Sagan, who describes a very similar phenomena for Mercury, which has an orbit elliptical enough so that when it is closest to the Sun, the observed path of the Sun not only stops, but actually moves backward across the sky, and as Mercury arcs away from the Sun along its orbit, the Sun then moves forward across the sky. Mercury's rotation does not stop at all to create this appearance. As Sagan explains,

"There is another strange thing about Mercury. It has a highly elliptical orbit. That is, there is a commensurate relation between how long the planet takes to turn once around its axis and how long it takes to go once around the Sun Suppose you stood at one particular place on the equator of Mercury. During the course of a day there you would observe the Sun do the following. You would see it rising . . . moving toward the zenith Then, one degree past the zenith, it stops, reverses its motion in the sky, stops again, then continues its original motion"⁴⁴⁹

Because Mercury rotates so slowly, 58.667 days long as compared to that of the Earth, the length of the day and night are extended for several days.⁴⁵⁰ So as one can see, Sagan knew that the Earth does not have to come to a dead stop and then begin to rotate again to have a longer day in Eurasia or a longer night in the Americas where this is indeed stated in the ancient literature. But, of course, Sagan never mentioned this in his criticism of Velikovsky's work.

⁴⁴⁸SCV, *op. cit.*, p. 64; BB, *op. cit.*, p. 100.

⁴⁴⁹Carl Sagan, "The Planets," *Man and Cosmos*, J. Cornell E. N. Hayes, eds., (New York, 1975), p. 80.

⁴⁵⁰Patrick Moore, *The Unfolding Universe*, (New York, 1982), p. 65.

Problem IV: Terrestrial Geology and Lunar Craters

Here Sagan states,

"To the best of my knowledge, there is no geological evidence for global inundation of all parts of the world either in the eighth or in the fifteenth century B.C. If such floods occurred even if they were brief, they should have left some trace in the geological record. And what of the archeological and paleontological evidence? Where are the extensive faunal extinctions of the correct date due to such floods. And where is the evidence of extensive melting in these centuries, near where the tidal distortion is greatest."⁴⁵¹

Sagan claims he cannot find in the Velikovskian literature evidence that the Earth experienced great recent catastrophes. This is so very like the Aristotelian professors in the time of Galileo who refused to look through the newly invented telescope, or claimed that the telescope distorted what it saw in the heavens. Velikovsky wrote an entire book, *Earth in Upheaval*, to answer precisely these accusations made by others before Sagan who raised this same issue.

"Velikovsky's opponents pointed out that he was talking about events that qualified as catastrophes, transcending anything that is now going on in scale and violence. They declared that this put Velikovsky out of court because the uniformitarian doctrine provided no room for such events. Velikovsky who had practiced medicine and was rather innocent as to Anglo-Saxon geological theory, was surprised at this reaction and at the violent feelings he had aroused. His response was admirable; without extensive public recrimination, he disappeared into the library for several years and compiled a book called *Earth in Upheaval* (Doubleday 1955). Here he marshals the original field reports on a large number of phenomena that point inexorably to catastrophes and (as a by-product since he was looking for events rather than dates) to fairly recent dates for the catastrophes. The impact of the details and the number of phenomena (close to forty) is shattering. I hold no brief for Velikovsky's theories, but I am indebted to him for collecting material that had never been assembled in one place before.

"The topics in the book are discussed on the basis of reports by orthodox and reputable scientists, with Velikovsky merely acting as master of ceremonies."⁴⁵²

"The reader should peruse Velikovsky himself so as to get the cumulative effect of his evidence, and he should also look at some of the original material . . . The wealth of specific cases pointing toward catastrophes make it impossible for me to accept the uniformitarian theory . . . Catastrophes have been taboo for a century among the orthodox."⁴⁵³

Sagan, on the other hand, claims he simply cannot locate such information. Is that truly believable?

"The late Dr. Albert Einstein, during the last eighteen months of his life (November 1953—April 1955), gave me much time and thought. He read several of my manuscripts, and

⁴⁵¹ *SCV, op. cit.*, pp. 67-68; *BB, op. cit.*, pp. 103-104.

⁴⁵² Norman Macbeth, *Darwin Retried, an appeal to reason*, (Ipswich, Mass., 1971), pp. 110-111.

⁴⁵³ *Ibid.*, p. 116.

supplied them with marginal notes. Of *Earth in Upheaval* he read chapters VII through XII; he made handwritten comments on this"⁴⁵⁴

Sagan claims that the book that Albert Einstein advised Velikovsky about does not seem to exist, to the best of his knowledge. Professor Harry H. Hess, Professor of Geology at Princeton University, President of the American Geological Society and Chairman of the Space Science Board of the National Academy of Science, told Velikovsky "that he knew the book by heart."⁴⁵⁵ But Sagan claims this book, so carefully memorized by one of America's foremost geologists, is too obscure to be known, to the best of his knowledge.

"When the manuscript of *Earth in Upheaval* was complete, I asked Professor Jepsen to read it; he pleasantly consented, but after a while he called me back and asked to be excused from the task since there was opposition in the department. However, in Professor Jepsen's paleontology course at Princeton, *Earth in Upheaval* was required reading for the next two decades, from its publication on."⁴⁵⁶

But Sagan doesn't want to know any of this. *Earth in Upheaval* is so difficult for him to deal with that he will have nothing to do with it. Sagan's desire to avoid this book and its evidence and keep it closeted and unknown to the public as if it was evil is certainly interesting. The reader is urged to read that book to understand what Sagan seems to be so desperate to keep hidden. With respect to that book, Sagan's shibboleth is "See no *Earth in Upheaval!* Hear no *Earth in Upheaval!* Say no *Earth in Upheaval!*" His fear of squarely facing this catastrophic geological, paleontological and archeological evidence shows, in detail, the depth of his aversion to dealing honestly with this material. Like the Aristotelian professors who would not look through the telescope at the evidence that clearly contradicted their dogmas, Sagan will not look into *Earth in Upheaval* at the evidence that clearly contradicts his own dogmas.

Problem V: Chemistry and Biology of the Terrestrial Planets

Several of the points Sagan makes in this problem were presented in our little dialogue, "Sagan Confronts Sagan." These were the points where Sagan said oxygen originated in the early Earth's atmosphere from green-plant photosynthesis, but then admits that without oxygen in the atmosphere, ozone will not form and ultraviolet light will kill these early plant forms. He also made the argument that petroleum comes from decaying plants from the Carboniferous epoch and not from comets, but then says that it is also probable and feasible that these organic products came from comets. He made another argument that Mars' polar caps do not contain organic substances, while at the same time admitting the composition of these polar caps is unknown. Further, he argued that there was never intelligent life on Mars, while having presented the view that Mars' satellites were placed in space by ancient intelligent life forms. He also argued that Mars shows no signs of recent catastrophes and its craters are ancient, while he claims the Tarsis plateau and its associated lineaments appear as though the planet suffered a colossal, recent catastrophe and that the erosion rate on Mars would remove the surface craters in a very short time which might lead one to conclude that these surface features are much younger than previously assumed.

Nearly every point he made in this problem against Velikovsky he contradicted by saying just the opposite about these concepts elsewhere, and yet he has the nerve to say "Velikovsky's thesis has some peculiar biological

⁴⁵⁴Immanuel Velikovsky, "Acknowledgements," *Earth in Upheaval*, (New York, 1955), p. IX.

⁴⁵⁵Immanuel Velikovsky, *Stargazers & Gravediggers*, (New York, 1983), p. 326.

⁴⁵⁶Immanuel Velikovsky, *Stargazers*, *op. cit.*, p. 323.

and chemical consequences, which are compounded by straightforward confusions of simple matters."⁴⁵⁷ Sagan's work on these points is not only peculiar but completely disjointed.

Problem VI: Manna

In this problem, Sagan also contradicts and misrepresents Sagan wherein he argued that comets contain hydrogen cyanide and methyl cyanide and are, therefore, not good to eat. He then wrote that the amount of these materials in comets is so minute that they could not poison anyone. He then ridiculed Velikovsky suggesting "Moses, however, may have been a better chemist than Velikovsky."⁴⁵⁸ However, he never has the courage to tell his large audiences or readers that Manfred A. Hollinger, of the University of California at Davis, Department of Pharmacology, showed that Sagan chemically could not distinguish the difference between a hormone and a *hallucinogen*, and stated he finds 'it distracting that the general public is exposed to [Sagan's] pseudoscientific writing that would not pass a credible thesis committee.'⁴⁵⁹ In this case, in Sagan's pseudoscientific writing, one does not know if Sagan is feeding the public and press corps hormones or hallucinogens. I personally opt for hallucinogens to make Sagan's comments in this problem palatable. As Andre Malraux wrote, "There are certain persons for whom pure truth is a poison."⁴⁶⁰

One most ironic aspect of this is that Sagan, at an Overseas's Press Club gathering, talked about a "baloney detection kit" to protect the people from those "not eager to tell the truth."⁴⁶¹ But Sagan was perfectly willing to serve cometary cyanide to the public to ridicule Velikovsky.

Problem VII: The Clouds of Venus

Here Sagan says that the clouds of Venus are not composed of hydrocarbons and that he knows "the question of the composition of the Venus clouds—a major enigma for centuries—has recently been solved The clouds of Venus are composed of an approximately 75 percent solution of sulfuric acid."⁴⁶² That is quite an announcement Sagan made in 1974 but, once again, only the gullible and naive took the bait.

The *Encyclopedia Britannica Macropedia*, for 1982, Vol. 2, page 327, eight years after Sagan proclaimed this established fact about sulfuric acid clouds in Venus' atmosphere, wrote:

"The Venus clouds, which cover the planet at a height of 60 to 70 kilometers (40 to 45 miles) shield the surface from ultraviolet radiation. The composition of those clouds remains uncertain; ice and mercurous chloride have been proposed, but there are difficulties with either hypothesis."

Why didn't the *Britannica* make any mention about sulfuric acid clouds? The articles in the *Britannica* are written by the world's leading authorities, yet eight years after Sagan's proclamation at the AAAS symposium the authorities say in complete contradiction to Sagan, "[t]he composition of those clouds remains uncertain." Whom

⁴⁵⁷ *SCV*, *op. cit.*, p. 68; *BB*, *op. cit.*, p. 104.

⁴⁵⁸ *SCV*, *op. cit.*, p. 72; *BB*, *op. cit.*, p. 107.

⁴⁵⁹ Manfred A. Hollinger, *Atlantic Monthly*, (June 1979), pp. 29-30.

⁴⁶⁰ Andre Malraux, *Ariel*, (1924), "Chap" 29.

⁴⁶¹ Fred Ferguson, *Overseas Press Club Bulletin*, (Vol. 43, No. 5), (May 1988), p. 1.

⁴⁶² *SCV*, *op. cit.*, p. 75; *BB*, *op. cit.*, p. 111.

should we trust, Sagan or the *Britannica*? But let me add to what I stated below with regard to David Morrison and Clark R. Chapman on this point:

John S. Lewis of M.I.T., in 1981, seven years after Sagan proclaimed that Venus' clouds are made up of sulfuric acid, said,

"The clouds of Venus have been a favorite topic of controversy, and here the matter is still in a very uncertain state. Half a dozen species [of gases] are currently favored by different individuals as making up the visible clouds. Among the most widely advertised are water or ice, silicate and carbonate dusts, ammonium chloride, compounds of the volatile elements, mercury, arsenic, etc., carbon suboxide and its polymers, hydrochloric acid solution or solid hydrates of ChL, ferrous chloride dihydrate, etc. Each species [of gas] has more detractors than supporters."⁴⁶³

Why didn't Lewis make any mention about sulfuric acid clouds? Does this parade of constituents in the cloud material of Venus being offered by scientists suggest, in any way, that the enigma that has puzzled scientists for centuries has been solved, as Sagan claims? Whom should we trust on this issue, Sagan or Lewis?

Billy P. Glass apparently has also not gotten Sagan's message that the clouds of Venus are made up of sulfuric acid; he wrote in 1982, eight years after Sagan made his claim,

"The nature of the clouds [of Venus] has been a question of great interest for a long time. Speculative interpretations [regarding] the principal constituent of the clouds include: water drops, ice, frozen carbon dioxide, carbon suboxide, mercury, halite, ammonium nitrate, ammonium chloride, silicate dust particles, carbonate particles, formaldehyde, hydrocarbon droplets, partially hydrated ferric chloride and hydrochloric acid."⁴⁶⁴

Again, eight years after Sagan's proclamation, Glass gives not the breath of a hint that the clouds of Venus are made up of sulfuric acid. Doesn't Glass know he is wrong and should apologize to Sagan for grossly contradicting him about the make-up of the clouds of Venus? Again, whom shall we trust, Sagan or Glass?

If Sagan is still not convinced by all this, it is suggested he reread *The Planetary System* by David Morrison and Tobias Owen. In the foreword of that book, Sagan lauds the authors, whose book is "marked by a judicious and comprehensive selection of topics [with] clear qualitative explanations,"⁴⁶⁵ as "pioneers in the modern exploration of the solar system." What did these two pioneers say thirteen years after Sagan said the clouds of Venus are made up of sulfuric acid?

"Space probes [to Venus] that have passed through the clouds have given us a picture . . . of discrete cloud layers But what are the various clouds made of? Are they all sulfuric acid, as are the visible [top most] layers? Only the Soviet probes have attempted compositional measurements and their results have been contradictory. Sulfur or possibly chlorine compounds of some sort are indicated, but *their exact identities are unknown*."⁴⁶⁶ (Emphasis added)

⁴⁶³John S. Lewis, "The Atmosphere, Clouds and Surface of Venus," *The Solar System and Its Strong Objects*, B. J. Skinner, ed., (Los Altos, Calif., 1981), p. 93.

⁴⁶⁴Billy P. Glass, *Introduction to Planetary Geology*, (New York, 1982), p. 310.

⁴⁶⁵Carl Sagan, "Forewords, *The Planetary System*, David Morrison, Tobias Owen, (New York, 1987), p. V.

⁴⁶⁶David Morrison, Tobias Owen, *The Planetary System, op. cit.*, p. 236.

Morrison also claims, on page 234, that the clouds are sulfuric acid. But this is merely game playing, since on page 235, after presenting a comparison of the reflectivity of various constituents, he then hedges his claim "that the clouds of Venus *could indeed* be made of sulfuric acid." (Emphasis added) "[C]ould indeed be" is again doubletalk.

Trying, as one may, to make the clouds of Venus sulfuric acid simply doesn't work. Sagan lauds the book that contradicts his sulfuric acid cloud model for Venus. No, I'm afraid Sagan cannot be trusted on this matter. As we pointed out above, it is now admitted that one of the probes to Venus found a great deal of methane, a hydrocarbon, on Venus, some of Sagan's colleagues are suggesting with a straight face, that this methane was vented by a volcano which would do this only once every 100 million years. This is so improbable an explanation that it requires one to swallow one of Sagan's hallucinogens to believe it.

Problem VIII: The Temperature of Venus

In this problem, Sagan argues that the source of Venus' heat is a runaway greenhouse effect, and not the planet itself.

"We now know from ground-based radio observations and from the remarkably successful direct entry and landing probes of the Soviet Union that the temperature of Venus is within a few degrees of 750 K The surface atmospheric pressure about ninety times that at the surface of the Earth consists primarily of carbon dioxide. This large abundance of carbon dioxide plus the smaller quantities of water vapor that have been detected on Venus, are adequate to heat the surface to the observed temperature via the greenhouse effect."⁴⁶⁷

Sagan is most disingenuous, as will be shown later in our discussion on Oberg and others in this volume, that all the probes to Venus—Venera 9, 10, 11 and 12 and the four Pioneer Venus probes, exhibited measurements that Venus was giving off more heat than could be provided by the Sun. Like those who raised this issue, Sagan's statement is dead wrong. As for his statement that "the smaller quantities of water vapor have been detected on Venus are adequate [with carbon dioxide] to heat the surface to the observed temperature via the greenhouse effect," this too is completely false. Various teams have been looking for adequate water vapor in Venus' atmosphere for years and admit they cannot find what is needed in this respect. R. Cowen writes in *Science News* in 1991, several years after Sagan made this assertion, that a

". . . research team focused on a greenhouse puzzle The absence of water vapor above Venus mystifies scientists because models of the planet's strong greenhouse effect suggests that vapor plays a key role in maintaining the warming. Researchers have . . . looked *below* the cloud deck and down to the surface—and their search has come up dry

"Evidence of a dry Venus may force researchers to consider whether other chemicals could create and sustain the planets greenhouse effect said David Crisp of the Jet Propulsion Laboratory . . . who co-authored the report."⁴⁶⁸

Both the measurements by Venus probes and the measurements of water vapor in Venus' atmosphere contradict Sagan. How can an astronomer be so ignorant of fundamental evidence? One is asked to notice that Sagan's statements on these points are not supported by a single citation to the scientific literature or to anything

⁴⁶⁷SCV, *op. cit.*, pp. 81-82; BB, *op. cit.*, p. 116.

⁴⁶⁸R. Cowen, *Science News*, (September 14, 1991) p. 167.

else? Sagan expected everyone to take his word for the accuracy of these statements, and they did in general when, in fact, there was not one iota of solid truth to his remarks!

Problem IX: The Craters of Venus

While Sagan suggested that Venus "is cratered and, perhaps like the moon, is saturation-cratered, that is, so packed with craters that one crater overlaps the other,"⁴⁶⁹ in our play we showed Sagan claimed that there was a relative "sparseness of craters on Venus."⁴⁷⁰

Like so much of his work, this is more of Sagan's disinformation.

Problem X: The Circularization of the Orbit of Venus and Nongravitational Forces in the Solar System

Sagan argues that Newtonian mechanics will not allow for the circularization of a planet's orbit in a short time from one that is highly elliptical. Here is what Albert Einstein said regarding this problem. He claimed he could explain everything described in Velikovsky's book "on the basis of the accepted celestial mechanics of gravitation and inertia . . . Even the circular orbit of Venus, though it would require a very unusual degree of coincidences."⁴⁷¹

Was Albert Einstein so inferior a scientist to Carl Sagan that he made a claim that categorically ran contrary to the claim made by Sagan? In many places in Velikovskian literature, Einstein's working with Velikovsky's ideas and concepts is remarked upon. The difference between Einstein's attitude and that of Sagan in their analysis of Velikovsky's work is quite pronounced. Why then has Sagan had so much more influence with the press and the public than Einstein? Apparently the press and the public are enamored by the glib statements and glitter of celebrity which Sagan offers as opposed to the long studies of Einstein and the careful analysis he offered which lacks the razzle-dazzle and pizzazz of Sagan. Why Sagan should have greater influence than Einstein with the press and the public proves that public relations fluff carries much more weight than solid thoughtful analysis!

There is a great deal more that can be written about Sagan's largely dishonest comments on Velikovsky. These are contained in my book, *Carl Sagan & Immanuel Velikovsky* (New Falcon Publications) 1995. Those interested in over 400 pages of a most thorough debunking of Sagan's criticism of Velikovsky should read the book. As Robert Anton Wilson stated,

"I could go on and on for hundreds of pages [on Sagan's goofs] but instead I refer you to Ginenthal's book . . . Ginenthal does spend hundreds of pages documenting one fallacy after another—literally dozens and dozens of them—in Sagan's smear campaign against Dr. Velikovsky."⁴⁷²

And I would point out that Robert Anton Wilson is extremely skeptical of Velikovsky's ideas. But he does know dirt when it is being thrown at Velikovsky by Sagan and is ready to admit it. This the press is sadly loath to investigate and present to the public.

⁴⁶⁹*SCV, op. cit.*, p. 84; *BB, op. cit.*, p. 118.

⁴⁷⁰Carl Sagan and Ann Druyan, *Comet, op. cit.*, p. 258.

⁴⁷¹Charles Ginenthal, "Before the Day Breaks—A PERSPECTIVE," *The Velikovskian*, Vol. 1, No. 4, (1993), p. 102.

⁴⁷²Wilson, *op. cit.*, p. 198.

I shall deal with one final comment by Sagan which, I suspect, is one of the main sources of his and many other scientists' resentment of Velikovsky's thesis. Since Robert Anton Wilson said it so much better than I ever could, I will let him explain this point.

"Sagan continually states bluntly and falsely, that Dr. Velikovsky intends his cosmic catastrophe theory to revive the old time religion: 'It is an attempted validation of religion' . . . 'Velikovsky attempts to rescue not only religion but also astrology.' (*Broca's Brain*, p. 126) We can only conclude that Sagan either reads carelessly or engages in deliberate lying. Any close reading of Dr. Velikovsky shows numerous expressions of skepticism about both religion and astrology.

"In addition, Dr. Velikovsky's theory of cometary near collisions offers a naturalistic scientific explanation for many events or alleged events in ancient history which the religious prefer to explain supernaturally as miracles. Nobody who suggests a *natural* explanation for allegedly *supernatural* events offers real support to religion, in either the judgment of the religious themselves or of those of us with agnostic disposition.

"Only Sagan—and a few others, who seem never to have read Velikovsky and obtained their 'knowledge' about his works from Sagan—think of the comet model as 'validating' religion, since Dr. Velikovsky uses a hypothetical comet to replace a hypothetical god in explaining huge reported floods and other catastrophes. Most of us think of Dr. Velikovsky's theory as one which if proven, would knock one more leg from under the edifice of Bible Fundamentalism. Nobody seems likely to worship Dr. Velikovsky's comet, but millions still worship the Bible's god.

"In the 30 years or more that Sagan has engaged in diatribes against Dr. Velikovsky, somebody must have pointed out this fundamental confusion to him—mis-identifying a naturalistic theory with a supernatural theory. Evidently, he has a lot of trouble remembering such corrections. You become a leading **Expert** by acting as if everybody else's opinion deserves no attention"⁴⁷³

Sagan, of course, has never answered the criticisms made by me and many others of his analysis of Velikovsky. To do so would bring public and press attention to the poverty of his science and the depths to which he was ready to descend to destroy Velikovskianism.

As I have shown elsewhere, the aim of the symposium was to educate the public and the press against Velikovskian concepts.

As Broad and Wade show,

"Paul Feyerabend of the University of California, Berkeley . . . not only admits nonrational elements into the scientific [debate] process but sees them as dominant. Science, he says, is an ideology, completely shaped at any moment in time by its historical and cultural context. Scientific disputes are resolved not on their merits but by the theatrical and oratorical skills of their advocates, much as are legal cases

"Feyerabend . . . believes that the distinction commonly made between science and other modes of thought is unjustified, an artificial barrier erected by scientists to set them above their fellow citizens. 'Those who do not like to see the state meddling in scientific matters should remember the sizeable chauvinism of science: for most scientists the slogan "freedom for science" means the freedom to indoctrinate not only those who have joined them, but the rest of society as well.'"⁴⁷⁴

⁴⁷³Wilson, *op. cit.*, pp. 194-195.

⁴⁷⁴Broad and Wade, *Betrayers of the Truth, op. cit.*, pp. 133-134.

Paul Feyerabend, himself, states on this matter,

"Finally, let me repeat that, for me, the chauvinism of science is a much greater problem . . . It may even be one of its major causes. Scientists are not content with running their own playpens in accordance with what they regard as the rules of the scientific method; they want to universalize these rules, they want them to become part of society at large and they use every means at their disposal—argument, propaganda, pressure tactics, intimidation, lobbying—to achieve their aims."⁴⁷⁵

The theatrics and machinations carried out at the AAAS symposium on Velikovsky were only public relations and indoctrination procedures. They were anything but open and honest scientific discourse.

When scientists feel they are on the defensive they act irrationally, as has been amply demonstrated in this volume. The behavior of Sagan, Mulholland, Huber and Storer is the behavior of frightened, angry men seeking victory rather than the behavior of rational men seeking the truth. The whole symposium was "bread and circuses" for the press, and the press reveled in every moment of it. Nowhere will one read a press account that condemns such wantonly deliberate press grandstanding misrepresentation and hype, instead of truly thoughtful and respectful criticism, as that offered by Einstein and a few others. With enough of the press favorable to all this behavior at the AAAS symposium on Velikovsky, the scientists and organizers were able to convince their fellow scientists and the public that what they did was honorable and decent science.

In fact, this book makes clear that the Velikovsky Affair has not ended. The second generation of the scientific establishment has been shown to have used all the same indecent methods employed by the first. They have misrepresented Velikovsky's work and ridiculed it even as they misrepresented it. They have suppressed Velikovsky's supporters' responses to attacks hurled at Velikovsky in the very journals where these assaults were published or promulgated. They have used administrative tactics to assure that evidence relative to the debate was never acknowledged. They have used public relations smear techniques to educate the public. They have failed to acknowledge tests in historical archeology that indicate that this area of established research is in serious error. They have ignored Velikovsky's rebuttals to their attacks and then misrepresented the very nature of these rebuttals. They have used every measure available to them to destroy, by means that are unethical, every vestige of evidence favorable to Velikovsky. Every norm of science they have violated repeatedly.

Over 2,000 years ago, there were other guardians of science and astronomy who wanted to create a stable universe, and thus they adopted an Earth-centered (geocentric) universe. They, too, fought to hold on to that earlier concept of solar system stability against the sun-centered (heliocentric) universe in which the planets moved around the Sun. Arthur Koestler explains why and how they fought against this dangerous concept.

"The earth-centered . . . circular view of the cosmos excluded all progress and all compromise for fear of endangering its main principle, stability. Thus, it could not be admitted that the two inner planets circled the Sun, because once you gave way on this apparently harmless point, the next logical step would be to extend the idea to the outer planets and to the earth itself . . . The frightened mind, always on the defensive, is particularly aware of the dangers of yielding an inch to the devil."⁴⁷⁶

This solar system stability concept is the other dogma that astronomers hold as their main principle, combined with their distrust of ancient legends. If today's scientists surrender once to the notion that even one planet has had an unstable orbit, the next logical step would be to extend the idea to other planets also. Koestler realizes that, "once the earth's stability was shaken, the world would fly to pieces."⁴⁷⁷ So, too, Velikovsky's view

⁴⁷⁵Feyerabend, *Against Method*, *op. cit.*, p. 169.

⁴⁷⁶Arthur Koestler, *The Sleepwalkers*, (New York, 1959), p. 76.

⁴⁷⁷*Ibid.*

that our solar system was recently unstable creates the same kind of dread in the deepest recesses of the minds of modern scientists that the ancients felt. Velikovsky's views are an horrendous reality that goes far beyond any danger ever conceived as solar system history. Velikovsky would say that the defensive behavior of the scientists is proof that, at some level of consciousness, they are "aware of the dangers of yielding an inch to the devil." Lucifer, I hasten to add, is one of the ancient names of Venus! Because of this renewed campaign of defamation, Velikovsky has become a non-person among editors and science writers of newspapers and mass-circulation magazines. Nothing has changed since the start of this smear campaign.

Lastly, I wish to point out that the influence all Velikovsky's critics had was largely based on the fact that these men were all authorities and the press and public gave them and their statements far more credence than the words of Velikovsky. However, it must be stressed that relying so greatly on such authorities is one of the problems related to the Velikovsky affair. This is not a new aspect of it. Scientific authorities have been misrepresenting Velikovsky since the very beginning. Sagan, Mulholland, and Huber's behavior was no different than those that went before. Therefore, the words of Thomas Huxley on authorities is apropos to conclude this essay:

"Authorities' . . . are the curse of science; and do more to interfere with the work of the scientific spirit than all its enemies."⁴⁷⁸

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THE A.A.A.S. AFFAIR FROM TWENTY YEARS AFTER, by Lynn E. Rose

PROLOGUE

"QUOTA PARS OPERIS TANTI NOBIS COMMITTITUR?" (SENECA)

It is now more than twenty years since the American Association for the Advancement of Science held its infamous Symposium on "Velikovsky's Challenge to Science" in San Francisco on February 25, 1974. Lately, in preparing this retrospective treatment of the "A.A.A.S. Affair," as it has come to be called, I find my thoughts continually returning to the above quotation from Seneca ("What part of such a work is committed to us?"). Velikovsky had used that question as his motto in *Worlds in Collision*, pages 3 and 389.

A considerable amount of material concerning the A.A.A.S. Affair has already been printed: *Pensée* IVR VII (edited by Stephen L. Talbott); *The Age of Velikovsky* (C. J. Ransom); *KRONOS* III:2, the *Velikovsky and Establishment Science* issue (edited by Lewis M. Greenberg); *Velikovsky and his Critics* (Shane Mage); *Scientists Confront Velikovsky* (edited by Donald Goldsmith); *KRONOS* IV:2, the *Scientists Confront Scientists Who Confront Velikovsky* issue (edited by Lewis M. Greenberg); *Carl Sagan and Immanuel Velikovsky* (Charles Ginenthal); and the numerous other pieces that have appeared in more sporadic form. Probably the most complete discussion is in *The Sins of the Sons: A Critique of Velikovsky's A.A.A.S. Critics*, by Immanuel Velikovsky and Lynn E. Rose, but that book has not been published yet. Nonetheless, even the amount of A.A.A.S. material already published would be far too massive for anything more than a very cursory reexamination in this one chapter. Thus it seems best to

⁴⁷⁸C. Bibby, *T. H. Huxley: Humanist and Educator*, (New York, 1959), p. 18.

concentrate mostly on matters that have *not* received enough attention before. In fact, there is no real substitute for this extensive array of further reading.

Since what we are concerned with in this chapter is the A.A.A.S. Affair as one instance of the continuing Velikovsky Affair, the focus is not really on Velikovsky himself or on his own most impressive and courageous performance at San Francisco. That is best conveyed by the transcripts anyway. Rather, our focus here must be more on the deeds of his A.A.A.S. critics, both before and after the Symposium, and not exclusively or even primarily on what was actually said at the Symposium itself.

Velikovsky's own Symposium paper has been published at least three times that I know of, and he and numerous others have already published lengthy accounts and

analyses of the discussions at the Symposium, as well as the papers of the various critics—either in their San Francisco versions or in their *Scientists Confront Velikovsky* versions.

Previously, the discussions during the morning and evening sessions of the Symposium have been quoted or summarized by various parties, but the transcripts of those discussions have never been available in their entirety. Those transcripts are newly-published here, at the end of this book. But such transcripts have a tendency to speak for themselves anyway; thus they, too, need not be dealt with in this chapter.

What, then, *should* I be doing in this chapter? *Quota pars* indeed?

As I see it, my most important task is to set the A.A.A.S. Symposium in its proper context, from the perspective of twenty years later. The A.A.A.S. Affair was, after all, a major event in the continuing Velikovsky Affair, and can be at least as instructive as the events of 1950.

THE FATHERS AND THE SONS

But what *was* the relationship between the A.A.A.S. Affair and the events of 1950? In other words, what *was* the relationship between the older generation that was responsible for the events of 1950 and the younger generation of nearly a quarter century later?

J. Derral Mulholland, one of the four anti-Velikovsky panelists at the A.A.A.S. Symposium, suggested that Velikovsky should not lay the sins of the fathers on the sons, that is, that he should not blame the scientists of 1974 for the wrongs done back in 1950.

As I see it, the new generation of bookburners differed from the previous generation of bookburners, not by being guilty of less, but by being guilty of *more*. After all, the sons had had ample opportunity to witness the record of confirmation of Velikovsky's theory and to observe that Velikovsky's work had stood the test of time. Also, the judgments of the sons were more deliberate and considered, in that they were reached over many years; they were not the more-or-less snap judgments that had been made by some of the fathers. Thus the sins of the sons were even *worse* than the sins of the fathers.

This very *worseness* of the behavior of the sons might have been taken as a clear sign that the continuing Velikovsky Affair was getting stronger, not weaker.

Aside from these aspects of worseness, however, there is a noticeable *sameness* to much of the sons' behavior. The continuing Velikovsky Affair, especially in the form of the A.A.A.S. Affair, involved more and more of the *same*: the same kind of insult, the same kind of distortion, the same kind of lie.

There is little relief to this overall sameness: most of the faces are new, but the two-facedness is not; some of the insulting names are new, but the practice of name-calling is not; some of the lies are new, but the practice of lying is not, some of the arguments are new, but the practice of using fallacious and irrelevant arguments is not; some of the articulations of establishment ideology are new, but the ideology itself is not.

How is this overall *sameness* to be reconciled with the *worseness* that was mentioned earlier? If the behavior of the sons is the *same* as the behavior of the fathers, how can it also be *worse* than the behavior of the fathers? There is no inconsistency here. It is the passage of time that is the key. The very *same* overt action would have been *worse* in 1974 than in 1950, precisely because by 1974 the sons had indeed had ample opportunity to reflect upon the sins of their fathers, ample opportunity to recognize the record of confirmations of the Velikovsky theory and the way it had passed the test of time, and ample opportunity to make up their own minds in a deliberate and considered manner, as opposed to the snap judgments and knee-jerk reactions of the fathers.

In spite of these opportunities, the sons continued to behave very much as their fathers before them had behaved. That is why I maintain that the sins of the sons were even *greater* than the sins of the fathers.

THE MAIN TOPICS

But why have there been so *many* of these sins? And why is the Velikovsky Affair still *continuing*, after so many years? In an effort to address these and other such questions, I shall—in the pages that follow—be placing great emphasis on Velikovsky's own view that the Velikovsky Affair should be understood in terms of psychoanalytical *resistance*.

I shall also be dealing with the notable *incompetence* that afflicts so many of Velikovsky's critics, and with some of the possible psychological *differences* between Velikovsky's supporters and Velikovsky's critics.

The *individuals* receiving the most attention here will be Huber, Michelson, and Gingerich—Huber because I am one of those best prepared to answer him (thus he has become my special responsibility), and Michelson and Gingerich because their overall roles have never been examined before.

I then conclude this look back with a brief look toward the future.

* * * * *

PART ONE: THE PSYCHOLOGY

PSYCHOANALYTIC RESISTANCE

Psychoanalysts find that their patients characteristically display very strong *resistance* to any unwelcome truths that the analysts have uncovered about the patient's early traumas. The patients have long since buried their memories of those early traumatic experiences, and now they simply cannot accommodate having them stirred up prematurely.

Thus even if the analysts do come to realize just what the traumatic experiences were (and perhaps after only a few sessions), it is still necessary to wait, sometimes for years, and to try to enable the patients to work toward a recognition of the facts for themselves.

If an analyst does not wait, but reveals the nature of the traumas too soon, and before the patient is ready, the patient will lash out at the analyst in angry denial.

It may be quite evident to the analyst, and perhaps even to others, that such a patient "doth protest too much." But this is rarely evident to the patient. To the patient, the unwelcome revelations are quite inconceivable; they are plainly impossible, and can in no way be true. Yet the angry resistance, being so much out of place, only confirms that those revelations probably *are* true.

THE COLLECTIVE UNCONSCIOUS HUMAN MIND

Velikovsky followed Sigmund Freud in recognizing a collective unconscious human mind, a single repository of buried racial memories of the collective experiences of our remote human ancestors.

(When Freud and Velikovsky use such words as "race" and "racial" in these sorts of contexts, they are not referring to race in the sense of black, white, yellow; rather, they are referring to race in the sense of the *human* race. We are, after all, one species, and the theory of racial memory is to the effect that there is *one* repository of traumatic memories, *one* collective unconscious mind, for the *entire* human race. Its content, which is part of us all, is the heavy and rather encumbering baggage that has been passed down from our ancestors.)

Freud's reluctance to use the phrase "collective unconscious" (see *Moses and Monotheism*, page 132, quoted below) is probably a matter of his not wanting to owe anything, even a phrase, to Alfred Jung. Anyway, Freud was talking about a collective human mind as early as *Totem and Taboo*, which he published in 1912-1913, and that work seems to have inspired Jung to pursue similar investigations of his own. Freud continued to recognize the collective human mind even in his last work, *Moses and Monotheism* (1939).

Since Freud *did* recognize a collective mind, and since he *did* recognize that its content was unconscious, it would seem that his only reason for avoiding any concatenation of the words "collective" and "unconscious" was his distaste for Jung. (Velikovsky followed Freud here, *both* in tending to avoid the Jungian phrase "collective unconscious" *and* in having a strong distaste for Jung.) All that notwithstanding, it is clear that both Freud and Velikovsky are committed to what might most conveniently be called a collective unconscious human mind, whether they choose to phrase it that way or not.

All of the quotations in the next two sections are from *The Standard Edition of the Complete Psychological Works of Sigmund Freud* (hereafter "SE"), edited by James Strachey.

TOTEM AND TABOO

Let us look first at *Totem and Taboo*, SE XIII, pages 157-158:

". . . I have taken as the basis of my whole position the existence of a collective mind, in which mental processes occur just as they do in the mind of an individual. In particular, I have supposed that the sense of guilt for an action has persisted for many thousands of years and has remained operative in generations which can have had no knowledge of that action. I have supposed that an emotional process, such as might have developed in generations of sons who were ill-treated by their father, has extended to new generations which were exempt from such treatment for the very reason that their father had been eliminated."

Also on page 158, a little farther down, we read:

"Without the assumption of a collective mind, which makes it possible to neglect the interruptions of mental acts caused by the extinction of the individual, social psychology in general cannot exist. Unless psychical processes were continued from one generation to another, if each generation were obliged to acquire its attitude to life anew, there would be no progress in this field and next to no development. This gives rise to two further questions: how much can we attribute to psychical continuity in the sequence of generations? and what are the ways and means employed by one generation in order to hand on its mental states to the next one? I shall not pretend that these problems are sufficiently explained or that direct communication and tradition—which are the first things that occur to one—are enough to account for the process. Social psychology shows very little interest, on the whole, in the manner in which the required continuity in the mental life of successive generations is established. A part of the problem seems to be met by the inheritance of psychical dispositions which, however, need to be given some sort of impetus in the life of the individual before they can be roused into actual operation."

MOSES AND MONOTHEISM

More than a quarter of a century later, Freud's view had little changed (*Moses and Monotheism*, SE XXIII, page 80):

"The reader is now invited to take the step of supposing that something occurred in the life of the human species similar to what occurs in the life of individuals: of supposing, that is, that here too events occurred of a sexually aggressive nature, which left behind them permanent consequences but were for the most part fended off and forgotten, and which after a long latency came into effect and created phenomena similar to symptoms in their structure and purpose."

See also pages 99-100:

". . . I have behaved for a long time as though the inheritance of memory-traces of the experience of our ancestors, independently of direct communication and of the influence of education by the setting of an example, were established beyond question. When I spoke of the survival of a tradition among a people or of the formation of a people's character, I had mostly in mind an inherited tradition of this kind and not one transmitted by communication. Or at least I made no distinction between the two and was not clearly aware of my audacity in neglecting to do so. My position, no doubt, is made more difficult by the present attitude of biological science, which refuses to hear of the inheritance of acquired characters by succeeding generations. I must, however, in all modesty confess that, nevertheless I cannot do without this factor in biological evolution."

See also on page 100, a little farther down:

"If we assume the survival of these memory-traces in the archaic heritage, we have bridged the gulf between individual and group psychology: we can deal with peoples as we do with an individual neurotic. Granted that at the time we have no stronger evidence for the presence of memory-traces in the archaic heritage than the residual phenomena of the work of analysis which call for a phylogenetic derivation, yet this evidence seems to us strong enough to postulate that such is the fact."

Finally, see *Moses and Monotheism*, SE XXIII, page 132:

"It is not easy for us to carry over the concepts of individual psychology into group psychology; and I do not think we gain anything by introducing the concept of a 'collective' unconscious. The content of the unconscious, indeed, is in any case a collective, universal property of mankind. For the moment, then, we will make shift with the use of analogies. The processes in the life of peoples which we are studying here are very similar to those familiar to us in psychopathology, but nevertheless not quite the same. We must finally make up our minds to adopt the hypothesis that the psychical precipitates of the primeval period became inherited property which, in each fresh generation, called not for acquisition but only for awakening."

Freud saw those traumatic early racial experiences in terms of cave men killing their fathers and mating with their mothers. To Velikovsky, the Oedipal insights of Freud were of great importance in treating the individual patient, but Velikovsky did not believe that the content of the collective unconscious human mind was these forgotten Oedipal behaviors from the days of the cave. Rather, what the collective unconscious human mind actually held, for Velikovsky, was our repressed collective and racial memories of interplanetary near-collisions and the resulting global catastrophes that have occurred within historical times.

It is arguable that Velikovsky went well beyond Freud in this matter. For Velikovsky was much clearer than Freud both about what happened in our racial past, and about how that affects what is going on in our present day. Velikovsky's views are elaborated in considerable detail in *Mankind in Amnesia* (1982).

THE HEALER'S PARADOX

Anyone who confronts the human race with the fact of interplanetary near-collisions and the resulting global catastrophes that occurred within historical times can expect a collective resistance, a lashing out, an angry denial, precisely the same as that displayed by the individual patients in psychoanalysis, when they are finally confronted with what they have so long kept buried.

Thus Velikovsky found himself in a very peculiar position.

On the one hand, as a historian, he felt obliged to report the interplanetary near-collisions and the resulting global catastrophes that he found to have occurred within historical times.

On the other hand, as a psychoanalytically-oriented healer, he knew full well that it is undesirable to confront the individual patients too soon with traumatic experiences from those patients' early lives. Rather, the patients must be carefully guided, even enticed, through sometimes very lengthy processes of preparation, and through long journeys toward self-awareness—until the patients themselves are ready to recover those long-buried memories. This journey *is* the psychoanalytic process. It would seem that such a journey is just as important for the species as for the individual patients.

"PSYCHOANALYSIS" FROM AFAR

Velikovsky knew very well that he could not psychoanalyze the human race, at least not in the sense that he could psychoanalyze an individual patient—that is, with the patient's participation and involvement. The only sense in which Velikovsky could psychoanalyze the human race was the very loose sense in which one "psychoanalyzes" someone from afar, without the permission or cooperation or involvement of the subject. In this way, Freud might be said to have "psychoanalyzed" Leonardo da Vinci, and Velikovsky might be said to have "psychoanalyzed" Leo Tolstoi—or even to have "psychoanalyzed" Sigmund Freud himself!

This "psychoanalysis" from afar can be a valuable and informative procedure, but it is not the real thing. It does not do anything at all with respect to the subjects. That is, the analysts and their readers may be enlightened and thereby benefitted as a result of this exercise, but the subjects are not altered at all. Many of those subjects are dead! But even the "psychoanalysis" from afar of some living person cannot be expected to have altered that person's psychic health.

Most importantly, "psychoanalysis" from afar does not deal at all with the problem of psychoanalytic resistance.

IN LIEU OF THE COUCH

Velikovsky put his dilemma rather literally: "You cannot put the human race on the couch."

He knew very well that he was violating the time-tested techniques of his profession in laying unwanted truths on subjects who were unprepared to receive them.

So far the choice might seem to be in favor of inaction. Why, then, did Velikovsky choose to do something that he *knew* was unlikely to work?

For one thing, the historian and scientist must report what is found, regardless of the consequences, and regardless of anyone's readiness to hear.

For another thing, the dangers of inaction were great. Velikovsky was far more worried about Freudian repetition-compulsion than about Freudian resistance.

REPETITION COMPULSION

Traumatic experiences in early life, with oneself as the passive victim of outside forces, are duly suppressed and buried in the unconscious. There they lie hidden, only to manifest themselves much later in the compulsion to repeat or to relive or to reenact those traumatic experiences, but this time with oneself as the active agent. An extreme case is the molested child who grows into a child-molester.

This repetition compulsion can also occur in a collective context. We as a species were long ago subjected to interplanetary near-collisions and global destructions in which we were but passive victims. Now there is an unconscious need or compulsion to repeat and relive and reenact those sorts of experiences, but this time with ourselves as the potent and powerful agents, rather than merely as the passive and helpless and impotent victims.

Thus Velikovsky can explain the need to pollute and ravage and befoul our planetary nest, to blast and devastate the very globe on which we live. These irrational compulsions are driving our actions even today—and we now have the technological capacity to carry out and to satisfy our unconscious needs, even if the satisfaction of those needs would directly lead to the extinction of the human race.

Velikovsky took very seriously this idea that the human species was blindly self-destructive—and was now in possession of the technological *means* of self-destruction.

That is why Velikovsky thought it so urgent and necessary that he proceed with the writing and publication of *Mankind in Amnesia*, even though this might only aggravate the resistance that was already so widespread.

PSYCHOANALYTIC RESISTANCE AND THE VELIKOVSKY AFFAIR

As a Freudian, Velikovsky saw psychoanalytic resistance as *the* mechanism underlying the Velikovsky Affair. People lash out at anyone who would confront them with forgotten and buried memories of the repeated catastrophes that were experienced by our ancestors.

The logic of this is very simple. The Velikovsky theory *implies* the Velikovsky Affair. If the Velikovsky theory is true, then there had to have been a Velikovsky Affair. Thus the occurrence of the latter is one confirmation of the former. Conversely, if there had been no Velikovsky Affair, then it would follow that the Velikovsky theory was false.

I take a firm stance on all this: until I see solid evidence to the contrary, I shall continue to insist that resistance is the *only* effective explanation of the Velikovsky Affair. In this I am but following Velikovsky's own lead. "McCarthyism" (which McCarthy himself seems to have joined only after it had already started), the Cold War, and whatever else might be cited as causes of the Velikovsky Affair should be rejected as adequate overall explanatory factors. It is not that they were not *present* during the Velikovsky Affair. It is just that they do not *explain* the Velikovsky Affair.

Somewhere, sometime, there may have been other factors that led to certain kinds of hostility toward Velikovsky. Examples are legion:

In his capacity as the author of *Worlds in Collision* and other books, Velikovsky did occasionally receive anti-Semitic mail, and that alone makes it clear that antisemitism was responsible for *some* of the negative judgments about Velikovsky and his work.

Velikovsky's Russian background and the fact that he lived (albeit briefly) under a communist government may also have aroused suspicion, in at least *some* quarters, despite the fact that he and his parents suffered greatly under the Lenin regime and soon had to flee to Palestine.

Some of Harlow Shapley's need to find somebody to kick around may have resulted from the fact that he himself was being kicked around by the House Un-American Activities Committee.

Some of Cecilia Payne-Gaposchkin's venom may have resulted from bitterness over the fact that her own career was entirely dependent upon her playing up to Shapley and his agenda; even her attack on Velikovsky came about because of direct orders from the little Bonaparte.

But these droplets of hostility and irrationality are not enough to explain the unprecedented *torrent* that is the Velikovsky Affair. That torrent seems to flow from the very depths of the collective unconscious human mind, and manifests itself as an angry repudiation of anyone who would dare confront us with our repressed human past. It also manifests itself in firings, boycotts, censorship, bookburnings, threats, blackmail, and the like, as well as in vile rhetoric of all sorts.

AWAY FROM THE STORM

Mrs. Velikovsky once said to me that for many years after 1950 she and her husband were busy with their lives, their children, their grandchildren, their many interests, and that they paid little heed either to the critics or to the controversy. It was peripheral, and they were occupied with other things. She had her music and her sculpture, and he had his research and his writing.

Thus Velikovsky continued with his work, aided continuously by his wife, and their lives went on. Days or weeks would go by, with no mention or even thought of "the Velikovsky Affair." Velikovsky was content to finish his research, and to let his opponents think what they might. He understood them, and their blind anger, but they were not on *his* couch, and there was nothing that he could do for them.

BACK TO THE FRAY

Somehow, Velikovsky's expectations gradually changed. Slowly, he re-entered the fray. I think that there were good reasons for this, or at least reasons that looked good at the time.

The birth of the Space Age had caught the West by surprise. Suddenly something called Sputnik was in orbit around Earth. It was quickly followed by other probes. (Initially, all of these probes, whether manned or unmanned, were Russian. As the years went on, however, they came increasingly to be American.)

Much new information could be expected concerning the solar system and its members. The Space Age was dawning.

Velikovsky began to keep one eye on space developments. Soon the flood of new data occupied nearly all of his time.

Velikovsky had published *Worlds in Collision* in 1950, *Ages in Chaos* in 1952, *Earth in Upheaval* in 1955, and *Oedipus and Akhnaton* in 1960.

Other books were in the pipeline, and would soon have followed: *The Dark Age of Greece*; the succeeding volumes of the *Ages in Chaos* series, including *The Assyrian Conquest*, *Ramses II and His Time*, and *Peoples of the Sea*; *In the Beginning* (the "prequel" to *Worlds in Collision*); *Before the Day Breaks* (the Einstein book); *The Test of Time*; *Mankind in Amnesia*, *Stargazers and Gravediggers*; and Velikovsky's autobiography, *Days and Years*.

All of these books were near completion. But every one of them was put on hold while Velikovsky sought to take full advantage of the late-breaking news from space. Probably the *Mankind in Amnesia* delay was what bothered him the most, but when Velikovsky did finally publish another book, it was *Peoples of the Sea* (1977). Nor was the Space Age the only distraction during those 17 years. Velikovsky had witnessed a long series of events that had led him to believe that his works might be much nearer to winning acceptance, or at least much nearer to winning a hearing. He felt that he could not afford to stand aside from those unfolding events, even though they consumed a great deal of his time.

SIGNS OF CHANGE?

Among the apparent signs of a changing intellectual atmosphere were the following:

Sputnik, in 1957.

The Bargmann-Motz letter, *Science*, December 21, 1962.

The Venus probes, especially the Mariner in 1962.

American Behavioral Scientist's Velikovsky issue, September, 1963.

The Velikovsky Symposium at Brown University, in March of 1965.

The Velikovsky Affair, published in 1966.

Yale Scientific Magazine's Velikovsky issue, April, 1967.

The New York Times of July 21, 1969, with its "MEN WALK ON MOON" headline. (This was indeed a breakthrough, in that the Early City Edition actually included an article by Velikovsky. But his article was mysteriously pulled from the Late City Edition. That could have been taken as a clear warning that the Velikovsky Affair was not over. It may also be noteworthy that the microfilm version of *The New York Times* uses the Late City Edition. With the advantages of hindsight, we can identify many such warning signs from those years.)

The Switzerland conference on Velikovsky, in the summer of 1971.

The CBC activities in Toronto, all concerning Velikovsky, October 13-22, 1971.

Lecture by Velikovsky at Harvard University, February 17, 1972.

Other lectures by Velikovsky, such as at the State University of New York at Buffalo, March 20, and at McMaster University, March 22, all in one trip, 1972.

Pensée, started in May, 1972.

Lectures by Velikovsky at the NASA Ames Research Center, August 14, 1972.

Conference at Lewis and Clark College, in Portland, Oregon, August 16-19, 1972.

Lectures by Velikovsky at the IBM Research Center, October 15-16, 1973.

Lectures by Velikovsky in the Carolinas: University of North Carolina (Chapel Hill), December 4; University of North Carolina (Charlotte), December 6; and Furman University, Greenville, South Carolina, December 7, 1973.

Lecture by Velikovsky at the NASA Langley Research Center, December 10, 1973.

And then there were the *five* major conferences in 1974 that held Symposia on Velikovsky's work:

American Association for the Advancement of Science, Symposium on "Velikovsky's Challenge to Science," San Francisco, February 25, 1974.

University of Lethbridge, May 9-10, 1974, Symposium on "Recollections of a Fallen Sky," followed by Velikovsky's receiving an honorary degree, Doctor of Arts and Science, on May 11, 1974.

McMaster University, Symposium on "Velikovsky and the Recent History of the Solar System," in Hamilton, Ontario, June 16-19, 1974.

Duquesne University History Forum, in Pittsburgh, Pennsylvania, on October 30, 1974.

The Philosophy of Science Association, meeting at Notre Dame University, in South Bend, Indiana, on November 2, 1974. (This Velikovsky session came about because of the remarks of Antoinette M. Paterson during the business meeting of an earlier conference, when she had urged her colleagues to deal with the work of Velikovsky.)

THE END OF THE AFFAIR?

This flurry of activities in the 1960's and 1970's may have caused Velikovsky to change his expectations.

He himself had predicted the Velikovsky Affair. (In Toronto, on the very day when Toni Paterson and I had first met the Velikovskys, Velikovsky was asked, by one of the guests attending the reception at the home of Robert Zend, "After you first saw your overall theory, what was your very next thought?" His answer was immediate: "That they will not accept it." As a psychoanalyst, he seems to have understood this at once.)

But how long was the Velikovsky Affair really supposed to last?

Until the accumulation of evidence forces people to look? Probably not. *Nothing* forces people to look at what they will not see.

Until the old critics died off? But hadn't they? When men first stepped on the Moon, Velikovsky was in his seventy-fifth year. He had seen his old antagonists dropping like flies.

Planck says that a new theory will not be accepted until the defenders of the old theory die off. But Shapley and others *did* die. The ranks of the suppressors from 1950 were growing very thin.

Dead or not, the old guard were threatened by a new generation that professed to think for itself and to choose for itself. Or so they said. Politically, socially, intellectually, this new generation would not be bound by the hypocrisy and folly of past generations. Or so they said.

THE INERTIA OF THEORY

The Velikovsky theory does indeed imply that there will be a Velikovsky Affair. But the Velikovsky theory does not imply how long the Velikovsky Affair will continue.

I still do not know how long it will continue. Nor does anyone else.

Velikovsky seems to have been right the first time. It would be a long struggle, and the end was not in sight. Perhaps he should *not* have changed his mind about this.

No one foresaw that the age of space exploration would go into hiatus almost as soon as it had begun. The space program was largely a spectacle, an entertainment, with gladiators and daredevils, to justify science budgets. Even the occasional casualties grew boring.

No one foresaw that the surviving flower children would get haircuts, disappear into suburbia, take up bathing (in Jaccuzzis), claim their portions of the pie, vote for Ronald Reagan, and make especially sure that *their* children never strayed from the straight and narrow.

Most importantly, no one foresaw that the resistance to catastrophist historiography would grow more entrenched with the passing years. But the A.A.A.S. Affair showed that things were as vicious as ever. In fact, the sins of the sons were even *worse* than the sins of the fathers. As we have seen, the sons had had the opportunity to witness the long record of confirmations of Velikovsky's work, and to see that that work had survived the test of time. The choices of the sons were deliberate, in clear contradistinction to the *snap* judgments that their fathers had made in 1950. The sons knew very well that their fathers had made fools of themselves in 1950; nonetheless, in spite of everything that had happened in the intervening years, the sons would not budge. They adhered to their dogmas with intense zeal, no matter what.

Theories seems to obey a set of physical laws all their own. Even if they are subjected to great forces and impacts, they may remain where they are. Knock all of the props from under them, and they may not fall. They are like Magritte's "Castle in the Pyrenees"—in which a huge rock is suspended in space over a seashore: atop the rock there is a stone castle, with foundations that *appear* to be as solid as any that ever could be.

THE UNFEELING

There are many people who feel *nothing* in the way of resistance when they are confronted with the idea that there were interplanetary near-collisions within historical times, resulting in global catastrophes. They may be skeptical, and they may want to see some evidence. But then they *look* at the evidence. They do *not* go into a rage, or froth at the mouth, or burn books, or fire anybody, or boycott publishers.

It is not a matter of what one has studied. Gray headed scholars in all fields have run into Velikovsky's theories *without* going into a rage; teenagers who have studied very little of anything have blown their tops. And vice versa.

How do people who are open-minded toward Velikovsky differ from those who are the instigators and perpetrators of the Velikovsky Affair? And how do Velikovsky's supporters differ from those who are his most rabid opponents?

Velikovsky often distinguished those who supported him from those who opposed him by saying that the former had read his works and the latter had not. That is almost always the case, but we need to ask questions that are much more basic. *Why* do the former read him and the latter not? *Why* do some display symptoms of psychoanalytic resistance and others not? *Why* do some go into a rage and others not? Those are the questions that I seek to answer.

ASIMOV

I used to think that Velikovsky's supporters, including myself, were somehow superior to those others, or had some special advantage over them.

We were prepared to look at the evidence, calmly, and without getting excited; the critics were obviously *lacking* in something that we had.

Lately, I have come to wonder about this. Why is it that an intelligent and talented person like Isaac Asimov (in no way a practicing scientist, but an intelligent and talented person, nonetheless) could understand so much and still not be capable of looking at Velikovsky's evidence?

Asimov constitutes a further paradox in that his non-fiction was strictly uniformitarian and orthodox, while his fiction could be quite otherwise. In his non-fiction, Asimov never deviated from what he was taught. That bears upon my assertion that he did nothing at all as a scientist. He questioned nothing, discovered nothing, proved nothing, and thus in the end contributed nothing. He merely explained the established orthodoxies to the unwashed multitudes, in volume after volume.

But in his fiction, he could explore forbidden subjects: astronomical catastrophes, global cataclysms, racial amnesia, collective intelligences, psyches with only tenuous physical bases, telepathy, horrors in the sky, darkness without precedent, frightful stars, crippling and archaic guilt, civilizations destroyed, and all of humankind lost in amnesia. Any of that sound familiar? See the discussions of *Pebble in the Sky* and of "Nightfall" in Lewis M. Greenberg's "Phobia, Amnesia, and the Psyche," *KRONOS* I:1 (Spring, 1975), pages 21-26. For further illustrations, see Asimov's *The Gods Themselves* (1972).

It is as if Asimov were actually in tune with the collective unconscious human mind, and able to draw upon it in his fiction. From his fiction, at least, it would seem that he was *much* more sensitive to the collective unconscious human mind than most people are.

SENSITIVE SOULS?

Perhaps Velikovsky's critics, or at least many of them (I cannot believe that the opportunistic Carl Sagan is sensitive to much of anything), are more sensitive to the content of the collective unconscious human mind, and thus have a greater need to keep it all buried. They are frightened by their suppressed racial memories, and in a rage at any who would uncover those past traumas.

HERE AND NOW

Perhaps Velikovsky's supporters are able to look at his theories in terms of the evidence because they are essentially *insensitive* to the human past, and are rather poor conduits of our collective heritage. Perhaps they are over-particularized, though I would be hard pressed to define that term, except to say that the collective element in them seems to have become quite weak. They do not associate *themselves* with humanity's past. *Their* selves are *here* and *now*. I of course do not mean to suggest that they are *totally* insensitive. For they are not necessarily insensitive in *other* ways, just insensitive with respect to archaic memories of past traumas. Nor do I mean that they have no creativity or imagination or courage. Many of them do have all of that—and more. But they also have a certain ruthlessness about them when it comes to looking at the evidence of our catastrophic past. They do want to *know*, but they are quite unconcerned about what they might find. They could care less.

IMMUNE TO RESISTANCE?

Velikovsky himself, for example, was a warm, feeling person, who could be quite emotional at times. In his lectures, his conversations, and his writings, he displayed much enthusiasm and even lyricism regarding the

subjects at hand, including ancient catastrophes. (After all, he *was* a poet, among other things.) But he could contemplate the evidence of our catastrophic past without feeling personally involved at all. The collective in him seemed to be quite attenuated. Or perhaps the very intensity of his individual personality kept the racial memories in check. In any case, he did not seem to *feel* the catastrophes. He followed the lines of evidence and argument in an orderly, rational manner, and was *not* swayed by emotion in the way that his critics are. (In another chapter of this book we shall have occasion to refer once again to the working relationship between emotion and reason.)

What I am suggesting is that Velikovsky and his supporters can *discover* and *understand* the past, but that they seem to have no personal or emotional involvement with the events in question: *they* were not there. They do not *feel* the Great Fear or the Great Terror. Many of them do not even *identify* with the human species. Thus *they* do not react in anger when confronted with *humanity's* catastrophic past. They seem to have a kind of *immunity* to the resistance that cripples the critics.

VELIKOVSKY NOT A CATASTROPHIST?

I once told Velikovsky that in a sense he was *not* a catastrophist—and he did not disagree. For he certainly did not start out to be a catastrophist. And he *never* treated catastrophism as a methodological given. Rather, it was simply that the evidence *led* him to catastrophist conclusions. Until then, he had held no brief at all for catastrophism. (Owen Gingerich, on the other hand, has uniformitarianism as one of his fundamental beliefs: it is "essential" to everything that he does; he brings it with him from the start.)

HIGHER FORMS?

Though I have been presenting Velikovsky and his supporters as lacking some of the usual linkage with the collective unconscious human mind, this may or may not be a mark of inferiority. Velikovsky even hints that it is a mark of "development" or of some "higher" level that has been attained. See *Mankind in Amnesia*, page 15, where Velikovsky is describing his 1931 paper, "On the Physical Existence of the World of Thought":

"I also claimed the existence of a collective mind in the early stages of the development of the species. Individualization accompanies the evolution from lower to higher forms . . ."

He then *quotes* from that paper of nearly half a century earlier:

". . . the autonomy of the mental domains of separate individuals must have developed as a more complicated and higher state in the origin of the species. In our concept telepathy is an archaic form of thought-transmission. The more a species is developed, the more is the single creature separated as a thinking ego from the world around it."

Velikovsky's remarks were not written with himself and his supporters in mind. But I am suggesting that they may be *applicable* to Velikovsky and his supporters, nonetheless. There seems to be *something* that distinguishes those who go into an indignant rage at the very thought of interplanetary near-collisions within historical times from those who could care less.

But I am not at all convinced that those who are cut off emotionally from our catastrophic past are in any way "higher forms" or more "developed." It might just as well be that they are stunted or even retarded. In any case, I still think that they are *missing* something that could eventually prove to be as precious as it is dangerous: our collective heritage.

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PART TWO: PLANS, PLOTS, AND PROMISES

BUT IT WAS ROBERTS!

The idea of having the American Association for Advancement of Science sponsor a Symposium on the works of Velikovsky was first recommended by Walter Orr Roberts. This was on July 18, 1972, in a letter addressed to Stephen L. Talbott, editor of *Pensée*.

That plain fact has been distorted by the likes of King and Goldsmith. They prefer to give the credit to Sagan! (See King's remarks at the end of the morning session, and Goldsmith's claims in *Scientists Confront Velikovsky*, page 22-23.)

Sagan got into the act only *after* Roberts had already made the recommendation. The correspondence of Roberts, Walter Berl, Talbott, and Sagan makes this unmistakably clear. Sagan heard about Roberts' recommendation from Talbott, and subsequently *supported* it. Sagan even used that very word—"supported"—in reference to his own role in the matter. Roberts himself later spoke of Sagan's "endorsing" and "seconding" Roberts' own recommendation.

This matter is seemingly unimportant. I stress it only because I have learned that when people like King and Goldsmith spread falsehoods, there is probably some devious purpose to it. I am not entirely sure what their purpose was here. (To make Sagan look like less of a villain? To conceal the fact that they had feared that people like Roberts might set up a *fair* Symposium, and that they had wanted to get the project out of his hands and into their own?) Perhaps we can frustrate that purpose, whatever it was, by emphasizing that it was *Roberts* and no one else who made the recommendation.

TWO PROPOSALS

On June 1, 1973, *Science* asked for specific Symposium proposals for the Annual Meeting of the A.A.A.S. that was being planned for February of 1974 in San Francisco.

On June 11, 1973, C. J. Ransom submitted a proposal to the A.A.A.S. that a Velikovsky Symposium be held on the topic of "Venus—A Youthful Planet," with Ransom himself as the organizer. Ransom, a Ph.D. in plasma physics, and a long-time student of the work of Velikovsky, was very well-qualified for this role. He also suggested several speakers who might be asked to participate in such a Symposium.

Nonetheless, Ransom's proposal was rejected, without explanation, on July 9, 1973. A very similar Symposium, but offered by Ivan King, was approved. The main difference, at that point, was that it would be organized by various critics of Velikovsky (Ivan King, Owen Gingerich, Donald Goldsmith), rather than by a supporter of Velikovsky. As time went on, that one difference degenerated into further such differences, all tilted to the advantage of the organizers and to the disadvantage of Velikovsky and his supporters.

KING

Almost immediately, King began discussing Symposium arrangements with Velikovsky. It was promised that the organizers would be strictly impartial, that the proceedings would be given wide publicity, that there would be six panelists, three anti-Velikovsky and three pro-Velikovsky, and that Velikovsky, whose own work was at issue and who was himself the subject of the Symposium, would be given an amount of time—both for the presentation of his paper and for the answering of his opponents—that would be "in clear excess of the time planned for the other participants." Velikovsky told King that he wanted C. J. Ransom and me as the other two members, besides himself, on the pro-Velikovsky side of the panel.

IF

If all of those promises had been kept, there might have been some element of fairness to the entire affair. But fairness was never wanted. Every opportunity to introduce unfairness was taken, and any part of the playing field that was seen to be approximately level was subjected to seismic tilts, always in the critics' own favor and always to Velikovsky's disadvantage.

BROKEN PROMISES

Donald Goldsmith was also brought into the act. Goldsmith helped with the Symposium planning, chaired the evening session, and later arranged and edited the book that became *Scientists Confront Velikovsky*. His primary responsibilities were obviously those of front-man and gopher. In back of the puppet, pulling the strings, was Gingerich.

All of the earlier promises were abandoned.

THE NEW RULES

Both Ransom and I were rejected as panelists, Ransom because he was at that point employed as a research physicist in industry, and was not currently an academician, and I because I was not a physical scientist.

These criteria were obviously *ad hoc*, aimed directly at us. The original criteria were simply that there should be two pro-Velikovsky panelists, familiar with Velikovsky's work. It was even said that one of them might be from sociology or history of science or some such area as that. The word "academic" was never introduced until it was convenient to use it to disqualify Ransom. Similarly, the "physical science" requirement was never mentioned until that became convenient as a means of disqualifying me.

Meanwhile, Carl Sagan and J. Derral Mulholland had been appointed to the panel, along with Velikovsky. Goldsmith's stated formula for picking the rest of the panel was that "one of these should be a person well acquainted with your views" and "a person with good academic standing in physical science." Notice that it is now singular, rather than plural: he wants at most *one* pro-Velikovsky panelist, not two.

These new criteria were used only for the purpose of eliminating Ransom and me. They were *not* invoked when it came to selecting the *additional* anti-Velikovsky panelists, Norman W. Storer and Peter J. Huber. Storer was not "in physical science"; neither was Huber. Storer was a sociologist, quite unfamiliar with Velikovsky's work. Huber was a statistician, and a self-styled "hobby-Assyriologist," likewise quite unfamiliar with Velikovsky's work. The criteria that had eliminated Ransom and me would clearly have eliminated both of them. They were deemed entirely acceptable, however, because they were anti-Velikovsky. (That their ignorance of Velikovsky's work did not prevent them from opposing it is of course nothing very surprising.)

The organizers also dropped the promised extra time for Velikovsky. Instead, the phrase "equal time" now began to be used. That might not have been too bad, if there had been equal numbers of panelists on the two sides. But with Huber and Storer now appointed to the panel, there were *four* anti-Velikovsky speakers.

(Storer *claimed* to be neutral, but that was of course a lie. As Velikovsky pointed out later, Storer's idea of neutrality was to divide the blame equally between the suppressors and the victim of suppression. He spread many falsehoods, including his now-notorious remark that: "No, I don't think that the panel has been set up. It's not rigged." He showed his true colors once more when he wrote to Professor Sidney Wilhelm that Velikovsky was "quite out of his tree"!)

Storer was something of a cream-puff, to be sure, but he *was* anti-Velikovsky and he *did* manage to say a number of things that needed to be answered. Almost anybody could have handled the job of answering him, but in order to answer all of his charges it would of course be necessary to *speak* to each of them, point by tedious point. If there was no time for answering him, then even his incompetent distortions and falsehoods, no matter how easy to

answer, would become damaging—if only because they had *not* been answered. Time is indeed of the essence in a situation like this; after all, even an easy-out has to be pitched to.

UNRIGGED AND EQUAL

In any case, there were now four anti-Velikovsky panelists, and still only Velikovsky himself on the pro-Velikovsky side.

The "equal time" provisions were soon being interpreted as equal time not for the two sides, but for each individual panelist. It was under *those* outrageous circumstances that Storer had had the gall to say: "No, I don't think that the panel has been set up. It's not rigged." That is a recurring theme from the organizers and the participants: four to one, with equal time for each of the five, and with a hostile chair, is as fair as fair could be!

TO ANSWER A CRITIC

Velikovsky often said that to answer a critic, you must first report what the critic said, then state what the truth is, then explain what the reasons are for that being the truth, and then show how the truth clashes with what the critic said, which usually means that you have to go back and *repeat* what the critic had said. All this takes considerably more time than was needed by the critic in the first place.

At the A.A.A.S. Symposium, Velikovsky was expected to answer *four* such critics, plus questions from audience. And there was to be "equal time" for *each* of the five! That, of course, was exactly how the organizers wanted it.

IMPARTIAL?

King's treacherous duplicity is best illustrated by his own statement regarding the planned Symposium; this was submitted to *Pensée* on November 28, 1973, and was intended for publication prior to the Symposium.

". . . the physical scientist . . . regards the present challenge as of too little weight to take seriously. It is a question of balance. In order to preserve an interpretation of ancient history that is denied even by the great majority of experts in that field, Velikovsky would have us set aside the results of three centuries of progress in physical science.

"What disturbs the scientists is the persistence of these views, in spite of all the efforts that scientists have spent on educating the public. It is in this context that the AAAS undertakes the Velikovsky symposium. This is not a debate on the correctness of Velikovsky's view of the planetary system; none of us in the scientific community believes that such a debate would be remotely justified at a serious scientific meeting."

Obviously, King was under severe pressure to take a hard line on all this. That pressure must have materialized *after* all of the fair and sensible arrangements that King had been willing to make when he was dealing with Velikovsky some three months earlier.

On January 14, 1974, I wrote to Talbott concerning this matter:

"It is rather awkward to bring the originator of a theory to a scientific gathering, and then let it be known that the theory is not under discussion, but only the state of affairs that has led to the growth and spread of the theory. Velikovsky's presence at the AAAS is not being regarded as analogous to the scientist's presence in the laboratory; it is being treated as analogous to the virus' presence in the laboratory.

"This would be an intolerable situation for most people, but I think that Velikovsky can handle it. His understanding both of himself and of his opponents, his ability to make his case to the audience, and his unequalled grasp of the facts and of the issues, should enable him to make a good showing, even in the face of a hostile chair and a predominately hostile panel of speakers. My main worry is that the schedule may be deliberately (consciously or unconsciously) rigged so that Velikovsky is not given time or opportunity to answer. I fear that he might be given five minutes to answer hundreds of errors. Consider the Sagan tape: Sagan manages to tell three or four lies (more charitably, makes three or four errors) per minute, for a grand total of several score. You couldn't even *list* them in five minutes, let alone refute them. So I worry that Velikovsky may not be given enough time to defend himself against all the things that the other speakers may say."

(For a predictably rather *lengthy* refutation of that Sagan tape, see my "The Cornell Lecture: Sagan on a Wednesday," *The Velikovskian* I:3 (1993), pages 101-114.)

SOME TERRIFYING THOUGHTS

What if the Symposium had been *fairly* arranged? By *impartial* organizers?

What if the Symposium had been widely publicized as a *serious* examination of the Velikovsky theory?

What if the moderator had been someone *decent* and *honest*? Like Walter Orr Roberts?

What if the two sides had had *equal* numbers of representatives? Like Mulholland, Sagan, and Huber for the anti-Velikovsky side, and Velikovsky, Ransom, and Rose for the pro-Velikovsky side?

What if the promise of additional *time* for Velikovsky's presentation and for his answers to criticisms had actually been kept?

These must have been terrifying prospects for the organizers.

THE TEAM THAT WASN'T

This is not a time for false modesty: Our team could have covered just about all bases. With our diverse backgrounds and experience—personal, avocational, educational, and professional—the team of Velikovsky, Ransom, and Rose would have been formidable indeed. There was very little in the way of relevant points or areas or subjects that could not have been handled by one or another of us.

Velikovsky, Ransom, and I were well prepared to discuss the issues at hand, but Mulholland, Sagan, and Huber clearly knew very little about the Velikovsky theory, and would have had to do a great deal of cramming just to keep us in sight.

Between the two of us, Ransom and I would have rather nicely supplemented Velikovsky's own vast knowledge. When Velikovsky recommended Ransom and me, he had even elaborated on the reasons: Ransom was a Ph.D. in physics now doing plasma research; he had also worked with L. H. Hoffee on the problems of orbital changes in historical times. I was a Ph.D. in philosophy who had taught both the history of science and the philosophy of science, including logic and methodology, and who had earlier majored in ancient history and

classical languages; I had also worked with Raymond C. Vaughan both on the sequence of planetary orbits in historical times and on the Babylonian observations of Venus, the very subject that Peter Huber would be treating at the Symposium.

Such a *troika* would have done well. The organizers could not afford to let that happen. They *had* to rig the Symposium, in every way that they could. Otherwise, Velikovsky might have made them look even more foolish than he did.

AGE

Another consideration on the mind of the organizers was age. With Velikovsky as our team captain, neither Ransom nor I would have expected to monopolize the discussion. But we would have carried part of the load. We would have handled some of the objections, and answered some of the questions. Between us, we would have been able to deal with at least some areas that might have been less familiar to Velikovsky. He acquitted himself extremely well, of course, but our participation would have helped, if only by easing the burden.

This is one aspect of our involvement that has not been much discussed, but it must have been *very* important in the minds of the organizers. At the time of the Symposium, Velikovsky was already in his seventy-ninth year. He was still vigorous and sharp, with more energy than most of those who were only half his age. But this Symposium was long, amounting to something like seven hours altogether. It was well after the 1:00 p.m. deadline when the morning session finally stopped, and the evening session involved several more hours of strenuous activity. Anyone who has taught or lectured knows that it can be very exhausting indeed. All of the organizers had that knowledge, too, and they did everything they could to exploit it.

Velikovsky was 78, while Sagan and Mulholland were both 39. Huber was probably in his early 40's, as was Storer. I had just turned 40, and Ransom was perhaps a little younger. If Ransom and I had been there to do some of the bull work, the playing field would have been more nearly level, in that we would have been three against four. As it turned out, Velikovsky's opponents were roughly about half his age, and he was outnumbered four-to-one. The organizers did not want to lose even this advantage that they had managed to arrange for themselves. Velikovsky had to be alone.

THE MAN ALONE

Actually, that device has been used repeatedly throughout the entire Velikovsky Affair. They have always wanted to depict Velikovsky as alone, a hermit savant, a man with no professional colleagues, and lacking in any support whatsoever from the scholarly and scientific communities. To have Velikovsky on the stage, with a Ph.D. on his right and a Ph.D. on his left, both arguing vigorously in his defense, was anathema. How could he be depicted as a hermit heretic if he were allowed to be seen in company with his scholarly and scientific supporters?

SHADOW BOXING

After Velikovsky died, there was for some years an almost schizophrenic contortion in the establishment's stance. They had grown accustomed to describing Velikovsky as if he were alone. Anyone wanting to debate his views insisted upon dealing with Velikovsky himself, not with his supporters. The conceit was that he *had* no learned supporters, and that he was always to be characterized as a one-man movement. When he died, the movement should have been over; after all, the man had no supporters!

But the movement was *not* over. So the critics now had to begin attacking the Velikovsky movement, a movement that according to them now had a membership of zero. What were they attacking? Why did they need to keep on attacking a one-man movement that no longer existed? Why were they boxing with shadows? This absurd posture continued for about a decade or so, before the critics realized that there was somebody out there fighting

them, and that it wasn't Velikovsky any more. Thus they came grudgingly to change their rhetoric. Some of them are finally learning not to refer to Velikovsky as lacking supporters.

But in 1974 the old stratagem was very much in vogue. The fact is that the A.A.A.S. was holding this Symposium precisely because Velikovsky seemed to be receiving far too much attention and interest from *scientists* and *scholars*, as well as from the general public. But they could never admit that he enjoyed any such following. They needed to present him to the world and to the media as standing alone.

There was no way in hell that a Ransom or a Rose would ever be allowed on that panel.

TO WEAR HIM DOWN

It is difficult to rest a competitor like Velikovsky, but Ransom and I could probably have managed it, at least to a degree. We could have prevailed upon him to let *us* first handle any issues that we were up to handling, which should have been quite a few, and *then*, if he felt that he had to, he could always add some brief remark. In most cases even that should not have been necessary. With Velikovsky, however, it probably *would* have been necessary, almost every time. (He was the sort of quarterback who doesn't want to stay out of the game for even one play.) Nonetheless, he would often have been merely supplementing what we had already said, and the physical strain on him would have been greatly reduced. He could have sat out somewhat more of the debate, and he could have used his voice and his lungs far less than he in fact did.

I am not suggesting that Velikovsky could not make it through the ordeal. The mere fact that he *did* stick it out to the end proves that he was indeed up to doing so. What I *am* stressing is how the *organizers* must have seen all this. If they could arrange things so that there was some better chance of wearing Velikovsky down, that would have been seen as very much to their advantage. And that is precisely how they did arrange things. They were not successful, of course, but it was not for lack of trying.

* * * * *

PART THREE: THE PLAYERS

SCOUTING REPORT

The performances of the critics were monotonously mediocre. That is perhaps the most striking thing that I have noticed in reviewing all of these events from the vantage point of twenty years later.

Certainly there were no major intellects among the critics who stood at the podium that day. Huber had taught himself some cuneiform, though he did not know what to do with it. Sagan could crack his cheap jokes, of course, and snow the snowable. But the fact is that Sagan, Mulholland, Storer and Huber were all quite mediocre as thinkers and investigators. King and Goldsmith were not particularly impressive either. (King was a nasty little fellow, with his face set in a permanent snarl. Goldsmith was sort of a gopher, thrilled to be mingling with his superiors. They chaired the morning and evening sessions, respectively, and had also been co-organizers of the Symposium.)

The only really powerful thinker in the bunch was the third co-organizer, Owen Gingerich, who was not even on the stage that day. Gingerich is nobody's fool. He has contributed much more to scholarship than the likes of Sagan, Mulholland, Storer, and Huber ever could, even if they were summed and squared. It was Gingerich who helped as much as anyone in orchestrating the press accounts. He met "over a dish of ice cream" with Robert Gillette of *Science*, and fed him some vitriol along with the ice cream, which was duly and uncritically repeated in *Science* on March 15, 1974. (A few years afterward, I had occasion to take Gingerich to task for this behavior, and we exchanged several letters. A little later in this chapter, I shall give a lengthy account of that exchange, with excerpts from both sides.)

Gingerich's other statements to the press appeared in *Newsday* and in *Rolling Stone*. Both of these magazines quoted Gingerich as saying that the reasons for Velikovsky's popularity were his "literal explanation of miracles" and "the appeal of Old Testament fundamentalism." People like Gingerich cannot accept Velikovsky's disavowals of fundamentalism, even when they hear them from Velikovsky himself. Thus in San Francisco Gingerich sat watching and listening when Velikovsky spoke of his frequent "disagreement with the Bible" and when he said, "I am not a fundamentalist at all, and I oppose fundamentalism." That isn't what Gingerich *wanted* to hear, so he went around saying the opposite anyway. One wonders how many other stories Gingerich might have been responsible for, perhaps on a "background" basis.

MULHOLLAND, STORER, AND SAGAN

King's introduction of Mulholland as "a celestial mechanic whose name is almost synonymous with high precision" says very little for King's own precision. Mulholland *claimed* to have read *Worlds in Collision* in 1950, when he was 16, in *Collier's Magazine*. Elsewhere he said he was "about 13." In fact he was 15. The *most* that he could have read in *Collier's* when he was 15 was *six* large, *Collier's*-sized pages that were excerpted from the book! Mulholland began his paper with the ridiculous statement that *Mars* and *Venus* erupted into the sky as comets! In the discussion period he had a long, embarrassing struggle with the concept of *minus* Kelvin! (There is no minus on the Kelvin scale: zero is "absolute.") Mulholland has been dealt with at great length in Immanuel Velikovsky and Lynn E. Rose, "Mulholland: 'A Celestial Mechanic Whose Name is Almost Synonymous With High Precision,'" *KRONOS* X:1 (Fall, 1984), pages 69-88; that piece provides a sample of *The Sins of the Sons*.

* * *

Storer is quite out of it. He is able to sit on a rigged panel without even noticing that the panel is rigged!

Storer can't even figure out what he's reading; thus he refers to things said by "de Grazia," but he means Juergens!

When Storer thinks that the whole world is watching, he flaunts his "determined, dogged neutrality." But when he thinks that no one will know about it, he says that Velikovsky is "quite out of his tree"!

* * *

A Sagan cannot afford to cheat (at least not too much) in his derivations: his colleagues will know all about the derivations, and may spot any flaws. But the colleagues do not know very much about Sagan's premisses. Indeed, they are quite eager to swallow almost *any* premiss that Sagan introduces as a statement of Velikovsky's position. The Velikovskians, of course, know that many of those premisses are false. But no one cares what they think; they never even get a hearing. And there seems to be no one else who wants to check whether those premisses are accurate or not.

It is extremely difficult for me to assess what Sagan really thinks. Sometimes I suspect that he knows exactly what he is doing, and that he has opted for publicity and applause and money during his lifetime, even if it means going down in history as one of his own bad little jokes. Perhaps he not only has no sensitivity to the content of the collective unconscious human mind, but is so wrapped up in the here and now that he does not even care how he will be remembered by posterity. Most of the scholars and scientists with whom I am acquainted are exceedingly concerned about how they will look a century from now. That is, they want to do serious work, work that will still look quite competent, even if it turns out to be wrong. Being wrong is not what they fear. Rather, they want to avoid doing anything that will look like an unworthy effort.

But a sophist will be known for a sophist. Posterity will see a Sagan as having crafted *deliberately* fraudulent arguments, which are *designed* to make the worse appear the better cause. (Sagan had the gall to send Velikovsky a copy of one of his books, inscribed "with all good wishes." Velikovsky sent him one of his own, which he inscribed as follows:

To Professor Carl Sagan

Civilities are met with civilities and in this spirit this book is being sent to you.

But perfidies, if persistent, find their retribution.

Immanuel Velikovsky

14 May 1977

Sagan's perfidies are persistent indeed.)

Other times, however, I wonder if Sagan might be kidding himself, too, and if he might actually believe that he is seeking the truth? Probably not.

Probably he realized early on that he had not the making of a scientist. Instead, he would popularize the work of the real scientists. And he would attack the perceived enemies of science.

This not only would increase his own income, but also would increase the popular support for scientific budgets. Thus the real scientists would find him useful to have around.

Sagan's name appears regularly as a co-author of non-popular articles, but how do his co-authors see this? Frank Drake, one of Sagan's frequent co-authors, says of Sagan: "when it comes to actually doing something, I do it. He's an expositor." (*Newsweek*, August 15, 1977)

To undermine Sagan's arguments, we need simply identify his false premisses. For this purpose, identifying *one* false premiss is sufficient, though there are usually *several* false premisses in any argument that Sagan advances against Velikovsky.

Even those of us who may feel swamped by all the physics and mathematics should find that this approach is quite simple and quite effective.

Any phony, perfidious argument that Sagan offers against Velikovsky *and* that has an orthodox derivation *must* have at least one glaringly false premiss, and perhaps more than one. Sagan's arguments against Velikovsky illustrate the computer programmers' principle of GIGO: garbage in, garbage out. Thus all that we really have to do in order to dispose of them is to identify the garbage input.

To illustrate how easy it is to dispose of a Sagan, let us take as samples the first three of his notorious *ten points*:

Point One: Venus from Jupiter. Sagan's premiss that the escape velocity from Jupiter is 70 km/sec is false. Sagan's premiss that the birth of Venus from Jupiter was a volcanic process is also false.

Point Two: repeated collisions. Sagan's premiss that the near-collisions recognized by Velikovsky were independent events is false. Sagan's premiss that the near-collisions recognized by Velikovsky were grazing collisions is also false.

Point Three: stasis and restarting. Sagan's premiss that Velikovsky recognized a stasis and restarting of Earth's rotation is false. (Any careful reader of *Worlds in Collision* knows that Velikovsky says that *either* a stasis or a slowing *or* an axial tilting would be consistent with the surviving accounts, but that Velikovsky was unable to determine *which* of these might have been involved.)

And so on.

(There is a large and growing literature on the details of Sagan's various "arguments" against Velikovsky. The Bibliography at the end of the book introduces many of these further discussions.)

PETER HUBER

Quota pars . . . ? This part.

I spent well over a year (in 1976-1977) working on the refutation of Huber. Ray Vaughan would have been more than able to handle this, either alone or with me, but he did not want to be involved in such a polemical project. That is the only reason that the two installments of the critique—JPW1 and JPW2—were not co-authored. Nonetheless, Ray helped me tremendously with the project—which is why I included the following acknowledgement in JPW2 ("‘Just Plainly Wrong’: A Critique of Peter Huber" (Second Installment), *KRONOS* IV:2, page 33):

"This entire critique of Huber draws very heavily upon joint research and writing done during the past seven years with Raymond C. Vaughan. This is especially true regarding the Ninsianna discussions (which appear in the present installment), but there is no section of the critique that has not benefitted from Vaughan's valuable corrections, clarifications, and suggested additions of further materials."

Thus Huber very definitely *nobis committitur*—even if in this case Vaughan preferred to leave the polemics to me.

There have been several other jobs that I also felt especially responsible for, if only because I happened to be in right (or wrong) place:

I felt obliged to spend most of 1986 studying ice cores. Others could have done that job, too, but it seemed to be primarily up to me because most of the important ice cores were stored right here in Buffalo, under the supervision of my own University. I was the Velikovskian on the scene.

I was also the Velikovskian on the scene when it came to monitoring my colleague Paul Kurtz, then editor of *The Humanist* and co-chairman of the Committee for the Scientific Investigation of Claims of the Paranormal (CSICOP), especially when the Committee was operating out of Buffalo.

* * *

The Babylonian Venus tablets, sometimes called the Venus tablets of Ammisaduqa, are a group of cuneiform tablets that describe the appearances and disappearances of Ninsianna (or Venus) over the course of a number of years. The relevance of these observations is that they might permit us to determine the astronomical situation that obtained at the time. Raymond Vaughan and I have discussed these issues in a series of articles over the past twenty years.

Other Velikovskians tend not to participate in the discussions of these Babylonian observations of Venus (or Ninsianna). They usually let Vaughan and me have our say, but without much comment one way or the other. Several of them have told me quite frankly that they find the Ninsianna discussions very difficult, and that they just don't understand them at all. For present purposes, then, a refutation of Huber is not quite enough: I must also try to make it clear to the reader just *why* he is wrong.

In this one case, some additional treatment of the main issues may therefore be helpful. It is for this reason also that I have made it a point to include many Ninsianna-related items in the Bibliography.

Besides, there have been at least a couple of significant new developments affecting the Huber story.

* * *

Huber's long, difficult paper boils down to two principal claims:

Materials from Uruk show that Venus existed as early as the early third millennium, presumably as an inner planet.

The Ninsianna document shows Venus already on its present orbit in the first half of the second millennium.

JPW1 (in *KRONOS* III:2) showed that Huber had carefully twisted his evidence, and had ignored what is perhaps the most important *representational* information about a cometary Venus that we have from antiquity (see page 111).

Velikovsky never denied that Venus had existed prior to the second millennium. On page 107, I even quoted his statement during the morning session:

"That Venus was observed before it came into conflict with Earth is clear from what I wrote. It did not come from Jupiter just on the eve of that collision. It came thousands of years before. It could be seen."

Without trying to go over all of the details of JPW1, let me simply point out that those ancient depictions of Venus that I brought together on page 111 make it unmistakable that the ancients *did* see Venus as a *comet*.

In recent years, some radical chronological revisions have been offered, notably by Gunnar Heinsohn and myself. Our discoveries have emerged from quite diverse avenues of investigation, but a number of our results are surprisingly consistent. (Here and there we do disagree. About the Sargonids, for example: I tend to accept virtually all of conventional Assyrian chronology, at least from the eighth century on; Heinsohn does not.)

What Huber had considered "early" may not be so early after all. Thus it is no longer plausible to say that the Uruk materials are from the early third millennium, nor to claim that the First Babylonian Dynasty materials are from the first half of the second millennium. This is another respect in which Huber's arguments are completely undermined.

* * *

Vaughan and I put the Ninsianna tablets in the eighth and seventh centuries. Huber follows the conventional view that the Venus observations are from the reign of Ammisaduqa of the First Babylonian Dynasty. (That is why they are sometimes called "the Venus tablets of Ammisaduqa.") According to conventional chronology, the First Babylonian Dynasty would have been in the first half of the second millennium. But Vaughan and I deny that the Ninsianna tablets are in any way connected with Ammisaduqa. Thus we do not want to see any intercalation records from Ammisaduqa forced upon the Ninsianna document.

Heinsohn puts Ammisaduqa in the fourth century, and identifies him with the Persian king Artaxerxes III Ochus; Heinsohn also identifies Hammurabi of the First Babylonian Dynasty with the Persian king Darius the Great. For reasons completely different from Heinsohn's, I agree with both of these identifications. (Notice that "Ammisaduqa" lived something like four centuries *after* the Ninsianna observations: no wonder the Ninsianna document does not mention him!)

* * *

Huber's contention that the Ninsianna observations reflect the present orbits of Earth and Venus actually depends upon *two* interrelated fits that he claims to have found. One is that the reported dates marking the disappearances and appearances of Venus at inferior and superior conjunctions are in accord with retrocalculation.

The other is that the attested intercalary months from the reign of Ammisaduqa are in accord with the intercalary months that are needed if the Ninsianna document is to feature the even spacing of the invisibilities that would be expected with the present orbits. (Among those so "needed" are the 11U and the 19U that are attested in the Ninsianna document.)

Huber admits that 15 of the 50 surviving dates are misses. This would be 30%. (The "15%" on page 59 of JPW2 should read "30%." But this is "a simple misprint of no concern.") Huber's count of the misses is not an honest one. The fact is that his own graph (*Scientists Confront Velikovsky*, page 126) shows that 60% of the dates miss. See JPW2, pages 43-48, especially page 46.

Huber's graph uses 4A, 5U, 10U, 11U, 13U, 19U, and 20A. (4A means an intercalary Adar in Year 4, 5U means an intercalary Ulul in Year 5, and so on.) If Huber had been honest and had used 13A and 14U (which are attested) instead of 13U (which he simply fabricates), his error rate would have risen from 60% to 76%. See JPW2, pages 53-63, especially page 59.

Huber claims that the fit between the attested intercalary months from Ammisaduqa and the intercalary months that are needed in order for the Ninsianna document to reflect the present orbits is perfect (page 126):

"The odds against finding such good agreement by chance between the intercalations in the [Ammisaduqa] tablets and those called for by the present value of the synodic period of Venus are greater than one thousand to one."

Let us look at these "greater than one thousand to one" odds. Between Year 1 and Year 21b there are about 25 intervals within which an intercalary month might fall. (The missing "Year 18" complicates the picture slightly, as does the possibility that an intercalary month might fall within, or at least overlap, an invisibility. For purposes of estimating the probabilities, however, we can speak of 25 intervals here. After all, Huber himself recognizes 25 such intervals.)

According to Huber, there are seven intercalary months that are *needed*, in order to get a spacing of invisibilities that would be very *roughly* comparable to the present state of affairs. But Huber's claim that only seven intercalations are needed is false. Actually, *nine* are needed. Huber prefers not to mention the 8U and 16U that also cry out to be included among the intercalations if we are to have anything even close to the present orbits. In any case, *his* seven needed intercalary months are: 4U or 4A, 5U, 9A or 10U, 11U, 13U, 19U, and 20U or 20A. He has never faced up to his need for 8U and 16U.

According to Huber, there are also seven *attested* intercalations under Ammisaduqa. This claim that seven intercalations were attested *was* true, as far as Huber knew in 1974. He was still unaware of the 13A contract that had been published by Finkelstein in 1972. The seven he knew about were 4A, 5U, 10U, 11U, 14U, 19U and 20A. By the time *Scientists Confront Velikovsky* was finally published, Huber had become aware of the 13A; he put it in his list (page 125), but not in his chart (page 126). The chart continued to use the fabricated 13U!

This "new" 13A (new to him, anyway) doesn't help him at all, so he just pretends that it does. In order to use 13A instead of 13U, he has to shift the Year 13b invisibility to one month later. So he tries to read it as if it were from X to XII instead of from IX to XI. He even says that these are "[t]he best readings" (page 142), which is a blatant lie. X to XII cannot be the preferred reading. The listing by months (in Section IV of the Ninsianna document) clearly shows 13b beginning in month IX. There is no hope of avoiding that, as Huber perfectly well knows. But the attested and the needed *must* fit, so he simply *says* that they fit. Q.E.D.

Despite the clear facts to the contrary, let us assume with Huber, if only for the moment, that there are indeed seven intercalary months that are needed, and that there are indeed seven attested intercalations under Ammisaduqa.

If there is no more than one intercalation in any one interval, then the chances that the seven attested intercalations would fall in the same intervals as the seven needed intercalations would be $7/25 \times 6/24 \times 5/23 \times 4/22 \times 3/21 \times 2/20 \times 1/19 =$ just over 0.000002. The odds would be more than 480,000 to one. (If we had to *hit* that magic number of *seven*, rather than some other number, that would only raise the odds still higher.)

Huber is indeed an amateur when it comes to inflating odds: compared to a Sagan, for example, he is clearly but a piker. (The thousand to one is a falsehood, of course, but that is not the point. Rather, the point is that a *Sagan* would have inflated it into a much bigger falsehood.)

Huber's alleged fit is itself a fabrication, and is based upon two false claims. One of these (that there are seven attested intercalations) he may have believed to be true, because he was not as up on the relevant literature as he supposed, and had not yet learned about the 13A that is attested.

The other false claim (that there are seven needed intercalations) he knew to be false all along. Thus *it* clearly *was* a misrepresentation all along.

Perhaps he felt it necessary to restrain himself when offering those "greater than one thousand to one" odds because he *knew* that his perfect fit was a fabrication. Perhaps he was afraid that if he claimed odds of *millions* to one someone might be more inclined to check up on him. After all, look what happens to Sagan every time *he* offers fabricated odds! (Yet Sagan never learns. Or at least doesn't mind. Sagan seems to know that his colleagues will not even care how many falsehoods he presents, as long as they are about Velikovsky.)

In San Francisco, and in *Scientists Confront Velikovsky* (page 126), Huber *claimed* that he had batted one thousand, by going seven for seven. (My metaphor.) But in JPW2 pages 53-63, especially pages 60-62), I demonstrated that he had actually batted only about $5/11 = .455$. Both his claim to have batted one thousand (seven for seven) and his claim that the odds against his doing this were "greater than one thousand to one" are completely bogus. They are fantasies, fabrications, and, in fact, lies. Gingerich's later assessment of me—"I find you overdrawing your case like a crooked lawyer"—would have better been directed at Huber!

In his 1982 book, *Astronomical Dating of Babylon I and Ur II*, Huber no longer claims a perfect fit that would defy odds of "greater than one thousand to one." Another big switch is that he has now decided to forget the 13U that he had simply fabricated in the first place. He continues to accept 5U and to reject 14U. He does not say *why* he does these things. (Could it be that my criticisms of him in JPW2, as on page 54 and on page 62, might have forced these changes of position?) By 1982, it is as if 13U had never played any role at all in his previous discussions. But of course it *did*.

Anyway, his 1982 list of attested intercalations under Ammisaduqa is 4A, 5U (which he describes as "LFS unpubl."), 10U, 11U, 13A, 14U (which he describes as "LFS unpubl., probably wrong"), 19U, and 20U or 20A. The 13U has quietly dropped out of sight. We hear nothing more about it.

Quite aside from the many other difficulties that Huber faces, this amended list of attested intercalations still fails to fit the "needed" intercalations, which would include 8U, 13U, and 16U, and would *not* include either 13A or 14U.

The bottom line, of course, is that the Ninsianna document simply does not agree with what is observed at present! Despite all of his faking and fudging, Huber cannot alter the embarrassing fact that these tablets do *not* reflect the present astronomical situation.

IRVING MICHELSON

For our present purposes, it is at least arguable that the most important person at the San Francisco Symposium, after Velikovsky, was neither Huber nor Sagan nor any of the others: it was Irving Michelson. We may be able to learn much more about the Velikovsky Affair from scrutinizing Michelson's behavior than from scrutinizing anything else.

Irving Michelson first came to my attention in the autumn of 1973, shortly after the A.A.A.S. had appointed him as one of the speakers for the Symposium that was to be held in San Francisco the following February. Michelson was interested in finding out as much as he could for purposes of the paper that he would deliver there. It was arranged that he would come to Princeton in November, to meet with Velikovsky and with a number of people who had some familiarity with various aspects of Velikovsky's work.

Antoinette Paterson and I flew down from Buffalo for these sessions. Among the others attending were C. J. Ransom, William Mullen, and Chris Sherrerd. Michelson sought out our help and commentary and input. He had many questions. He had a strong desire to get everything right, to know what he was talking about, to make sure that there were no slip-ups in what he had to say. I emphasize all this because it is in marked contrast to his later efforts.

In San Francisco, I saw a great deal of Michelson. We had several meals together, and we spent many hours discussing the matters at hand. He was a highly civilized person, friendly, gracious, warm, and open. He was full of questions about the Velikovsky theory. He was also extremely careful and cautious about his own work, which he took *very* seriously. He was not one to speak before thinking. If he did not know, he said so. He was also a man of considerable *pride*.

During the Symposium itself, he was a model of poise and scholarly decorum. He took some flack from the likes of J. Derral Mulholland, but he kept his cool, always treating the serious questioners like serious questioners and the jackals like jackals. (See the earlier stretches of the evening session.)

McMASTER UNIVERSITY

I do not know just what happened later on. Scholars have every right to change their minds, and that is what Michelson seems to have done.

Was he subjected to pressure? Was his pride offended? I'm not sure.

The first sign of change was at the McMaster University conference in June of 1974. Michelson gave a long, technical paper (entitled "Celestial ELECTROmechanics and Velikovsky's Catastrophism") on whether or not various combinations of magnetic and electrical circumstances might have been responsible for circularizing the orbit of Venus.

It was puzzling and even rather sad that he would be under the impression that all of this had anything much to do with Velikovsky's scenario. Nearly all of what happened to Venus during its turbulent career would have been due to gravity. Even Einstein had told Velikovsky that the Velikovsky scenario, including the circular orbit of Venus, could have come about *entirely* by way of gravitational effects!

Some electrical or magnetic effects, especially on close approach to other bodies, may have made the near-collisions and other passages a bit more spectacular, of course, but those effects had *never* been asked to carry the entire burden of changing Venus from comet to planet.

A *small, terminal* portion of the circularization of the orbit of Venus was attributed by Sherrerd not to electrical or magnetic effects, but to the effects of tidal friction. Michelson had of course *met* Sherrerd at the

Velikovskys', and he knew of Sherrerd's piece in *Pensée* IVR I, but in his McMaster paper he did not mention Sherrerd at all. (He did say that tidal friction effects are "weak" and that it would take "hundreds of millions of years at least" to change an orbit of high eccentricity to one of nearly zero eccentricity. But that is not what Sherrerd was talking about.)

The general consensus of those attending the McMaster conference was that it was too bad that Michelson had wasted his considerable talents on a non-problem, and that it was also too bad that nobody had steered him onto a more useful project.

Looking back at it, I think that perhaps the most significant aspect of Michelson's performance at McMaster was that it showed him much more inclined to try to throw monkeywrenches into the Velikovsky scenario. But almost no one paid him any attention, because it was so obvious that he still didn't really know very much about the Velikovsky scenario, and that if he had understood it better, and had realized that just about everything that happened to Venus was gravitational, then he would have known better than to offer his musings about ELECTROmechanics as having much to do with Velikovsky.

AT THE END OF *PENSÉE*

Perhaps he was expecting much more fuss. Perhaps the lack of gushing compliments on his McMaster paper drove him to an even more hostile stance.

Or maybe it *was* outside pressure, professional pressure to prove by his own actions that he was *not* a Velikovskian.

Michelson's next and final offering was a strange essay at the very end of *Pensée* IVR X. (That was also the very end of the *Pensée* IVR series itself; thus no one ever got a chance to answer him in that same forum.) Here he tossed off a series of ill-considered criticisms of the Velikovsky theory. His tone had now become pompous and hostile. (See "Scientifically Speaking . . . 19-Year Lunar Calendar: Accurate Adjustment to 365¹/₄-Day Civil Calendar," *Pensée* IVR X, pages 50-52.)

He had completed the transition from neutral to negative.

I had known since the late 1960's that nearly all of the published criticisms of Velikovsky's work had been of very poor quality. Even if the critics usually managed to behave competently in other areas, they would, when discussing Velikovsky, wane into incompetence and carelessness. It was a shame to see Michelson following in their footsteps.

PRESUMPTIONS

Notice, by the way, that Michelson's attacks on Velikovsky were accepted and published in a supposedly "Velikovskian" outlet, even though pro-Velikovsky pieces are rarely if ever accepted for establishment outlets.

Anti-Velikovskians, like Morrison, always want to debate Velikovsky himself, and always in a Velikovskian journal. They wouldn't *think* of debating Velikovsky or any of his supporters in an establishment journal.

Indeed, their line was that Velikovsky had no supporters. How can you debate them when they do not exist? And of course no one associated with an establishment journal would ever have considered Velikovsky a suitable topic for *their* journal.

THE MICHELSON METAMORPHOSIS

I am not concerned about Michelson's change of mind. That is something he had every right to do. But I *do* wonder why he would change his tone, his style, his method, his very persona? Even the characteristic carefulness and competence were absent now. It might as well have been the work of another person entirely.

The Irving Michelson with whom I spent many hours both in Princeton and in San Francisco (and with whom I had considerable correspondence during 1973-1974) was nobody's fool and nobody's patsy.

I admired the lengths to which he would go to try to ensure that he had everything right, that his calculations were accurate, and that his arguments and remarks were all in order. He was tough-minded and right on target. He was almost ruthless in his dedication to precision and accuracy. It was a matter of great pride to him.

SCIENCE

The Michelson with whom I was acquainted had no time for fools, and no patience with incompetence. This is well illustrated by the way he came down hard on the *Science* reporter who did not like the way he had dismissed the rabid and irrelevant babblings of Mulholland. See *Science*, July 19, 1974, pages 207-208:

"In his account of the untidy debate featured as Velikovsky's Challenge to Science at the AAAS meeting in San Francisco, Robert Gillette (News and Comment, 15 Mar. 1974, p. 1062) omitted mention of the irrelevance of the outburst from the floor to which I responded 'I'll let that go.' Those who heard my presentation as symposium panelist were aware that it deserved no other reply; your readers are entitled to know a bit more, having been given what Gillette told them."

"I did point out, among other things, that the energy required to turn the Earth's magnetic dipole through 180 degrees (interchanging positions of north and south poles) happened to be equal to that of a moderately strong *geomagnetic storm*."

"In the discussion period someone who wanted to voice an 'objection' talked about the energy of a *solar flare* and the spatial attenuation at Earth's distance from the Sun—declaring that one of my numbers was therefore very wrong. The relevance of solar flare energy to the geomagnetic storm energy confined to the geomagnetic cavity surrounding the Earth is about as small as the Sun's distance from the Earth is large. At most, we can say that the sudden influx of charged particles from the Sun triggers geomagnetic storms—their energy is to the energy of the storm as the detonator energy is to the energy released by the bomb it activates.

"There had already been all too much acrimony, back-biting, and anger expressed in the Symposium—and too many long-winded replies to comments from the floor. For me to launch into a lecture explaining the difference between the Sun's solar flare and the Earth's geomagnetic storms to one who either knew it already or never would know it, while all others present wanted to get on to more meaningful discussion of real questions raised by my presentation, seemed inappropriate. I hoped that most others present knew this was my meaning in refusing to enter into heated or lengthy dialogue with an individual whose zealous opposition to Velikovsky outran his reason."

For further treatment of this incident, see *KRONOS* X:1 (Fall, 1984), pages 77-79.

This demolition of Mulholland does not sound much like a man who would succumb to pressure from anybody. Thus I hesitate to conclude that there was any such pressure. Certainly there *has* been unseemly pressure on people to change their positions when those positions are favorable to Velikovsky. (Witness the Menzel and Bailey episode, described by Ralph Juergens in "Aftermath to Exposure," pages 51-53.) Even King was browbeaten by his colleagues into abandoning the fairness and the neutrality that he had originally espoused.

SCIENTIFICALLY SPEAKING?

Whatever the cause, Michelson did change, from a cautious, careful investigator to a shoot-from-the-hip oracle, one who had neither inspiration nor perspiration going for him. In the *Pensée* piece, he tossed off a series of ill-considered ideas in the manner typical of so many of Velikovsky's critics. He offered mathematical proofs in which the arithmetic was wrong. He got many of his facts wrong. He ended up saying nothing that could not be refuted by almost anyone with the patience to check up on him. His earlier work was simply not of that sort.

I am not proud of it, and I do not recommend it, but the fact is that I never took a mathematics course beyond high school. Michelson was a Ph.D., with a long and distinguished career as an astro-physicist and celestial mechanician. He thrived at altitudes where I could not even breathe.

How then was *I* able to spot so many flaws in his critique of Velikovsky? (See my "Michelson and Meton," *KRONOS* I:3 (Fall, 1975), pages 49-51, and the two following articles by de Grazia and by Stecchini.)

The reason is that almost everything that he said in *Pensée* IVR X was mistaken, either factually or mathematically. Furthermore, the technical level of this paper was *not* over my head. Maybe he kept it low because he was talking down to his audience. Or maybe he kept it low so that someone like me could trip him up. It was not all that much to my credit that I was able to refute him. It *was* not all that much to my credit that I was able to refute him. It *was* very much to his *discredit* that I was able to refute him. The mistakes that he made were simple, elementary, cut-and-dried. If *I* could find them, then *he* could and should have found them—and in about one-tenth of the time that it took me. They need never have left his desk. Indeed, they need never have been written down in the first place. The Michelson whom I knew would never in a million years have gone into print with *any* of those mistakes. A Michelson will never be shot down by someone armed only with general information and simple arithmetic. A Michelson's not about to have his errors enumerated by a Rose. It just wouldn't happen. Ever.

It cannot have been the same Michelson.

FREUDIAN SLIPS?

People like Michelson are caught because they want to be caught. There is no way that an Irving Michelson could be refuted by the likes of me unless part of him wanted to be caught. He was much too smart, much too knowledgeable, much too *careful*.

Deep down, they know that what they are saying is wrong. The only way they can argue a false cause is with sham arguments. But sham arguments do not fool people forever. Detection is inevitable.

Are all of these slips Freudian?

Yes, I suggest that they probably are. Indeed, I suggest that just about all of the slips that just about all of the critics of Velikovsky have made and continue to make are precisely that, Freudian slips. They seem to *want* the unspeakable truth to be noticed.

The case of Irving Michelson is magnificently instructive. With the possible exception of Abraham Sachs, who will be discussed in another chapter, I know of no other instance in which the transition from competence to incompetence is so clearly documented.

With most of the other critics, it is just unrelieved incompetence. There is nothing particularly *new* in their behavior, and thus it is not all that easy to attribute their incompetence to the fact that they are arguing against Velikovsky.

In a very small number of other cases, the competence level remains fairly high, even when Velikovsky is being criticized. I have people like R. R. Newton in mind here. But when we are looking for a *sudden* onset of incompetence, Newton is not a suitable example. (Even though his criticisms of Velikovsky *are* answerable: see S. F. Kogan, *Physics Today*, April, 1981, pages 15, 72-73.) It is only with Michelson, and perhaps Sachs, that we have the spectacle of someone with demonstrated competence losing that competence when it is a matter of attacking Velikovsky. Why this sudden *loss* of competence?

I do know of cases where people have been subjected to massive professional and personal pressure, and have had to change their ways. But that is usually quite different. Those people tend to go from open support of Velikovsky to discrete silence. I do not criticize them: I have not had to walk in their shoes.

Nor do I criticize those who have found it necessary to do their pro-Velikovsky writing under pseudonyms, to keep their professional associates and colleagues off their backs.

Nor am I particularly criticizing Michelson. I liked him back then, and I can still say that I like him, even though I have had no contact with him in twenty years, and do not even know if he is still alive. I am *not* prepared to say that I still admire him. That would depend to a large extent upon just what it was that actually happened to him. In any case, no one should be encouraged to emulate the flip-flop that he executed. Whatever the reasons, he *caved*.

For all I know, perhaps anybody else would have, too, in his shoes.

Michelson's deterioration probably did not hurt the Velikovsky movement in any serious way. By abandoning his own standards, however, he would very clearly have done considerable damage to himself.

OWEN GINGERICH

On March 31, 1978, I sent Owen Gingerich a copy of *Velikovsky and Establishment Science*, with the compliments of *KRONOS* Press. I called his attention to the first installment of "'Just Plainly Wrong': A Critique of Peter Huber," on pages 102-112. I then continued as follows:

"You were quoted by Robert Gillette in *Science* (183, 1061) as having said that "He [Huber] demolished Velikovsky" and that "There was really no point in continuing after that."

"I am presently putting the final touches on the concluding installment of my critique of Huber. In the course of refuting Huber, I had intended also to take you severely to task, on the basis of the quotations in *Science*, for your uncritical acceptance of Huber's handiwork.

"But were the remarks that Gillette attributed to you in his article accurately reported? If they were inaccurate, and if you are willing to repudiate them in print, then I would like the opportunity to revise my own remarks prior to their publication. But if the attributions by Gillette were accurate, or if you do not wish to repudiate them in print, then I shall proceed with my second installment as planned."

My letter (but not the book) was forwarded to Gingerich in Germany, where he was on sabbatical. He answered on April 26, 1978, without having seen the book. In his letter he remarked that:

". . . I really felt that Prof. Huber had demolished Velikovsky both with respect to his paper (with which I could find no fault apart from its being too technical for a general audience) as well as in the remarkable exchange between him and Velikovsky with respect to their respective controls of the cuneiform language. Prof. Huber certainly came to this assignment with

impeccable [sic] credentials. I have been told by one acquaintance at Yale (who are [sic] miffed when [sic] he turned down a full professorship there) that he is one of the two leading statisticians in the world today, and one of his specialties is the treatment of data contaminated by wildly erroneous data, precisely the situation when scribal errors are superimposed upon generally smaller observational errors."

Gingerich concluded with some mild reservations about whether he had been quoted verbatim, but he also acknowledged that "Gillette represented the general tone of the conversation correctly."

I replied on May 15, 1978. The bulk of my letter read as follows:

"There are several points in your letter that I wish to address.

"You referred to 'the remarkable exchange between him [Huber] and Velikovsky with respect to their respective controls of the cuneiform language.' My understanding of what was going on at that point is quite different.

"Since Velikovsky does not read any of the cuneiform languages, and never claimed to do so, there would have been no point to his trying to show that his cuneiform credentials were superior to Huber's. Besides, I am sure that you would agree that credentials are in any event no substitute for evidence and argument: mere credentials do not make a bad case good or a good case better, nor does the lack of credentials make a good case bad or a bad case worse. No, Velikovsky was not comparing his and Huber's cuneiform credentials. Nor was Velikovsky arguing that, because Huber is not a professional in cuneiform languages, Huber's case must be wrong. If such an argument were valid, which it is not, it would just as well apply to Velikovsky himself.

"Rather, Velikovsky simply wanted to bring out the fact that Huber's professional field was statistics and that Huber had described Assyriology as his 'hobby.' There were several reasons why this needed to be brought out. For one, the A.A.A.S. program described Huber as a 'Prof. of Ancient History' speaking on 'Ancient Historical Records,' as if that were his area of professional expertise. For another, even though Velikovsky had been assured that there would be equal numbers of 'pro- and anti-Velikovsky speakers,' his own nominees (C. J. Ransom and myself) were rejected for what Velikovsky considered irrelevant reasons. I was rejected because my degrees were not in the hard sciences. That would not work for Ransom, who is a Ph.D. in plasma physics, but he was rejected on the grounds that he was employed by a corporation rather than by a university. The sponsors then appointed their own choices and declared the panel closed. One of their selections was Huber. The rigid criteria that served to exclude strongly pro-Velikovsky panelists like Ransom and me suddenly and curiously became very flexible when a self-styled 'hobby-assyriologist' wanted to present what he took to be 'early cuneiform evidence' against Velikovsky. This, I think, is what Velikovsky was driving at.

"You mention a report from Yale that Huber 'is one of the two leading statisticians in the world today.' I cannot disprove that, but if it is true, then there must be an extreme paucity of responsible talent in the field of statistics. For Huber does not display even minimal respect for data. Some cases of this were reported in my first installment, and others will be reported in my second installment. Thus Huber claims that the attested intercalations under Ammizaduga and the 'needed' and/or reported intercalations in the Ninsianna observations match up in every case, and he brags that the odds against this happening by chance are greater than 1000 to one (he gives no calculation of these odds). But the fact is that only about half of these intercalations involve matches; the rest are clear misses. For example, Huber needs a second Ulul in Year 13, so he puts one in, even though none is attested under Ammizaduga! He needs *not* to have a second Adar in Year 13 and a second Ulul in Year 14, both of which are attested, so he just throws them out! No wonder he beats 1000 to one odds! Huber's 1000 to one odds are worth about as much as Sagan's 10^{23} to one odds.

"Huber also claims that only thirty percent of the reported disappearances and appearances of Ninsianna fail to fit his retrocalculations. But the truth is that *sixty* percent fail. Even then, he has fudged on the intercalations. If he had respected the attested intercalations under Ammizaduga, instead of rewriting them to fit his uniformitarian bias, he would have missed on seventy-six percent of the reports. That is a far cry from thirty percent.

"In San Francisco Huber himself told me that he has done a lot of work with bad data. You and he both seem to feel that that experience relates to the Ninsianna data. I would like to make two comments. In the first place, while I do not know what Huber's work with bad data amounted to, it should be noted that he perceived the Ninsianna data as 'one of the worst data sets' he had ever seen. Thus it is by no means clear that this is 'precisely the situation' that Huber had handled in his previous experience. It would be an *extreme* case, not a typical case of the sort that he had usually worked on. (I realize that that isn't *exactly* what you said, but I gathered that that might be what you had in mind.)

"In the second place, the Ninsianna data are not as bad as all that, anyway. In most cases it is fairly obvious what the original reading was. There are alternative readings, to be sure, but many of these involve only a day or two, and are of negligible import. Nearly all of the rest can be reconstructed on the basis of the relative weight of the surviving textual evidence. (This is a purely textual process; orbital calculations do not enter the picture until later.) Only the uniformitarians see the data as extremely bad. But what if both Venus and Earth were on orbits different from their present orbits? People who insist on using substantially the present orbits of Earth and Venus are inevitably going to have to reject most of the observations that were made when both orbits were different.

"Thus the work that needs to be done on the Ninsianna tablets—work that Raymond Vaughan and I have been engaged in since 1972—is not primarily statistical. It is mostly a matter of establishing the text and then determining what orbits of Earth and Venus would produce the observations reported in the text. It will not do to retroject substantially the present orbits and reject or rewrite any report that conflicts with the retrojections. People who try this typically have to reject the great bulk of such reports, whether they admit it or not. Huber's score at this is just as poor as the scores of his predecessors, such as van der Waerden."

Gingerich answered on June 29, 1978. He ignored most of what I had said, but he did address two points:

"The 'remarkable exchange between Huber and Velikovsky' that I was referring to was rather different from the one that you remembered. However, I was very interested to have your interpretation, because at the time I considered that the comments by Velikovsky were distastefully *ad hominem*, and in principle barred from the meeting by the rules agreed upon beforehand. But what I was actually commenting on was (if I can recall correctly) a little later in the debate, where Huber showed that Velikovsky's interpretation of a cuneiform text was quite erroneous because of a disregard for the standard superlative grammar form."

A little later in his letter, Gingerich made the incredible statement that "Velikovsky consistently refused to make any direct suggestions for speakers"!

In a postscript, he also suggested that I had:

". . . overlooked a rather significant point in Prof. Huber's argument. He had applied the same statistical analysis to much more recent Babylonian texts, at a time when presumably even Velikovsky agreed that Venus had its present orbit, and found that the data had precisely the same kind of errors that characterized the earlier Venus tablets of Ammizaduga."

I replied on September 15, 1978, and enclosed a copy of my second installment. The great bulk of my letter read as follows:

"In your letter you mentioned that at the time of the Symposium you thought that Velikovsky's questions about Huber's training in Sumerian and so on 'were distastefully *ad hominem*, and in principle barred from the meeting by the rules agreed upon beforehand.' I would like to pursue this a little farther.

"In my letter of May 15, 1978, as you will recall, I had mentioned several of the reasons for Velikovsky's concern about this matter. One reason was that the A.A.A.S. program described Huber as a 'Prof. of Ancient History' speaking on 'Ancient Historical Records.' This needed to be clarified.

"I also mentioned that when Velikovsky proposed Ransom and me as panelists, I was rejected from having no degree in physics, while Ransom, a Ph.D. in physics, was rejected for working in industry rather than in academia. Yet Huber, who has repeatedly called himself a 'hobby-Assyriologist,' was not asked to satisfy such rigid criteria. (If you are unaware of these developments, I suggest that you check with your fellow organizers, especially Goldsmith. The proposals *were* made, and the reasons for rejection *were* given.)

"But there are several additional reasons, which I did not mention in my letter, for Velikovsky's concern about this matter. Thus, King introduced Huber as one who 'has made a study of the ancient archaeological records relating to astronomy. He also, incidentally, has a second specialty in statistics, and we are very pleased to have him speaking with us, to us today on ancient historical records.' Is statistics Huber's 'second specialty,' something to be mentioned only 'incidentally'? Are we supposed to believe that Huber's *primary* specialty is then 'ancient archaeological records relating to astronomy'? It was because of such gross distortions as these that Velikovsky considered it necessary to clarify matters.

"In a conversation with the Velikovskys on February 24th, the day before the Symposium, Huber had referred to himself as a 'hobby-assyriologist' who was self-taught and who knew no Sumerian. He also admitted to a quite skimpy acquaintance with Velikovsky's work; he said that he had read only 'here and there' in *Worlds in Collision*. But at the Symposium on February 25th this restrained and self-effacing man behaved, as Velikovsky put it, like a district attorney asking for the death penalty. Having read 'here and there' in *Worlds in Collision* (and apparently never even noticing such things as Velikovsky's discussion of the Ninsianna tablets on pages 198-199), Huber was now prepared, on the basis of the very evidence *already used* by Velikovsky (as well as on the basis of Sumerian sources), to pronounce Velikovsky's entire reconstruction a failure.

"You may recall how deceptive and evasive Huber became. He knew exactly what Velikovsky was doing, but he had no intention of cooperating. When asked if he read Sumerian (the day before, he had said he didn't), he replied, 'No, I do not really speak the Sumerian language.' When Velikovsky repeated the question, emphasizing that he had said 'read,' not 'speak,' Huber's reply, again evasive, was that he was 'not so familiar with Sumerian as a Sumerologist would be.' And of course Huber was not about to call himself a 'hobby-assyriologist' on that occasion, although he has used that sort of language both before and since the Symposium. The hobbyist of February 24th had undergone a complete metamorphosis in just one day.

"If anyone at the Symposium was treated in an *ad hominem* manner, it was Velikovsky. The personal attacks on Velikovsky are of course a very long story, and I will mention just one example here: King's statement that 'none of us in the scientific establishment believes that such a debate [on the correctness of Velikovsky's view of the planetary system] would be remotely justified at a scientific meeting'; rather, according to King, the motivation for the Symposium was 'the persistence of these views,' and the Symposium was an effort at 'educating the public' that Velikovsky's views were 'of too little weight to take seriously.' As I wrote at the time, 'It is rather awkward to bring the originator of a theory to a scientific gathering, and then let it be known that

the theory is not under discussion, but only the state of affairs that has led to the growth and spread of the theory.' The 'rules agreed upon beforehand' were that it would be a debate on the theory. King and others tried to change those rules; Velikovsky did not.

"By the way, King said that his statement 'expresses accurately and faithfully the views of the symposium organizers.' Is that true of you? What else but *ad hominem* would you call King's approach?

I then continued as follows:

"The Weidner matter [having to do with Velikovsky's acceptance of Weidner's view about the apparent use of the superlative in that one particular text] is briefly referred to on page 4 of my second installment [pages 35-36 of the printed version]. The formation of superlatives is as Huber says, but I must reserve final judgment until the text itself is reexamined from a Velikovskian perspective. I suspect that Weidner knew at least as much about Assyrian superlatives as Huber does. It would be helpful if someone could discover just *why* Weidner believed that this was *not* a simple case of a superlative. Huber's conjectures are far from definitive.

"I wish that Huber—and you, too for that matter—had focused more on Weidner, and not exclusively on Velikovsky. Huber presented this point in San Francisco without even mentioning Weidner. And your letter speaks of 'Velikovsky's interpretation' without any indication that it is really Weidner's interpretation, cited by Velikovsky. Both you and Huber seem to have overlooked the fact that Velikovsky merely cited something that he found in the scholarly literature. Is it wrong to cite Weidner? (Neugebauer and van der Waerden cite Weidner from time to time.) In any case, Huber's criticism should have been directed primarily at Weidner. As I suggest in the paper, the weight of evidence in favor of Velikovsky's theory remains enormous, with or without Weidner's interpretation of this particular passage.

"I have *not* overlooked Huber's alleged comparison of the Ninsianna data to the late Babylonian data. Huber *doctored* and rewrote the Ninsianna data in a number of ways until he got them very *roughly* consistent with the present planetary system. Only then did he compare the doctored 'data' to the late Babylonian data. I discuss this on pages 18-19 of my second installment [pages 47-48 of the printed version]. There is no way that the Ninsianna data themselves can be reconciled with the present orbits of Earth and Venus."

(Eventually, Vaughan and I were to discover that the Ninsianna observations seem to reflect a situation where Venus was already on its present orbit but Earth was not yet on its present orbit. In particular, we found that the orbital eccentricity of Earth appears to have been considerably greater than it is now.)

That third letter of mine *really* seems to have gotten under Gingerich's skin. Instead of continuing to pretend that he was interested in looking at the evidence, he now switched to explaining why he, as a serious scientist, would find it necessary to make up his mind *without* looking at the evidence! He replied on September 22, 1978.

"Each scientist is obliged to survey the entirety and to decide for himself where to throw his efforts. Although major counterclaims are made by, say, astrology, and by Velikovsky, most scientists firmly believe that it would be profitless to investigate the claims. The reason is that it will not do to 'prove' something in one small area because the totality of all the claims go against too large a fraction of accepted science. The pattern of science simply does not show such signs of stress to warrant dropping such essential principles as uniformitarianism.

"For better or for worse, this means that the majority of choices of what to believe are made not on a detailed examination of evidence, but on how it fits into the entire framework of

science. It is plain to me that you and Huber are working in a very complex area of interpretation and that without my devoting an inordinate amount of effort to understand the individual details of the arguments and the sources, I am simply going to have to make my judgment on other criteria. Because Huber's *conclusions* make eminent sense to me, I accept them; and I assume that your arguments contain fundamental errors, even though I have neither the time nor the expertise to find them."

Finally, Gingerich turned to some of the specifics on hand:

"That I have made the correct judgment in this case is reinforced by reading your final appendix where I do know something of the circumstances and where I find you overdrawing your case like a crooked lawyer. You say that Velikovsky nominated you and Ransom for the panel. From the point of view of the organizers, Velikovsky was forever cagey and refused to suggest anyone. Only in a very frustrating and roundabout way could we finally determine that he would approve of certain choices such as Michelson. You go on to say that Velikovsky was 'dropped' from the final volume, never hinting that Velikovsky refused to have his paper published there. Yet in your writing elsewhere you applaud his refusal. Nor do you choose to remember Velikovsky's spoiled insouciance at the AAAS, how he refused to come down to the meeting unless he was always seated on the stage and had his own microphone so that he could interrupt whenever he chose. Nor do you mention that he simply took twice as much time as had been allocated to any other speaker; thus the program got completely off the track with respect to the schedule. Incidentally, we considered Professor Storer as a neutral participant, and with Velikovsky taking double time and having Michelson as his approved panelist, the panel was exactly balanced, contrary to your claim of four-to-one odds. Of course, Professor Michelson would have had time to speak at the morning session if Velikovsky had not balked and had arrived on time."

I replied in a very long letter of October 16, 1978. The concluding pages of my letter read as follows:

"You say that you 'do know something of the circumstances' discussed in my Appendix, and you accuse me of overdrawing my case 'like a crooked lawyer.' In support of this accusation you offer six points (as I count them).

"The main focus of my Appendix was of course on the inconsistency between the way Ransom and I were treated and the way Huber was treated. Your criticisms seems to be concentrated on four short paragraphs, totalling twenty-three lines typed, twenty-eight printed, in which I briefly summarized the background of the AAAS-Cornell affair. There was much that I did not mention in those brief remarks (a full account of the AAAS-Cornell affair would be quite lengthy), but I think that what I did say was fair, and that whatever I did not say was either irrelevant, or unnecessary in that context, or false. I will reply to your six points in order.

"1. Velikovsky *did* propose Ransom and me as panelists, and we were rejected, because I was not a physical scientist and because Ransom was not employed in academia. If, as you claim, Velikovsky 'refused to suggest anyone,' why do you suppose that those specific reasons were given for rejecting Velikovsky's suggestions? After Velikovsky's initial suggestions were rejected for such frivolous reasons, it is quite understandable that he was somewhat reluctant to propose further names for cavalier rejection. It is not that he was 'cagey'; rather it was that he had already been stung once (twice, actually), and was not eager to be stung again. *That* was the reason for what you call the 'very frustrating and roundabout' course that the negotiations subsequently took. If anyone is entitled to call the panel selection efforts 'very frustrating' it is Velikovsky: his proposals of Ransom and me *were* frustrated; your proposals of Huber, Mulholland, Sagan and Storer were *not* frustrated.

"2. One reason for my 'never hinting that Velikovsky refused to have his paper published' in the Cornell volume is that there was no such refusal. Nor did I in any of my writings 'applaud his refusal.' You may be confusing his AAAS paper that was read in San Francisco and his 'Afterword' (the real one) that was written in early 1976. Velikovsky's expressed desire, before as well as after the Symposium, was that his paper as delivered, the other papers as delivered, and the transcripts of the proceedings should all be published as soon as possible. It was the 'Afterword' that he decided not to submit. Even though he finished the 'Afterword' within the time and space limitations laid down by the AAAS-Cornell people, he finally decided that he would not provide them with the 'Afterword' under such unfair restrictions. (To what limitations of time and space was Sagan subjected when he took nearly two years to revise his paper and expanded it by nearly fifty percent?) Thus what Velikovsky withheld was his 'Afterword,' but the decision to delete Velikovsky's *lecture* and the transcripts was made by the AAAS-Cornell people, as I stated, not by Velikovsky.

"3. The debate was originally scheduled and described as a *panel*. When it was later decided that there would not be a panel in the strict sense, but that each speaker during the morning session would appear alone on the platform, Velikovsky did indeed protest, and he continued to protest right down to the moment the Symposium began. What you call 'spoiled insouciance' I would call a justified and emphatic objection to the abandonment of the promised panel format. Your supposition that Velikovsky wanted to be seated on the stage with 'his own microphone so that he could interrupt whenever he chose' is unfounded and unfair. I—both as a spectator in the audience and as a fellow-panelist on the platform—have observed Velikovsky's participation in a number of debates where there were other panelists who criticized him. At not one of these events did he ever behave in the way you speculate that he might have behaved had he had a microphone.

"4. The extra time that King grudgingly allowed for Velikovsky's delivery (hardly inappropriate, in that the Symposium was on Velikovsky's work) was not the only or even the decisive reason that 'the program got completely off the track with respect to the schedule,' especially as regards the moving of Michelson's paper to the evening session. An additional reason, and indeed the main reason, was the Sagan, late in the morning session, suddenly informed King that he was leaving for an appearance with Johnny Carson on *The Tonight Show*. This is what caused King to shift Michelson's paper to the evening session, in order to allow time in the morning session for a lengthy discussion of Sagan's paper. (You have from time to time alluded to various things that I have not mentioned; your point seems to be that I should have mentioned them. I do not understand why you would expect my paper about Huber to mention Velikovsky's efforts to get King to return to the promised panel format. But if you are going to bring this up, and if you are going to bring up Velikovsky's reading time as well, why don't you also mention the fact that it was Sagan's announcement to King, at the latest possible moment, that finally necessitated the rescheduling of Michelson's paper in the evening?)

"5. Storer hardly qualifies as a 'neutral,' despite his and your claim to that effect. Storer has described Velikovsky as 'quite out of his tree.' So much for neutrality! Aside from his paper's long list of gross misstatements—that rivaled Mulholland, Huber, and Sagan—Storer's main error in San Francisco (like yours—of which more later) lay in assuming that whatever the scientists did not have time to examine they were entitled to denounce as false. He also took it as a mark of his objectivity that he divided the guilt between the suppressors and the one suppressed. Velikovsky correctly described Storer's paper as a 'whitewash' of scientific misbehavior. Your suggestion that Storer doesn't count as a negative panelist is wrong. Velikovsky was obliged to spend just about as much time answering Storer as, say, answering Mulholland. The need to answer Storer was important enough that I directed my first (and only) question from the audience to Storer, rather than, as might have been expected, to Huber.

"6. Michelson did not utter a word during the morning session. His evening session paper could be regarded as generally (but not entirely) in harmony with Velikovsky's theories, though Michelson described himself as neutral; in any case, even in the evening session, Michelson (who has never claimed any special familiarity with Velikovsky's theories anyway) did not once attempt to defend Velikovsky against all the charges that had been brought earlier in the

day. Velikovsky was *alone* against *four* in conducting his defense. Thus your efforts to balance the panel—by calling Storer a neutral, by putting Michelson on the Velikovsky team, and by counting Velikovsky himself twice—strike me as unsuccessful, to say the least.

"You conclude by repeating your claim that it was because of Velikovsky that Michelson was moved to the evening session. But I too repeat: if Sagan had been available for discussion in the evening, Michelson could still have given his paper during the morning session. Why do you leave Sagan blameless in this?"

I then turned to Gingerich's incredible effort to wash all stain from his own hands, while asserting his right to continue to denounce as false whatever he and his colleagues think might be false, even though they are too busy to look into it!

"Your letter seems to be a declaration that henceforth you will not be concerned with evidence or argument about the matters at issue, but that you will make up your mind on the basis of whether someone's conclusions are compatible with various *beliefs* that you and your colleagues have long held.

"One such belief that you mention is uniformitarianism, which you call 'essential.' But 'essential' to what? Those who have discarded uniformitarianism have stepped on a few *toes*, and of course have scrapped a few *theories*, but they have lost nothing of value, and no known *facts* stand in the way of their rejection of uniformitarianism.

"Uniformitarianism itself is the major 'point of stress' in establishment science. The evidence against uniformitarianism has been mounting and is now decisive. The only reason establishment science does not crumble in the face of that evidence is that establishment scientists choose not to face the evidence and to pretend instead that establishment science 'simply does not show such signs of stress to warrant dropping such essential principles as uniformitarianism.' How long they can keep that up remains to be seen.

"I have now been studying Velikovsky's theories for over a dozen years, and I find that the establishment views have little to offer *except* points of stress. Using Velikovsky's work as a key, it is relatively easy to spot where the establishment's points of stress should be, whether they have been noticed or not. Invariably, I have found that inspection of such a point of stress turns up an establishment can of worms: suppression of negative evidence, fabrication of positive 'evidence,' fallacious reasoning, and acts of faith. Modern writing about antiquity consists almost entirely of this sort of thing. So does uniformitarian astronomy.

"I frequently run into people who say that while they have heard of Velikovsky they have decided because of the press of other interests and obligations not to look into his theories; consequently, they say, they have no opinion about whether he is right. That seems to me to be the only proper stance for such people to take. It is highly *improper* for them—if they don't have time to look into the issue—to take a stand on the issue, especially if they have some reputation as 'authorities.' Storer's position—and yours—seems to be that whatever is not accepted as true may properly be rejected as false, without any examination of such irrelevancies as evidence and argument. What happened to 'I don't know'? I suggest that those who don't know should simply say that they don't know, and not declare to be false all those scholarly efforts of which *they* happen to be ignorant.

"If you had simply said that you had not done the relevant homework and could not participate in the debate or offer an opinion, I could accept that. But you made sweeping pronouncements, such as 'He [Huber] demolished Velikovsky,' which went far beyond any mere judgment that Huber's *conclusions* were consistent with uniformitarianism. Clearly, you were speaking of Huber's *evidence* and *arguments*, and you were *endorsing* them in the strongest possible terms. Furthermore, your remarks carried great 'authority.' Your academic titles alone guaranteed that your views would have some influence, but you also have a considerable reputation that goes well beyond your titles. Furthermore, you have described yourself in the

Scientific American sketch as one whose interests *do* extend to matters Babylonian. I find it difficult to believe that anything in my paper was over your head, or that if there had been anything seriously wrong you would not have found it. If you are going to speak out as you did in the pages of *Science* through Gillette, people have a right to expect more from you than something like—and I paraphrase—‘I’m too busy to look, but you must be wrong because you disagree with what my colleagues and I choose to believe.’

"All along, your letters to me indicated that you *were* going to look, and were going to let the chips fall where they might. Frankly, I can't help but suspect that you *did* look, did not like the lay of the chips, and recoiled from any further looking. I hope that my suspicions are wrong."

Gingerich never responded. I now believe that my suspicions about his in 1978 were right on target.

EPILOGUE

SHOULD HE OR SHOULDN'T HE?

I felt, almost from the beginning, that the right thing for Velikovsky to do was to withdraw. (See "Some Good Advice," *KRONOS* III:2, pages iii-v.) The organizers had quickly proven themselves to be duplicitous, unfair, and unprincipled; they were petty little connivers who broke their old promises even faster than they could make new ones. Matters only got worse as time went on. After the Symposium, the crooked dealing only intensified. The arrangements for the book were even more dishonest than the arrangements for the Symposium itself.

Velikovsky's supporters had not been on the panel, so of course they could not be in the book. David Morrison was not on the panel either, nor even present at the Symposium, but he *was* allowed in the book. For good measure, Isaac Asimov was permitted to add a preface. He had not been in San Francisco either, let alone on the panel. The transcripts of the proceedings were supposed to be printed in the book, but the A.A.A.S. people did not even prepare them, let alone print them. (They did have a very rough version of the morning session prepared, but by someone who did not know or care to know the names, the vocabulary, and so on. It was of no value at all.)

Eventually, I prepared the transcripts myself. They are printed—for the first time, and in their entirety—at the end of this book.

RUPTURED DISK

I suffered a ruptured disk in 1977. There are very few advantages to a ruptured disk, but there are some: you are not allowed to use a vacuum cleaner, or a lawn mower, or a snow shovel. Unfortunately, there are a great many disadvantages. For about a month I could not walk more than fifty or sixty feet without stopping for several minutes to recuperate. I was unable to lie down a bed, and for some months I had to sleep in a partially reclined chair. Most of my days were passed in that same chair. I was still able to write fairly well with a pen and a tablet, but not on the typewriter. I could type for only a few minutes at a sitting. Thus I came to spend much of my time reading, rather than writing.

One of the few things that I could do well during that period of time was to transcribe the A.A.A.S. tapes. I would listen to them over and over, stopping and starting, going back over troublesome passages, transcribing them by hand onto a tablet, and then listening to them again and again until I had everything verbatim. I probably would not have bothered if I had not been laid up.

When I got better, I listened to the tapes again, carefully checking the handwritten versions, and then I typed both the morning and the evening transcripts. The A.A.A.S. people were supposed to do all this, but they did not. That was just one more of their broken promises, along with all of the others that have been enumerated.

THE CLUBS

There were two big clubs that kept Velikovsky putting up with their abuse. If he withdrew in advance of the Symposium, that could have been given what is now called spin, suggesting that he was afraid to deal with scientific critics in open debate. If he refused to participate in the book, they would say that he was unable to answer their charges, and so on.

My own feeling in those years was that neither of these clubs should have been allowed to determine Velikovsky's course of action. These people were behaving in a vile manner. The sooner he stopped his dealings with them and got back to writing his books, the better. Spin or not, the playing field was now tilted almost to the zenith, and it was just not worth it. Besides, they had already proved that they could orchestrate the press just as effectively as they orchestrated the Symposium and the book. I repeatedly urged Velikovsky to step away from them and declare them uninterested in examining the issues. Let them put whatever spin on it they could. A string of books from Velikovsky would give them more *tailspin* and apoplexy than they had ever bargained for.

Whether he should or should not have participated may depend ultimately upon what effect it would have had on shortening the Velikovsky Affair. In any case, I have recently abandoned my earlier attitude that he should definitely have withdrawn. Maybe, just maybe, then entire A.A.A.S. Affair in some way helped with the Velikovsky Affair. If nothing else, it showed that that Velikovsky Affair was not over. Besides, if Velikovsky had withdrawn, we might never have known precisely how desperate and wretched these people were. He gave them a lot of rope, and they used it to hang themselves.

Yet I do not believe that that was ever his wish. He always seemed to be hoping for something better from them. Their *relentless* and *shameless* duplicity genuinely surprised him.

POSTERITY

But perhaps Velikovsky was looking farther ahead than the rest of us. It took me twenty years, but I now realize that when historians of future centuries retell this story, they may think it just as well that Velikovsky did have to endure that abuse, and they may also think it just as well that Ransom and I were *not* on that stage with Velikovsky. To them, the picture of Velikovsky standing alone and triumphing against his assailants may seem to have been worth it. I just don't know.

QUO VADIMUS?

What are we to do from here on?

We must continue to discover, design, and distribute the ammunition that will be needed in the on-going struggle. This ammunition might be in the form of empirical evidence, or in the form of argument, or even in the form of relevant citation. Whatever it is, we need to make it all available. To sell the Velikovsky theory to the establishment may not be possible yet, but we can at least build the case—for anyone willing to look.

In Part One I speculated about just what sort of person it is who is willing to look at evidence of past catastrophe without wallowing in resistance. Whatever we Velikovskians are—mutants, advancements, retards, throw-backs—we are up to the tasks at hand. Our numbers are not important; we are not the world, but there are more than *enough* of us to do the work. I close as I began, with the motto from Seneca: *Quota pars operis tanti nobis committitur?* Whatever part of it most needs doing.

* * * * *

HENRY H. BAUER AND IMMANUEL VELIKOVSKY,
By Charles Ginenthal

"He flatters himself on being a man without any prejudices; and this pretension itself is a very great prejudice."

Anatole France
The Crime of Sylvestre
Bonnard, Chapter 4

". . . there is no objective test of whether notions that contravene scientific ideas and theories are the work of a crank or a genius, nor whether such ideas will forever seem crazy or perhaps become the orthodoxy of the future."

Albert Einstein,
Scientific American
(September 1955), pp. 14-16

"Every new movement or manifestation of human activity, when unfamiliar to people's minds, is sure to be misrepresented and misunderstood."

Edward Carpenter
The Drama of Love and Death
Chapter 8, Note

". . . the inexcusable guilt is that they lied."

Lodovico Geymonat
(discussing Galileo's critics)
Galileo Galilei (New York, 1965)
transl. Stillman Drake, p. 120

In 1984, five years after the death of Immanuel Velikovsky, the University of Illinois published a long critical analysis of Velikovsky and his work by Henry H. Bauer, *Beyond Velikovsky The History of a Public Controversy*. According to the blurb on the book's back cover, Bauer occupies the chair of Dean of the College of

Arts and Sciences at Virginia Polytechnic Institute and State University. He has also held positions at the universities of Sydney (Australia), Michigan, Southampton (England) and Kentucky. An Austrian from Vienna, Bauer, a chemist, has published several books and innumerable articles especially during the 1980's on controversy within science, like that engendered by Velikovsky. Bauer sees himself as an exponent of science and Velikovsky as an exponent of pseudoscience while, oddly enough, he is also an advocate that the Loch Ness monster is not a pseudoscientific concept but is a valid scientific reality.

C. Leroy Ellenberger, a former editor of a journal devoted to Velikovsky's ideas, *KRONOS*, who has come to reject all of Velikovsky's theories, with his long association with this material, acted to correct Bauer's errors. Ellenberger claims the book is "Fair-minded and lucid, Bauer's study is a model of how such public disputes about science or technical issues can be understood and even resolved."⁴⁷⁹

Ellenberger further claims that, unlike Bauer's analysis, "Many scientists derisively attacked Velikovsky's theories about terrestrial catastrophes, biblical chronologies, and Earth's 'close encounters' with other planets. But they seriously undercut their case by resorting to innuendo, ridicule, misrepresentation [and] *ad hominem* arguments" ⁴⁸⁰ Therefore, one would expect Bauer's analysis to be "fair-minded" and not to employ "innuendo, ridicule, misrepresentation [and] *ad hominem* arguments."

Indeed, I have read other such evaluations of Bauer's criticism which make the very same claim, namely that Bauer's book appears to be quite honest and not touched by the sordid taint of misrepresentation and *ad hominem* arguments found in the many earlier critics of Velikovsky and his theories. According to C. Warren Hunt, a geologist, "The best comprehensive analysis of the [Velikovsky] 'Affair' is the book of Henry H. Bauer, *Beyond Velikovsky*."⁴⁸¹ Ev Cochrane, an editor of *AEON*, a Velikovskian type journal, states, "Throughout *Beyond Velikovsky* Bauer documents that Velikovsky occasionally misinterpreted or misrepresented his sources, neglected to properly acknowledge his predecessors . . . ; failed to adequately respond to criticism or admit his errors" ⁴⁸² Robert Jastrow, director of NASA's Institute for Space Studies, calls Bauer's book: "A very valuable contribution to the literature on a major controversy of great psychological and sociological importance."⁴⁸³ Marcello Truzzi, sociologist from Eastern Michigan University also suggests: "Bauer's remarkable book will be viewed, even by Velikovskians, as a great improvement over previous critical studies [of Velikovsky's theories]."⁴⁸⁴ Michael W. Friedlander, a professor of physics at Washington University in St. Louis, states, "I strongly recommend Henry Bauer's, *Beyond Velikovsky*" ⁴⁸⁵ Even Bauer himself suggests that his book offers "a means of stimulating recognition of the many inaccuracies, half-truths, unsupported assertions and misleading statements to which we all are constantly exposed."⁴⁸⁶ Here Bauer, like Carl Sagan, suggests his criticism, unlike that of his predecessors, will not contain inaccuracies, half-truths, unsupported assertions and misleading statements. Is all of this true? The only way to answer this question is to compare Bauer's statements with the evidence supporting or contradicting them. If Bauer, Ellenberger and the others are correct, then the evidence will clearly support Bauer's critical assertions. If, however, the evidence clearly contradicts Bauer's critical assertions, then one may correctly conclude that Bauer's book suffers from the very same sorts of innuendo, ridicule, misrepresentation, *ad hominem* arguments, inaccuracies, half-truths, unsupported assertions and misleading statements he so deplores in those critics who preceded him. This is the question I shall attempt to elucidate in a critical analysis of Bauer's *Beyond Velikovsky*.

A few years ago I received a letter from an individual who wrote, of the good and decent nature of Bauer's work and character. Around the same time I received a letter from Henry Bauer⁴⁸⁷ in which he apprised me of the

⁴⁷⁹Henry H. Bauer, *Beyond Velikovsky The History of a Public Controversy*, (Urbana, Ill., 1984), blurb inside cover [henceforth, *Beyond*].

⁴⁸⁰*Ibid.*

⁴⁸¹C. Warren Hunt, *Environment of Violence*, (Calgary, Alberta, 1990), p. 14.

⁴⁸²Ev Cochrane, "Beyond Bauer," *AEON*, Vol. II, No. 6, (May 1992), p. 114.

⁴⁸³*Beyond*, *loc. cit.*

⁴⁸⁴*Ibid.*

⁴⁸⁵Michael W. Friedlander, *At the Fringes of Science*, (Boulder, Colo., 1995), p. 6.

⁴⁸⁶*Ibid.*, p. xii.

⁴⁸⁷Personal communique from Henry H. Bauer to Charles Ginenthal, October 26, 1990.

fact that C. Leroy Ellenberger had sent him a copy of the announcement of my own, then privately-published book, *Carl Sagan and Immanuel Velikovsky*. He suggested that if I sent him a free copy of the book he would attempt to write a review of it for the *Journal of Scientific Exploration* or perhaps *The Skeptical Inquirer*, although he could make no promises about his review being published in either journal.

In order to be sure that I was not submitting my book to someone who would treat it dishonestly, I finally got a copy of Bauer's book and read it with some care. What I discovered, shocked me, and will be exposed in full below. My reading of Bauer's analysis made me extremely cynical about Bauer's probity with respect to Velikovsky's material. To make my position clear to Bauer I wrote to him the following response and sent copies of my analysis of a part of his criticism to him and circulated it to several other individuals. The letter reads as follows:

Dec. 5, 1990

Dear Mr. Bauer,

I have not sent you my book for review because I do not find your work respecting Velikovsky to be balanced in either its viewpoint or analysis. Instead it is unbalanced and I don't need the same applied to my own work which gives me reason enough to conclude you cannot act in a manner consonant with anything resembling dispassion when it comes to Velikovsky.

Yours truly,

Charles Ginenthal.

Enclosed with this letter was a brief paper analyzing Bauer's use of *ad hominem* which I circulated. Eventually that paper was corrected, polished and published in the journal, *The Velikovskian*. Now one may wonder why an author would reject the opportunity of having his book reviewed and perhaps made well-known. As Clark Whelton once told me, "Even a bad review is better than none at all." The following analysis I wrote exposes the canards presented above regarding the high-minded quality of Bauer's criticism. It was published as follows:

**"Pseudo-Scientists, Cranks, Crackpots
and Henry Bauer"**

"In *Beyond Velikovsky The History of a Public Controversy*, (Chicago, 1984),

p. 152, Henry Bauer, the author states, "Pseudo-scientist, crackpot, crank—these are pejorative terms. If we can show an idea to be wrong, why not leave it at that? Why insult the man who put forth the idea." This sounds quite balanced and is an admirable way of discussing scientific issues that are being debated. The only problem with this assertion is that Bauer does not follow this course of action at all in *his* discussion of Velikovsky. In fact, he pursues just the opposite course. On the very page in which he makes this declaration, Bauer states of Velikovsky, "He is, I believe a pseudoscientist." On page 173, he further remarks, "To my satisfaction, Velikovsky is a crank." So we have Bauer on the record calling Velikovsky by pejorative terms—pseudoscientist and crank.

In his book, Bauer's Chapter 8 is titled "Pseudo-Scientists, Cranks, Crackpots." What then of the pejorative terms "crackpot"; does he also employ this unsavory term? On page 125, Bauer quotes Velikovsky thus from *Worlds in Collision*, ". . . [I]nterplanetary electric discharges could also initiate atomic fissions with ensuing radioactivity and emission of heat."

Velikovsky has suggested that transmutation of one element into another can be produced by fusing induced by planetary electric discharges. This process is discussed in *Velikovsky Reconsidered*,⁴⁸⁸

"On January 28, 1945, I registered a lecture copyright titled "Transmutation of Oxygen into Sulfur." This was over six months before the fission (atom) bomb was dropped on Hiroshima and years before the fusion (thermonuclear process) was worked out. In my understanding . . . the phenomenon of brimstone (sulfur) falling from the sky (or filling the air) in the course of great discharges as narrated in ancient sources . . . resulted from smashing two oxygen atoms into one of sulfur."

In his book, page 125, Bauer comments: "Atomic fissions initiated by such electric discharges would be a new phenomenon." What does Bauer think of Velikovsky, who suggested a new phenomenon that would transmute elements by electric discharges that induced fusing? On pages 288-289 he tells exactly what he thinks: "One [Velikovsky] who persists in asserting that elements have been transmuted in other ways than those known to be effective would be classed as a *crackpot*." (Emphasis added)

Hence, Bauer has rounded out his chapter heading by calling Velikovsky by all three pejorative terms, "pseudo-scientist, crank, crackpot." We, therefore, observe that in complete disregard to his earlier laudable statement, Bauer simply cannot refrain from insulting the man who has put forward the idea by employing terms that he himself calls pejorative. However, when we return to page 152 of Bauer's book where we first began this discussion, he tells us, "I should like at this juncture to disclaim any intention to insult or derogate Velikovsky the man."

The logic at this point seems to be as follows: Crank, crackpot and pseudo-scientist are pejorative terms and it is better when discussing or debating scientific concepts to attack the idea—not the man—by avoiding the use of pejorative terms. Yet in total disregard of this injunction, Bauer calls Velikovsky crank, crackpot and pseudo-scientist, but argues that he has no intention of insulting or derogating Velikovsky, which is, nevertheless, precisely what he has done.

I leave this tiny insight into Henry Bauer's method of analysis for the reader, who may now examine Bauer's arguments regarding this matter in the light of his or her own understanding.

The author of this paper has drawn his own conclusion which one may consider from the evidence presented above. I agree with Bauer when he states on page 279 of *Beyond Velikovsky The History of a Public Controversy*, "*ad hominem* arguments—these are not to be excused"⁴⁸⁹

Leroy Ellenberger, in a private communique sent to me December 18, 1990, called this analysis "a masterpiece of muddled thinking, illogic, equivocation and non-sequitur."

Also, interestingly, in the Winter 1985-86 issue of *The Skeptical Inquirer*, Bauer takes Martin Gardner to task stating: "I value logic and clarity of thought and appreciate them [But] Gardner's column [criticizing Velikovsky] illustrates some of the debunking tactics I criticize notably . . . comments '*ad hominem*'."⁴⁹⁰

I find it extremely fascinating that Bauer can chide Martin Gardner for employing pejorative *ad hominem* which he himself employed in his own book. Even Gardner whom Bauer berated for using this method was in no way fooled by Bauer's pretense of eschewing the use of *ad hominem*. Gardner in a turn-about describes Bauer's book in the following manner:

⁴⁸⁸Pocketbook ed., (New York, 1977), p. 247; Doubleday ed., p. 207.

⁴⁸⁹Charles Ginenthal, "Pseudo-Scientists, Cranks, Crackpots and Henry Bauer," *The Velikovskian*, Vol. I, No. 1, (New York, 1993), pp. 107-108.

⁴⁹⁰Henry Bauer, "Different Approaches?" *The Skeptical Inquirer*, Vol. 10, (Winter 1985-1986), pp. 186-187.

"The thoughts above [respecting debunking of pseudoscience] circulated through my brain while I was reading *Beyond Velikovsky*, by Henry H. Bauer 'Many scientists derisively attacked Velikovsky's theories . . . ,' says the book's jacket. 'But they seriously undercut their case by resorting to innuendo, ridicule, misrepresentation, *ad hominem* arguments' In brief, they were debunkers of the worst sort. Because these attackers of V (as I shall henceforth refer to Velikovsky) include such distinguished scientists as Carl Sagan and [Stephen Jay] Gould, science writers like myself . . . and other friends I expected to find a careful dispassionate evaluation of everybody involved in the V controversy. To my surprise, I found Bauer writing exactly as he accuses debunkers of doing.

"Some examples: V was 'an ignoramus masquerading as a sage' (p. 94) There is warrant to describe him as a pseudo-scientist' (p. 133). 'V is an arch-dogmatist' (p. 122)

"In plain language V was paranoid. 'He misrepresents theories and facts extant at the time he wrote—and not because he had not read about them, for he quotes from authoritative sources: either he did not understand, or he deliberately misrepresented He uses the jargon of science as though he understands it. His tone is that of one who is discussing subjects with which he is familiar, yet a close look at what he says he is not competent to carry on such discussion.' (p. 121) Bauer is here commenting on V's little-known *Cosmos Without Gravitation*, in which he defends the view that gravity is electromagnetic. On the basis of this book alone, [actually it is more of a pamphlet], Bauer writes, 'I would not hesitate to characterize the author as a crank or a charlatan.'⁴⁹¹

Can anyone believe that calling Velikovsky crank, crackpot, pseudoscientist, ignoramus, arch dogmatist, charlatan, *etc.* is not a carefully calculated smear and *ad hominem* attack and also not an attempt to derogate Velikovsky the man? I think at this point even the casual reader will see through this charade of Bauer's objectivity. However, in order to fully determine whether or not Bauer employs honest and appropriate methods of criticism one must examine his statements of evidence. It is specifically in the detailed exposure of the statements Bauer employs with respect to the evidence and Velikovsky which will separate Bauer from dishonest critics or make him merely another celebrated member of the Velikovsky debunking club.

Very sadly, it is here I must show that Bauer is not one iota different than Gardner, Sagan and the others he so censures because, as the reader will see, Bauer does clearly and demonstrably misrepresent Velikovsky and his work by citing statements out of context with all the other forms of fraudulent criticism.

Misrepresentation and Quoting Out of Context

Bauer has maintained that his calling Velikovsky pejorative names is based on knowledge. The justification for his critical remarks is found in the following statement.

"I found it no easy matter to reach an opinion about Velikovsky. I hold one now not because [critics of Velikovsky like] Gardner . . . , or anyone else said so but because of my own examination of most of the material about Velikovsky and by him."⁴⁹²

⁴⁹¹Martin Gardner, "Welcome to the Debunking Club," *The New Age*, (Buffalo, N.Y., 1991), pp. 65-66.

⁴⁹²*Beyond Velikovsky, op. cit.*, p. 138.

In essence, Bauer claims he has made an in-depth study of Velikovsky's work and found that Velikovsky "does not know what he is talking about."

Leroy Ellenberger, who condones Bauer's labeling Velikovsky by pejorative terms claimed in his communique, I "fail to distinguish between bad name calling and labeling deduced from specific examples I recommend you read Bauer's Chapter 7." Therefore, let us examine some examples in Chapter 7.

In Chapter 7, "Velikovsky's Physical Science," subchapter heading "Red Herrings and Straw Men," Bauer states:

"Velikovsky does not hesitate to use the specialists' jargon and the tone of authority, but his statements about methods, facts, and laws of science cover a range from correct, through almost correct, all the way to quite wrong. As a result the layman is virtually bound to be *misled*. Consider the following . . . [written by Velikovsky] (Emphasis added)

" . . . mutations . . . a process of spontaneous changes in living nature fundamentally different from the process of evolution . . . postulated by Darwin' [Bauer continues] . . . Darwin did not concern himself with genetic mutations because knowledge of genetics came much later than Darwin. But modern biologists see mutations as providing the variability (or some of it) upon which Darwin's natural selection acts to produce evolution. Mutation and natural selection go hand in hand in biological science, and it is quite *misleading* for Velikovsky to write as though mutation and evolution were in some way contradictory."⁴⁹³ (Emphasis added)

The reader is first invited to examine the "tone of authority" in Bauer's statement. It is quite apparent that he speaks with what is clearly an absolute assurance on this point. Secondly, he tells the reader that Velikovsky has *misled* the layman and, in particular, in this instance, Velikovsky's discussion of Darwin and mutation theory are "quite misleading." However, if we go back to *Earth in Upheaval* cited by Bauer, to the very page he quotes, we will discover that Velikovsky clearly stated that the theory of mutation came well after *Charles Darwin's death in 1882*. What Velikovsky actually stated is:

"And in the field of the origin of species, *in 1900* Van [sic] Vries announced mutation in plants, ***observed for the first time*** a process of spontaneous changes in living nature fundamentally different from the process of evolution through continuity as postulated by Darwin."⁴⁹⁴ (Emphasis and bold added.)

Bauer simply quoted Velikovsky out of context and assiduously omitted the cogent parts of Velikovsky's sentence in which Velikovsky specifically showed that the theory of mutation of species "***observed for the first time***" came "***in 1900***" after Darwin's death, in 1882, to attack Velikovsky as though he had never mentioned this time element at all. And yet Bauer accuses Velikovsky of using statements that are "quite wrong" or "quite misleading." When Velikovsky wrote that "in 1900 Van [sic] Vries announced mutation in plants for the first time," he made it quite clear that Darwin could not possibly have known about something discovered "for the first time," such as mutations, since he had been dead for eighteen years! When Bauer omitted this introductory statement from Velikovsky's citation he altered the meaning Velikovsky sought to convey to the reader. By his omission of these specific parts of this statement Bauer, in fact, changed the citation to make it suggest a totally different meaning than Velikovsky had presented. In fact, Bauer's omission of the 1900 date for the discovery of mutations is a direct misrepresentation of what Velikovsky wrote in full! In fact, Velikovsky fully explained that Darwin knew nothing of genetics. In *Earth in Upheaval*, page 250, he wrote, "Mendel's [genetic] work [was] unknown to Darwin and his followers." Why did Bauer omit this fact?! Why did he misrepresent the evidence?!

⁴⁹³ *Ibid.*, p. 129.

⁴⁹⁴ Immanuel Velikovsky, *Earth in Upheaval*, (New York: Doubleday, 1955), p. 271.

Furthermore, Bauer has with a tone of authority stated "Mutation and natural selection go hand in hand in biological science, and it is quite misleading for Velikovsky to write as though mutation and evolution were in some way contradictory."⁴⁹⁵

But contrary to Bauer's statement, Velikovsky never rejected mutations as a mechanism of evolution. Velikovsky has his own theory of evolution presented in the very book Bauer is criticizing, *Earth in Upheaval*. In fact, Velikovsky's chapter, "Cataclysmic Evolution," has a subchapter titled "**Mutations and New Species**." What Velikovsky claimed is that the mechanism of mutation was speeded up by catastrophes. That is, Velikovsky's theory of "cataclysmic evolution" is directly based on massive mutations. Again from *Earth in Upheaval*, Bauer omitted from his discussion on this point what Velikovsky actually wrote about "mutation and evolution."

"The theory of evolution is vindicated by catastrophic events in the earth's past; the proclaimed enemy of this theory proved to be its only ally. The real enemy of the theory of evolution is the teaching of uniformity

"Great catastrophes of the past accompanied by electrical discharges and followed by *radioactivity could have produced sudden and multiple mutations of the kind achieved today by experiments but on an immense scale*. The past of mankind, and of the animal and plant kingdoms too, must now be viewed in the light of the experience of Hiroshima and no longer from the portholes of the *Beagle*." (Emphasis added)⁴⁹⁶

So it is clear that Velikovsky stated that mutation theory was part of evolutionary theory contrary to Bauer's criticism. What he rejected was the gradualist pace that modern biologists who advocate Darwinian natural selection propose. The question is: Why did Bauer say Velikovsky ever suggested "mutation and evolution were in some way contradictory" when he never suggested, in any way, anything of the kind? And who then has been misleading whom?! Who has set up a strawman and presented red herrings?!

As the reader can see, I have avoided calling Bauer names of any kind. But Bauer has loosed an avalanche of personal abuse on Velikovsky. Where evidence is lacking, misrepresentation and personal insults are substituted.

Bauer also claims Velikovsky's "statements about methods, facts and laws of science cover a range from correct, through almost correct all the way to quite wrong."⁴⁹⁷ Let us, therefore, examine Bauer's criticism that "mutation and natural selection go hand in hand in biological science, and it is quite misleading . . . to write as though mutation and evolution were in some way contradictory."⁴⁹⁸ What do many of the biological authorities say about this assertion? Does Bauer know what he is talking about on this point?

Velikovsky, in his book *Earth in Upheaval*, discussed what the earlier authorities said with regard to mutations and evolution *via* natural selection. He cited V. L. Kellogg of Stanford University, who claimed in 1907 that natural selection does not seem to give support to evolutionary theory because "mutations seem to be too few and far between"⁴⁹⁹ Kellogg is shown to have also admitted that one group of scientists "denies in toto any effectiveness or capacity for species forming on the part of natural selection, while the other group, a larger . . . sees in natural selection an evolutionary factor capable of initiating nothing . . . but capable of extinguishing all unadaptable unfit lines of development."⁵⁰⁰ Does this statement by Kellogg suggest that evolution and natural selection go hand in hand as Bauer stated, and was Kellogg being misleading for denying the efficacy of this process? Velikovsky further cited H. Fairfield Osborn, a leading American evolutionist who wrote in 1917, "In fact,

⁴⁹⁵*Beyond, loc. cit.*

⁴⁹⁶*Earth in Upheaval, op. cit., p. 259.*

⁴⁹⁷*Beyond, loc. cit.*

⁴⁹⁸*Ibid.*

⁴⁹⁹*Earth in Upheaval, op. cit., p. 251.*

⁵⁰⁰*Ibid.*

the causes of the evolution of life are as mysterious as the law of evolution is certain."⁵⁰¹ Velikovsky cited William Bateson, a leading English evolutionist who stated in 1921, "When students of other sciences ask us what is now currently believed about the origin of species we have no clear answer to give."⁵⁰² He also cites L. T. More who said in 1925 about the nature of mutation as developed by DeVries: "The idea is destructive to scientific theory, as it really does away with the whole idea of [gradual Darwinian] continuity which should be the basis of evolution theory"⁵⁰³ More also stated, "If natural selection is a force which can destroy but cannot create species and if the reasons for this destruction are unknown, of what value is the theory to mankind? . . . The collapse of the theory of natural selection leaves the philosophy of mechanistic materialism in a sorry plight."⁵⁰⁴

What Bauer omitted from his criticism of Velikovsky on this point is that Velikovsky analyzed this concept of mutations and natural selection *via* Darwinian evolutionary theory and showed that after mutation evidence had developed, a "group of scientists denied in toto any effectiveness or capacity for species forming on the part of natural selection." He presented leading evolutionists who also denied the potency of this mechanism. But Bauer says Velikovsky is misleading us because *he knows* "mutation and natural selection go hand in hand." Were all these scientists also being misleading for not going along with Bauer's pronouncement?

Let us, furthermore, remember that Velikovsky wrote this book in 1955, while Bauer wrote his criticism in the 1980's and published his results in 1984. During this 34 year period between the publication of these books, what had become of the concepts of evolution *via* mutation, and natural selection Bauer is proposing? Had the concept of mutation, natural selection and evolution changed from what Velikovsky had presented? What will be disclosed below is that a great many modern biologists, evolutionists, geneticists and other scientists during this period came to the same conclusion, namely that mutations and natural selection cannot be the driving mechanism of evolution as Bauer has so authoritatively stated. Nearly all of the following citations were in wide circulation and extant long before Bauer wrote his criticism of Velikovsky regarding the question of whether or not mutation and natural selection go hand in hand in evolutionary theory. What do they tell us?

Professor, Sir Ernst Chain, a Nobel Prize laureate for his research into the curative properties of penicillin and world renowned biologist, said in 1970, fourteen years prior to Bauer's statement,

"To postulate that the development and survival of the fittest is entirely a consequence of chance mutations seems to me a hypothesis based on no evidence and irreconcilable with the facts. These classical evolutionary theories are a gross over-simplification of an immensely complex and intricate mass of facts, and it amazes me that they are swallowed so uncritically and readily, and for so long a time, by so many scientists without a murmur of protest."⁵⁰⁵

Was Chain misleading us on this issue, as Bauer claims, for not swallowing Bauer's viewpoint so uncritically?

According to Pierre P. Grasse, one of the world's leading biologists and evolutionists, mutations are

"merely heredity fluctuations around a medium position; a swing to the right, a swing to the left, but no final evolutionary effect . . . they modify what pre-exists."⁵⁰⁶ [Grasse goes on to state]
"Some contemporary biologists, as soon as they observe a mutation, talk about evolution.' This

⁵⁰¹*Ibid.*

⁵⁰²*Ibid*, p. 252.

⁵⁰³*Ibid.*

⁵⁰⁴*Ibid.*

⁵⁰⁵Francis Hitching, *The Neck of the Giraffe Where Darwin Went Wrong*, (New York, 1982), p. 82.

⁵⁰⁶Jeremy Rifkin, *Algeny*, (New York, 1983), p. 133.

conclusion, says Grasse, 'does not agree with the facts. No matter how numerous they may be, mutations do not produce any kind of evolution.'⁵⁰⁷

Poor world renowned biologist Grasse is also, in Bauer's terms, misleading his readers with such a gross contradiction to Bauer's mutation-evolution authoritative statement.

French biologist Jean Rostand states,

"No, decidedly, I cannot make myself think that these 'slips' [mutations] of heredity have been able, even with the cooperation of natural selection, even with the advantage of the immense periods of time in which evolution works on life, to build the entire world with its structural prodigality and refinement, its astounding 'adaptations.'⁵⁰⁸

Perhaps this French biologist ought to go back to school, say to Virginia Polytechnic Institute where Bauer teaches, and Bauer can excoriate him for presenting such a misleading statement.

C. H. Waddington, an internationally respected geneticist stated with respect to mutation:

"This is really the theory that if you start with any fourteen lines of coherent English and change it one letter at a time, keeping only those things that still make sense, you will eventually finish up with one of the sonnets of Shakespeare . . . it strikes me as a lunatic sort of logic, and I think we should be able to do better.⁵⁰⁹ [Elsewhere he states]

"The development of an animal under the influence of its genes is obviously an intricate and carefully controlled process which normally leads to a rather standard and invariant end result. A mutation of one or even several of the genes concerned can either disrupt the process completely, so that the animal dies, or it can produce effects only of a limited character . . . [W]ith a developing biological organism mutation is anything but omnipotent to produce change in any conceivable direction."⁵¹⁰

In fact, Waddington wrote in 1952, three years prior to Velikovsky's *Earth in Upheaval*, "To suppose that evolution . . . has depended only on a [natural] selection out of a haphazard set of variations [mutations] each produced by blind chance, is like suggesting that if we went on throwing bricks together into heaps, we should eventually be able to choose ourselves the most desirable house."⁵¹¹

Of course, we needn't listen to this international authority who contradicts Bauer's authoritative statement because Bauer is the real authority on evolution and Waddington is being misleading.

Professor John Moore has declared: "Upon rigorous examination and analysis, any dogmatic assertion . . . that gene mutations are the raw material for any evolutionary process involving natural selection is an utterance of myth."⁵¹²

⁵⁰⁷*Ibid.*, pp. 135-136.

⁵⁰⁸Jean Rostand, *The Orion Book of Evolution*, (New York, 1961), p. 79.

⁵⁰⁹C. H. Waddington, "Evolution," *Science Today*, (1961), p. 38.

⁵¹⁰E. Jantsch, C. Waddington, (eds.), *Evolution and Consciousness*, (Reading, Mass., 1976), p. 13.

⁵¹¹C. H. Waddington, in *The Listener* (February 13, 1952) in A. Koestler's, *The Ghost in the Machine*, (New York, 1967) p. 127.

⁵¹²John N. Moore, *On Chromosomes, Mutation and Phylogeny*, (December 27, 1971), p. 5.

Apparently, Professor Moore also requires instruction from Bauer on being misleading. How dare Moore suggest Bauer's pronouncement is "an utterance of myth."

Theodosius Dobzhansky, another world respected geneticist, apparently misleadingly said that a mutation was "An accident, a random change, in any delicate mechanism can hardly be expected to improve it. Poking a stick into the machinery of one's watch or one's radio set will seldom make it work better."⁵¹³

To sum up, the biologists' arguments on this point, F. B. Salisbury, in "Natural Selection and the Complexity of the Gene," *Nature* for October 25, 1969, page 343 states,

"In the evolution of life on Earth, we are dealing with millions of different life forms, each based on many genes. Yet the mutational mechanism as presently imagined could fall short by hundreds of orders of magnitude of producing, in a mere four billion years, even a single gene."

Now according to Bauer, Sir Ernst Chain has mislead everyone because he states that evolution based on "chance mutation seems to me a hypothesis based on no evidence and irreconcilable with the facts," contradicting Bauer who claims mutation and evolution by natural selection are not contradictory. Pierre P. Grasse is also being misleading when he states "mutations do not produce any kind of evolution," because Bauer apparently knows better. C. H. Waddington is also misleading his readers by offering "mutation is anything but omnipotent to produce change in any conceivable direction." Perhaps he should have a good talk with Bauer who will tell him he doesn't know what he is talking about. Professor John Moore must also be misleading the public by claiming that "any dogmatic assertion . . . that gene mutations are the raw material for any evolutionary process involving natural selection is an utterance of myth." Bauer is, therefore, quite undogmatic for asserting in contradiction that "mutation and natural selection go hand in hand." All these authorities, based on Bauer's statement on this matter, must have missed the boat because Bauer, without a single citation to support his assertion, knows that they are all wrong and are being misleading. All these authorities, according to Bauer, don't know what they are talking about, but he does! And Leroy Ellenberger suggests Bauer was justified in calling Velikovsky pejorative names based upon just such evidence.

Contrary to Bauer, all of these biologists do know what they are talking about, and in no uncertain terms say that mutations, natural selection and evolution do not go hand in hand as Bauer has so authoritatively told us. And this comes from the chapter in which Bauer is going to show that "Velikovsky does not know what he is talking about." But Bauer, who is completely oblivious to the debate among scientists on these crucial aspects of evolution, is going to tell us what he knows!

Not only do biologists, evolutionists and geneticists reject Bauer's views on the efficacy of mutations, natural selection and evolution, but so do many, many other scientists in other fields of science. Francis Hitching writes with respect to these others:

"In 1966 there was an inconclusive and often ill-tempered two-day symposium at the Wistar Institute of Anatomy and Biology in the University of Pennsylvania entitled 'Mathematical Challenges to the Neo-Darwinian Interpretation of Evolution.' Here it became clear that doubts among biologists were doubled and redoubled by physicists, mathematicians and engineers some of whom were openly incredulous at the *lack* of a testable scientific basis for evolutionary theory. (Few biologists expressed any uncertainty on this occasion about natural selection being the supreme explanatory law, prompting a delegate from the other side to remark, 'If I wanted to be nasty to the evolutionists, I would say that they are surer of themselves than we nuclear physicists are—and that's quite a lot.')

"Computer scientists, especially, were baffled as to how random mutations alone could possibly enrich the library of genetic information? A mutation, they repeatedly pointed out, is a mistake—the equivalent of a copying error. And how could copying mistakes build up into a body of complicated and ordered information? Murrery Eden, Professor of Engineering at

⁵¹³Theodosius Dobzhansky, *Heredity and the Nature of Man*, (New York, 1964), p. 126.

Massachusetts Institute of Technology, said that in plain language what the biologists were proposing went as follows:

"The chance emergence of man is like the probability of typing at random a meaningful library of one thousand volumes using the following procedure: Begin with a meaningful phrase, retype it with a few mistakes, make it longer by adding letters; then examine the result to see if the new phrase is meaningful. Repeat this process until the library is complete.

"He concluded this was so implausible that 'an adequate scientific theory of evolution must await the discovery and elucidation of new natural laws—physical, physico-chemical and biological'.

"Marcel P. Schutzenberger, a computer scientist from the University of Paris, agreed that spontaneous improvement and enlargement of the code through mutations and natural selection was 'not conceivable'.

'In fact, if we try to simulate such a situation by making changes randomly at the typographic letter (by letter or by blocks, the size of the unit does not really matter) on computer programmes, we find that we have no chance (that is, less than one chance in $10^{1,000}$) even to see what the modified programme would compute: it just jams'.

"And he summed up. 'We believe that there is a considerable gap in the neo-Darwinian theory of evolution, and we believe this gap to be of such a nature that it cannot be bridged within the current concepts of biology.'

"Repeatedly, the impasse that Darwinians reach is one with a sign saying there is no useful road ahead without a map showing the overall pattern of evolution; random mutations—at least sequential mutations of one-after-one Darwin kind—lead to yet more dead ends."⁵¹⁴

Arthur Koestler sums up the evidence thus regarding mutation and natural selection in evolution:

"The totalitarian claim of the neo-Darwinists that evolution is 'nothing but' chance mutation plus [natural] selection has, I think, been finally defeated, and a decade or two from now biologists—and philosophers—may well wonder what sort of benightedness it was that held their elders in its thrall."⁵¹⁵

With dogmatic ignorance of these scientists' findings that contradict Bauer's claim, one can well understand that only ignorant dogmatism allows his view of the nature of evolution to continue being presented.

From Bauer's viewpoint all these other scientists are also being utterly misleading because they have the gall to disagree with Bauer's authoritative statement regarding evolution *via* mutation and natural selection. And they too don't know what they are talking about based on Bauer's knowledge of what he as a real scientist and academic knows. I hope Bauer's real understanding of science and of scientific criticism is becoming clearer to the reader. I also hope that the reader will now understand my great reluctance to have my own work subjected to analysis of its scientific content by such an individual with his special insights into science and Velikovsky's writings. If Bauer had only taken even a little time to examine the literature regarding these matters, instead of speaking out on a subject so well documented he might have saved himself this criticism.

What I am suggesting is that far too much of Bauer's criticism is fraught with the same kind of scholarship or better—lack of scholarship. For example, he talks about natural selection as if this concept is a fact of science when in reality it is nothing of the kind as Jeremy Rifkin points out.

⁵¹⁴Hitching, *op. cit.*, pp. 82-83.

⁵¹⁵Koestler, *The Case of the Midwife Toad, op. cit.*, p. 129.

"The fact is, natural selection does indeed explain everything and nothing at the same time. It is pure tautology. It is instructive to see just exactly how the embarrassing state of affairs came to be.

"As the many contradictions to natural selection . . . began to surface over the years, its proponents continued to qualify the theory so as to incorporate and defuse the accumulating criticism. Each time natural selection was attacked from a new perspective, its supporters were forced to modify further the basic premise, until all that was left was a mathematical formulation of sorts. Sir Julian Huxley sums up the synthetic or neo-Darwinist conception of natural selection in the following terms: 'The struggle for existence merely signifies that a portion of each generation is bound to die before it can reproduce itself.'

"Not until Nobel prizewinning geneticist T. H. Morgan began to suspect that natural selection was a victim of circular reasoning did anyone in the scientific community even question what was regarded as a profound truth. Morgan looked at the definition of natural selection carefully worked out by the neo-Darwinists and then wrote that 'it may appear little more than a truism to state that the individuals that are the best adapted to survive have a better chance of surviving than those not so well adapted to survive.' Or, as Gertrude Himmelfarb puts it, 'The survivors, having survived, are thence judged to be the fittest.' Morgan's observation shocked the scientific establishment. It was like proclaiming to the whole world that 'the emperor has no clothes.' While Morgan helped focus attention on what was regarded as an unthinkable suggestion, a succession of critics have since that time taken turns at disrobing the proposition, until all that is left of natural selection today is a naked transparent tautology. C. H. Waddington, one of the greatest biologists of the twentieth century, committed the final act of desacralization when he wrote in a clear and uncompromising fashion the following statement:

'To speak of an animal as 'fittest' does not necessarily imply that it is strongest or most healthy . . . Essentially it denotes nothing more than leaving more offspring. The general principle of natural selection, in fact, merely amounts to the statement that the individuals which leave most offspring are those which leave most offspring. It is a tautology.'

"For years, then, scientists had been running around the same circle, faster and faster, until a few like Morgan and Waddington, began to realize they weren't going anywhere."⁵¹⁶

Bauer talks about natural selection and mutation going along hand in hand in evolution with an absolute sense of authority. But it has been known for a considerable time that natural selection is exactly what Bauer's criticism amounts to. It is insufferable ignorance masquerading as scientific criticism.

But even worse than that, there was a full discussion of the matter that natural selection is a tautology in the journal *KRONOS*, that Bauer claims to have read, since it is among his "References" on page 333. How could he have read this material and then act as if it does not exist? In *KRONOS*, Vol. VII, No. 4, (Summer 1982), pages 5-7 and pages 33-37, is a full discussion of this tautology. And to make matters even worse, Leroy Ellenberger, who advised Bauer by reading his book to remove errors, either out of sheer malice or willful dishonesty, failed to tell Bauer that he had written one of these articles titled "Editorial Postscript" about the tautological nature involved in the concept of natural selection. R. H. Brady, who carefully outlined this tautology in an article on this topic, is cited by Ellenberger, as is Tom Bethell, who also discusses this material in that same issue of *KRONOS*. Why, then, did Ellenberger allow Bauer to go forward with this concept when he had strongly attacked it? On page 6, Ellenberger then describes the concept of evolution *via* mutations and Mendelian genetics thus: "The Modern Synthesis (the Synthetic Theory), or Neo-Darwinism, is the view of evolutionary mechanisms that emerged between 1920 and 1950 from the integration of Mendelian genetics into evolutionary biology." That is, Ellenberger also understood that the concept of mutation *via* Mendelian genetic theory is an essential part of evolutionary theory. However, Ellenberger on page 7 goes on to claim, "Neo-Darwinism is a 'moribund explanation of how evolution occurs' shot through with more holes than Bonnie and Clyde's last ride." Ellenberger, therefore, shows that mutations, *vis-à-vis* genetic theory—mutations and natural selection—do not go hand in hand in evolutionary theory.

⁵¹⁶Jeremy Rifkin, *Algeny*, (New York, 1983), pp. 143-144.

Ellenberger, with all this, was perfectly willing to allow Bauer to raise these fraudulent criticisms. Ellenberger has claimed that I "fail to distinguish between bad name calling and labeling deduced from specific examples" What Ellenberger has failed to do is correct misrepresentations and misinformation presented by Bauer in Bauer's Chapter 7. On the basis of Ellenberger's claim he, too, qualifies to be called all the labels Bauer has placed on Velikovsky. But again, I will not stoop to that level of name calling and will allow the reader to decide what terms fit Ellenberger's total indifference to the evidence regarding "natural selection" and "mutation" going hand in hand in evolution. My only comment now is that Bauer's and Ellenberger's *behavior* on this matter is completely dishonest!

And I have specifically chosen books and materials written before Bauer's criticism appeared to show that he simply does not know what *he* is talking about with an absolute tone of authority. Numerous and highly regarded scientists are in total disagreement with Bauer's pronouncement. They vociferously reject natural selection and mutation theory as the mechanisms of evolution. Although Velikovsky had much earlier accepted mutation theory on a massive scale in an extremely short time frame, the mutation mechanism is not universally nor overwhelmingly accepted to go hand in hand by great authorities in the field or by other scientists in other fields. It is not Velikovsky who has misled his readers but Bauer regarding the extant evidence of the time he wrote. Not only has Bauer misrepresented Velikovsky's statements on Darwin, and mutations and natural selection, but he appears to be completely oblivious of the statements of many modern biologists on these mechanisms yet acts as if his word is correct and authoritative when, in fact, it is neither. But now, more of Bauer's Chapter 7.

Velikovsky, toward the latter part of his book, *Earth in Upheaval*, discussed the historical development of evolutionary theory. Bauer nevertheless, presents the following criticism in his Chapter 7:

"Further, he [Velikovsky] is wrong in ascribing 'evolution' to Darwin —many evolutionists preceded Darwin, whose notable contribution was to provide the idea for a mechanism that of natural selection through which the process of evolution could be understood."⁵¹⁷

Again the reader is requested to note in Bauer's statement the tone of authority of correcting Velikovsky and everyone else. But Bauer has once again omitted and misrepresented what Velikovsky specifically wrote about *these other evolutionists who preceded Darwin in the very book he is criticizing*. In *Earth in Upheaval*, pages 233-234, here is what Velikovsky wrote about some of the evolutionists who came before Darwin and also Darwin's unique contribution to the theory.

"The Theory of Evolution dates back to the age of classic Greece, one of its proponents having been Anaximander, and from time to time philosophers have offered the evolutionary explanation of the origin of the multiple forms of life on Earth, as opposed to the theory of special creation or the permanency of living forms from the day of Creation. Lamarck (1744- 1829) thought that acquired characteristics were transmissible by heredity and thus might lead to the appearance of new forms of life. In 1840 . . . an anonymously printed work *Vestiges of Creation*—written by Robert Chambers —caused a stir that did not subside for years. It was bitterly attacked by every British scientist for teaching that human beings are 'the children of apes and the breeders of monsters' . . . Darwin later acknowledged that the brunt of the attack against his own theory was absorbed by *Vestiges*.

"*What was new in Darwin's teaching was not the principle of evolution in general, but the explanation of its mechanism of natural selection*. This was an adaptation to biology of the Malthusian theory about population growing more quickly than the means of existence. Darwin acknowledged his debt to Malthus, whose book he read in 1838. Herbert Spencer and Alfred R. Wallace independently came to the same views as Darwin and the expression 'survival of the fittest' was Spencer's." (Emphasis added)

⁵¹⁷*Beyond, loc. cit.*

Velikovsky also wrote about the earlier contributions of Georges Cuvier in the same chapter; he distinctly pointed out these other evolutionists who preceded Darwin. And in the very same terms Bauer employed, *i.e.*, "Darwin, whose notable contribution was to provide the idea for a mechanism of natural selection through which the process of evolution could be understood."⁵¹⁸ Velikovsky said, "What was new in Darwin's teaching was not the principal of evolution . . . but . . . the explanation of its mechanism . . . natural selection."⁵¹⁹ Why did Bauer fail to inform his readers that Velikovsky, in fact, presented exactly this evidence of Darwin's precursors and that he did not ascribe "evolution only to Darwin"? One would naturally think that Bauer's information about Darwin not inventing evolution and merely contributing the mechanism of it was omitted by Velikovsky when it was not. This is once again sheer misrepresentation! Why did Bauer do this? Why didn't Ellenberger correct this?

There can be no doubt that Bauer read *Earth in Upheaval*, because he cited it verbatim. Thus we are left with two options by which to explain Bauer's behavior. Either he did not really read Velikovsky's book but skimmed it here and there to pick out points to criticize which is totally unscholarly and dishonest, or he read the book and then ignored what Velikovsky actually stated to fabricate and misrepresent the man whom he claims he does not wish to derogate. And all of this in Bauer's view is being employed to correct the scientific record with respect to Velikovsky. This is Bauer's form of in-depth analysis to show how he derived his firm opinion of Velikovsky and his writings.

Significantly, in another chapter of his book, Bauer goes on to state that Velikovsky's "critics made fools of themselves in a number of ways."⁵²⁰ How, one may ask, did they do that? To which Bauer replies: "By making sloppy . . . mistakes . . . misquoting Velikovsky . . . criticizing points on which established authorities agree with Velikovsky . . . and misrepresenting Velikovsky's argument."⁵²¹ But Bauer is guilty here precisely of making sloppy mistakes, misquoting Velikovsky, criticizing points on which established authorities agree with Velikovsky, and misrepresenting Velikovsky's argument. But I will not now stoop to Bauer's level of criticism to call him by pejorative terms. I will let the readers draw their own conclusions about his criticism.

How, then, can Bauer's criticism, containing such flagrant, outrageous misrepresentations of Velikovsky, be what he earlier claimed it to be, a "means of stimulating recognition of many inaccuracies and half-truths, unsupported by citations and assertions and misleading statements"; when in this short excursion into his analysis we can find so many distortions, inaccuracies, half-truths, unsupported assertions and misleading statements?! Bauer's self-serving claim about the in-depth accurate nature of his criticism is really no different than his claim that he does not wish to denigrate or derogate Velikovsky, the man. His statements on both points, upon investigation prove to be hypocrisy! To suggest as does Ellenberger that Bauer's criticism is "fair minded" or without "innuendo, ridicule, misrepresentation [or] *ad hominem* arguments" by an editor of a Velikovskian journal, is to be blind, incompetent or constitutionally incapable of understanding the tenets of ethical criticism! If, as Ellenberger claims, Chapter 7 is a basis for calling Velikovsky "bad names," it is also the basis for doing the same to Bauer and Ellenberger, whom Ellenberger carefully edited and advised on what Velikovsky wrote. Didn't Ellenberger know these aspects of Velikovsky's writings? Why didn't he correct them? How could he not know this material?

And for the second time I must cordially agree with Bauer when he states: "Quite inexcusable were the innumerable occasions on which critics of Velikovsky misquoted and misrepresented him It seems incredible that salient points of Velikovsky's scenario should not be known to those who comment on those specific points, yet that continues to be the case."⁵²² Finally, I add my own "amen" to Bauer's statement: "I find in the record that the critics were themselves guilty of things for which they castigated Velikovsky and his supporters."⁵²³ But irony of ironies, Bauer calls his chapter 13, "Blundering Critics," and so I quote to Bauer the very citation he presents on page 209 from St. Matthew:

⁵¹⁸*Beyond, loc. cit.*

⁵¹⁹*Earth in Upheaval, op. cit.*, p. 234.

⁵²⁰*Beyond, op. cit.*, p. 213.

⁵²¹*Ibid.*, p. 214.

⁵²²*Ibid.*, p. 224.

⁵²³*Ibid.*, p. 223.

"Why beholdest thou the mote that is in thy brother's eye, but considerest not the beam that is in thine own eye?"⁵²⁴

Bauer and his supporters, such as Ellenberger, may now shout "foul" because my condemnation of his criticism is based on only a few of his statements and doesn't truly reflect the full breadth of his work on Velikovsky. But I would then ask them, was it "fair" or "foul" for Bauer to condemn Velikovsky based mainly on only one small paper written many years ago? But I will not do as Bauer did. I will continue to show that his criticisms overall suffers from these same misrepresentations and distortions again and again and again that were just outlined. Let us proceed with more of Chapter 7.

Bauer, in order to show that Velikovsky was presenting half-truths, quotes Velikovsky who stated:

"W. H. McCrea . . . came to the conclusion that . . . no planet could have been formed inside the Jovian [Jupiter] orbit . . ." [Bauer then states] McCrea's argument,⁵²⁵ [is] based on the Roche limit (closest approach of two bodies under gravitational influence without disintegration), is that no planet having a density equal to, or less than that of Jupiter could have formed inside the Jovian orbit. Planets of greater density could—and all the inner planets in fact do have densities greater by a factor of about four than that of Jupiter . . . The manner in which Velikovsky quotes McCrea is *quite misleading*, [emphasis added] since it does not make clear that the question of density is crucial. The effect of Velikovsky's reference is to imply to the reader that the existence of planets inside the Jovian orbit is a mystery in the light of conventional theories, whereas it is not."⁵²⁶

Once again Bauer calls Velikovsky "quite misleading" as he did with respect to mutations, this time regarding his citation of McCrea's 1960 paper, in the light of conventional theories. McCrea was distinctly analyzing the roche limit in terms of conventional theories for the formation of the terrestrial planets, (Mercury, Venus, Earth, Moon and Mars) and indeed, density is crucial in this discussion. The conventional planetary formation theories McCrea was analyzing namely the nebula and tidal theories and/or derivations of them, contrary to Bauer's assertion, all begin with the terrestrial planets condensing gravitationally from **clouds of gas and dust**, not from solid bodies.

As Peter Cattermole states:

"Currently available geochemical and physical data for Venus, the Earth, Moon and Mars is consistent with their having been accreted from the same primordial nebular material. *Mercury is by way of an exception*. [Emphasis added]

"Despite fundamental differences between the two models [nebula and tidal theories] there is some ground which provides a basis for general discussion. Thus it is generally agreed (though not universally accepted) that all of the Sun's planets had their origin in an interstellar cloud of dust and gas."⁵²⁷

⁵²⁴*Ibid.*, p. 209.

⁵²⁵W. H. McCrea, *Proceedings of the Royal Society, A*, Vol. 256, (1960), p. 245.

⁵²⁶*Beyond*, p. 130.

⁵²⁷Peter Cattermole, *Venus The Geological Story*, (Baltimore, 1994), p. 12.

Clouds of gas and dust, Bauer should know, do not have greater density than Jupiter. McCrea distinctly described the process of Sun and planet formation thus: "We envisage a body of interstellar matter to be called a cloud . . ."⁵²⁸ which formed the solar system. After the sun had largely formed, McCrea then states, "there remain[ed] about 1,000 floccules [clouds] circulating around it."⁵²⁹ It is from these clouds or floccules that the planets formed or condensed. McCrea is quite specific that these clouds with extremely low density could not condense into planets out to "about the distance of Jupiter from the Sun."⁵³⁰ In order for these planets to form into terrestrial bodies they had to come into their present orbits as fully-formed solid bodies or from solids; since the planetesimals condensed from clouds, McCrea claimed they could not do so out to the orbit of Jupiter. This is precisely the argument McCrea was making, and Velikovsky was absolutely correct, that this evidence contradicts conventional theory. Bauer has reached into a magician's hat full of gas and dust and pulled out fully formed solid planets as criticism.

The real question is: Did Bauer know that the planets formed from condensation of dust and gas clouds but ignored this, as he has McCrea's analysis, based on planets forming from such clouds? He most certainly did, because he unwittingly allowed this statement to slip out. ". . . there is yet no agreed mechanism for the formation of the solar system. Some form of CONDENSATION FROM A CLOUD OF GAS containing vortices seems to be plausible, but the implications of the idea have not been fully worked out."⁵³¹ (Capitalization added)

Therefore, it is clear Bauer had to have known all along that McCrea was analyzing planetary formation from dust and gas clouds, not from solid bodies, which McCrea said will not condense out "to about the orbit of Jupiter." Speak of half-truths! It was Benjamin Franklin, I believe, who said, "half truths are often very great lies."

Bauer elsewhere (not Chapter 7) goes on to reinforce this point regarding the density of the terrestrial planets:

"Further, in other ways Venus is *not* anomalous. For example the densities of the planets are known. All the terrestrial ones (Mars, Earth, Venus, Mercury) have a density of about 5 grams per cubic centimeter (between 4 and 5.8); the others (Saturn, Jupiter, Neptune, Uranus) have a much smaller density between 0.7 and 1.6 grams per cubic centimeter. Apparently then, the terrestrial planets were all formed in a similar fashion or at least from the same primordial material, and this was not the same material as that from which the outer planets were formed. If Venus came out of Jupiter, how does one explain these groupings by density?"⁵³²

Bauer claims that the density of the terrestrial planets are so similar that this implies that Velikovsky, who took his view of Venus' creation from Lyttleton as fissioning of the solid terrestrial planets from the silicate core of the planet Jupiter, cannot be supported. But in fact, by comparing the densities of the terrestrial planets we were able to cite Cattermole above, who claimed "Mercury is by way an exception" to this concept. In addition, geophysicist T. F. Gaskell had also reached the conclusion that the densities of the terrestrial planets are much too different to be accounted for by dust- cloud condensation and accretion. He also advocates the fission process, but from the terrestrial and not the Jovian planets. He states that, "since the density of Mars—about 4—is substantially lower than the Earth's density of 5.5, those who assert that Mars is a fundamental planet [a planet born at the same time and manner as the other planets] cannot at the same time uphold the common dust-cloud origin of all planetary matter."⁵³³ In addition, Hannes Alfvén, a Nobel prize winner, and Gustav Arrhenius, in 1976 wrote:

⁵²⁸W. H. McCrea, *op. cit.*, p. 248.

⁵²⁹*Ibid.*, p. 249.

⁵³⁰*Ibid.*, p. 261.

⁵³¹*Beyond, op. cit.*, p. 88.

⁵³²*Ibid.*, p. 89.

⁵³³T. F. Gaskell, *Physics of the Earth*, (New York, 1970), p. 24.

"A consequence of the Laplacian model would be that the planetary masses obey a simple function of the solar distance; however, this conclusion is so obviously in disagreement with observations that this aspect has been avoided [no discussion of planetary mass with distance from the Sun makes sense.]

*"To reconstruct the distributed density of the solar system, some rather arbitrary assumptions must be made."*⁵³⁴ (Emphasis added)

What these distinguished scientists are saying is that the masses of the planets and their densities, as related to their distance from the sun, makes no sense and that to explain the densities of the planets in terms of the condensation concepts as Bauer puts forth, requires some "rather arbitrary assumptions." But Bauer never even mentions this and seems totally oblivious to this. Furthermore, McCrea made a careful analysis of the densities of the terrestrial bodies and also came to exactly the opposite conclusion Bauer has suggested on terrestrial planetary density.

In 1969 McCrea presented a paper in *Nature*, specifically titled, "Densities of the Terrestrial Planets," to show that neither the nebular nor tidal cloud condensation hypotheses explain the density of these planets. Bauer is simply discussing this aspect of scientific theory in exactly the same manner that he discussed mutation and evolutionary theory, ignorant of the relevant literature presented about this concept of that time. McCrea maintained that the fission process, Velikovsky borrowed from Lyttleton, explained the densities of the terrestrial planets much better than the conventional cloud condensation concepts. And most importantly **from a scientific requirement for rigor**, McCrea based his analysis on a quantitative basis, not on a qualitative one. After analyzing the densities of the terrestrial bodies McCrea stated:

"This shows that as these [terrestrial] planets exist now, there is no simple pattern in their densities as associated with their masses. Neither is there any clear trend in the densities as associated with their distances from the Sun. Now the varieties of densities demonstrates that there are wide differences in the chemical composition of these bodies. This would be astonishing were the bodies formed separately out of the same raw [cloud] material (in the absence of a correlation with mass, that might arise from different rates of loss of certain constituents, or of a correlation with distance from the Sun, that might result from a sorting out of constituents of the raw materials). The difficulty is elegantly resolved if the planets as they exist now were in fact produced by the break-up of two bodies having identical overall chemical composition. This is a compelling argument in support of this process of formation.

"I recently pointed out that the densities of the Earth, Moon and Mars are apparently understandable if these bodies resulted from the break-up [fission] of a single rotationally unstable planet . . . [whose] material had undergone some segregation. Here the original planet was considered to become unstable as a whole and then to break-up in accordance with Lyttleton's discussion. In the M[ercury] V[enus] case, on the other hand, if the core of the original planet became rotationally unstable, before the body as a whole became unstable, then Lyttleton's ideas would have to be applied to the core and not to the whole body

"The main point is that, in order to start with a common supply of raw material [as the nebular and tidal cloud condensation theories demand] and to finish up with several [terrestrial] planets possessed of a variety of masses and densities showing no simple correlation among themselves, we know no alternative . . . [except] that . . . [parent] bodies broke up in various ways to yield the existing planets. The only mechanism we know for such a break-up is rotational instability [*i.e.*, fission]."⁵³⁵

⁵³⁴Hannes Alfvén, Gustav Arrhenius, *Evolution of the Solar System*, (NASA SP 345), (Washington, D.C., 1976), pp. 25-28.

⁵³⁵W. H. McCrea, "Density of the Terrestrial Planets," *Nature*, Vol. 224, (1969), pp. 28-29.

In essence, McCrea, by using a rigorous analysis of the densities of the terrestrial planets by their masses and distances from the sun, showed in complete contradiction to Bauer's statement, "If Venus came out of Jupiter how does one explain these groupings?" [And] "The existence of planets inside the Jovian orbit is a mystery . . . whereas it is not" that these planets were not formed inside the orbit of Jupiter by any form of condensation from clouds of similar raw material. McCrea then rigorously proved that their densities precluded this. He showed that the densities of the terrestrial planets only made scientific sense if they were fissioned from other planets. But Bauer is oblivious to this because he has not done any research of this material; yet he then makes unsubstantiated, authoritative statements on precisely these concepts he has not investigated and for which he is wrong. How do clouds of gas and dust which have practically no density condense inside the roche limit of the early Sun to form solid bodies?

This only illustrates how Bauer indulges in a biased selection, or more accurately, non-selection of concepts and source materials. As has been and will be shown again and again this is not an isolated case; this approach to concepts and source materials permeates his criticism. Evidence, no matter how well-known or easily available, is either ignored or not sought out if it does not aid his criticism or what Bauer desires to prove.

To illustrate this, let us examine Bauer's statement that the terrestrial planets inside the Jovian orbit, according to Velikovsky, is a mystery in the light of conventional theory, while according to Bauer, "it is not." The simplest source on this matter, the *Encyclopedia Britannica*, states about planetary formation theory: "It should be emphasized that no theory of the origin of the solar system [which includes the condensation of gas and dust cloud theory] has yet won general acceptance. All involve highly improbably assumptions. But the difficulty is in trying to find a theory with any degree of probability at all."⁵³⁶ Does this description of planetary formation sound as though the formation of even the terrestrial planets is not a mystery, as Bauer proclaims? Bauer's argument on this point is simply science by proclamation divorced from the scientific literature. Since it was known even in the *Britannica* that all theories of solar system formation lack "any degree of probability at all," how can Bauer authoritatively claim that formation of the terrestrial planets *via* the condensation theory, or "any other theory," is not a mystery? And this basic knowledge of what is understood was available to Bauer in the simplest source, an authoritative encyclopedia!

Bauer, on a different point, criticizes Velikovsky's *interpretation* of the written documentation (not in his Chapter 7).

"Throughout his writings Velikovsky dogmatically asserts the correctness of interpretations that are at best possible. Here are some examples from *Ages in Chaos*:

"On p. 45 the Ermitage Papyrus is said to relate [Velikovsky said] 'the same story . . . that we now know from the Papyrus Ipuwer, but in a different way . . . as things that are to come. Obviously this indicates only a preference for the literary form of foretelling.' The papyrus tells of a perished land, a veiled sun, a dry river, and Velikovsky assures us that 'we can recognize' this 'description of the changes in nature . . . belonging to the period when the Israelites roamed in the desert, under a cloudy sky' But this description is surely applicable to any time of drought and dust storms."⁵³⁷

Bauer has only drawn this conclusion by precisely omitting the rest of the materials that Velikovsky cited on those pages and also in *Worlds in Collision* with respect to both the Ermitage Papyrus and the Ipuwer Papyrus, as these documents relate to Velikovsky's interpretation of them, which decidedly does not refer "to any time of drought and dust storms." In *Worlds in Collision*, Velikovsky discusses both documents:

"In the Papyrus Ipuwer it is similarly stated that 'the land turns round [over] as does a potter's wheel' and the 'Earth turned upside down.' This papyrus bewails the terrible devastations wrought by the upheaval of nature. IN THE ERMITAGE PAPYRUS . . . also, reference is made

⁵³⁶*Encyclopedia Britannica, Macropedia*, Vol. 16, (London, Eng., 1982), p. 1032.

⁵³⁷*Beyond, op. cit.*, p. 158.

to a catastrophe that turned the *'land upside down; happens that which never (yet) had happened.'*"⁵³⁸ (Capitalization and emphasis added)

Now, is the land turned upside down common to any period of drought or sandstorms? It would be forgivable if Bauer just happened to have not read this material in the other book, *Worlds in Collision*, instead of the book he is citing, *Ages in Chaos*, but the very same material is also presented on the pages he is describing from *Ages in Chaos*, which I now quote, so the reader will see Bauer's omission of this material.

"In this [Ermitage] papyrus the same story is related that we now know from the Papyrus Ipuwer, but in a different way. The upheavals of nature . . . [One paragraph below this one Velikovskiy cites the Ermitage Papyrus]

"The land is utterly perished and nought remains . . . The river is dry (even the river) of Egypt . . . This land shall be in perturbation . . ." "I SHOW THEE THE LAND UPSIDE DOWN, HAPPENED THAT WHICH NEVER (YET) HAD HAPPENED . . ." ⁵³⁹ (Capitalization added)

By again omitting direct statements that Velikovskiy wrote on the very pages he is describing about the Earth turning upside down, Bauer first misrepresents the evidence then claims it is Velikovskiy who has misinterpreted these documents. It is rather obvious that the world does not turn upside down during any drought or sandstorm. And the ancient writer clearly said this event "*happened which never (yet) had happened*" which again could never refer to any ordinary drought or sandstorms since these kinds of events (sandstorms) are quite common in that desert region of the Earth. The real question is: Why did Bauer omit this evidence from his criticism? I think the reader is fast becoming aware of what Bauer is really up to.

With respect to Venus, Bauer states:

"In referring to his successful advanced claims, Velikovskiy said that he had included in them that Venus has . . . 'abnormal (disturbed) rotation.'⁵⁴⁰ Such claims are not found in *Worlds in Collision*."⁵⁴¹

What Bauer has omitted, once again, is that Velikovskiy in *KRONOS*, Vol. IV, No. 3, pages 66-67 cited the specific pages in *Worlds in Collision* as evidence of his claim about the possible erratic orbital parameters that would be induced by a close planetary interaction of Venus on a cometary orbit with the Earth or other large celestial bodies as is found in *Worlds in Collision*. On page 156 of *Worlds in Collision*, Velikovskiy specifically states,

"A planet [Earth] turns [rotates] and revolves on a quite circular orbit around a great body the sun; it makes contact with another body, a comet [Venus] that travels on a stretched out ellipse. The planet slips from its axis, runs into disorder off its orbit, wanders rather erratically, and in the end is freed from the embrace of the comet.

"The body on the long ellipse experiences similar disturbances." (Emphasis added)

⁵³⁸I. Velikovskiy, *Worlds in Collision*, (New York, 1950), p. 107.

⁵³⁹Immanuel Velikovskiy, *Ages in Chaos*, (New York, 1952), pp. 45-46)

⁵⁴⁰"Venus and Hydrocarbons," *Pensée*, Vol. 6, pp. 21-23, and "My Challenge to Conventional Views in Science," *Pensée*, Vol. 7, pp. 10-14.

⁵⁴¹*Beyond, op. cit.*, pp. 168-169.

What are these orbital disturbances that Velikovsky described? Velikovsky clearly claimed that in the "close contact with another large body" a planet "slips from its axis [and] runs in disorder off its orbit." He claimed this could happen to both bodies, and his section of the book deals specifically with the interaction of Venus and Earth during a near collision. As was pointed out above, both the Ipuwer and Ermitage papyri claimed the Earth, after the catastrophe with Venus, had turned *upside down*. As proof of the Earth turning upside down, Velikovsky devoted a chapter to *Worlds in Collision* titled "East and West" in which he cites historical and legendary evidence that the rotation of the Earth was upside down or reversed from normal rotation: Citing Herodotus and then many others along the same lines, Velikovsky wrote on page 105:

"The priests asserted that within historical ages and since Egypt became a kingdom, "four times in this period (so they told me) the sun rose contrary to his wont; twice he rose where he now sets, and twice he set where he now rises."

Here Velikovsky made it quite clear that one of these orbital changes was that of axial rotation caused by the catastrophic close interaction between the Earth and Venus. Not only did Velikovsky write in *Worlds in Collision* that the Earth reversed its rotation, but there he specifically stated, "The body on the long ellipse [Venus] experiences similar disturbances." Even Patrick Moore describes Venus' rotation with the very same words used in the Ipuwer and Ermitage Papyri, namely "upside-down." "Why" he asks, "should Venus spin in what might be called an upside-down direction?"⁵⁴² Here we have both planets, Venus and, in ancient times Earth, said to have rotation described as upside down **which means reversed rotation**.

But Bauer does not want to accept any of this apparently because, in my opinion, as his misrepresentations of Velikovsky show, he has an anti-Velikovsky agenda and any evidence that supports Velikovsky's claims may be omitted or misrepresented. As Bauer finally states, "As regards Venus, Velikovsky's advanced claims in *Worlds in Collision* did *not* include a statement that Venus rotates in retrograde fashion; he quoted extant estimates for the rate of rotation without giving any inkling of a suspicion that the sense of the rotation might be unusual."⁵⁴³ How could Bauer be so ignorant of what Velikovsky specifically wrote about the possible rotation of Venus in *Worlds in Collision* when the evidence was there right before his eyes?! And how could Ellenberger have not known this when he was an editor of *KRONOS* where Velikovsky specifically pointed to the pages in *Worlds in Collision* which contained this information?!

But let us get back to "misleading" statements by Bauer as he continues.

"Moreover, it is in itself misleading to refer to the rotation [of Venus] as "disturbed"—no evidence has been presented that Venus's motion ever was other than it now is; the observed retrograde motion simply means that Venus rotates in the opposite direction from most of the other planets and their satellites."⁵⁴⁴

This is the fourth time Bauer calls Velikovsky "misleading." He did so first with mutation and evolutionary theory. He did so a second time with the accusation that it was misleading to write of Darwin as the first evolutionist when others preceded him. And then he did so a third time with McCrea's paper on terrestrial planetary formation from clouds. Now he again raises the ugly specter of "misleading" on Velikovsky's part for suggesting Venus' rotation was "disturbed."

Again, what do astronomers, scientists and science writers say with respect to whether or not Venus' retrograde rotation appears to be "disturbed." Do they share Bauer's evident conviction that this motion is not "disturbed" and to suggest so is "misleading"? This I was able to answer fully in only about two hours by perusing

⁵⁴²Patrick Moore, *The Unfolding Universe*, (New York, 1982), p. 52.

⁵⁴³*Beyond, op. cit.*, p. 235.

⁵⁴⁴*Beyond, loc. cit.*

some of the astronomy and science books in my own library. It is a great pity that Bauer did not see fit to do this simple research at his university library.

James E. Oberg, who is extremely critical of Velikovsky, has suggested the unthinkable or "misleading" regarding Venus' retrograde rotation. "Explanations for the anomalous rotation of Venus . . . have been typified by desperation. Perhaps Venus was hit by a giant asteroid."⁵⁴⁵ In direct contradiction to Bauer's assertion, Oberg puts forth the suggestion that an asteroid "disturbed" Venus' rotation. Oberg, apparently is "misleading" us, according to Bauer.

William R. Corliss, another well respected science writer, also claims the absurd notion that Venus' rotation is anomalous and shows that scientists have proposed various explanations to explain what "disturbed" its original rotation.

"Anomaly Evaluation. If Venus was formed like the rest of the planets, it should have originally possessed appreciable prograde spin. Its current slow retrograde spin implies: (1) A different origin; or (2) A dramatic solar system event that despun the planet."⁵⁴⁶

Poor Corliss is obviously "misleading" his readers and perhaps should have spoken with Henry Bauer who will correct his thinking on this point because, in that section of his book, Corliss cites several scientists who say essentially the same thing.⁵⁴⁷

Irwin I. Shapiro, a professor of geophysics and physics at M.I.T. states with respect to Venus' retrograde rotation that the possibility has been presented. "Venus once collided with an asteroid with a diameter of about 200 kilometers, a cataclysm that could reverse the planets rotation . . . into a retrograde one near the present value."⁵⁴⁸ This time a highly respected scientist says that Venus' rotation may be interpreted as having been "disturbed" by an immense cataclysmic impact that reversed its spin. Is Shapiro "misleading" the public by presenting this concept as Bauer authoritatively claims?

Eric Burgess states,

"A major mystery about Venus is why the planet should rotate so slowly and in a direction different from the other terrestrial planets. One possibility is that Venus encountered another astronomical body either in a close approach or an actual impact. One theory suggests that Venus may have had a satellite moving in a retrograde orbit which decayed and struck the planet, thus slowing its rotation. Or perhaps a body from outside the Solar System, or one of the big planetesimals from the outer Solar System, hurtled through the inner Solar System and gravitationally altered the rotation period of Venus, and its direction."⁵⁴⁹

To be sure, Burgess must clearly be "misleading" his readers with these suggestions. But will an internationally respected science journal like *Science*, the journal of the American Association for the Advancement of Science, allow its pages to be used to present the "misleading" concept that Venus' rotation was disturbed after being struck by a natural satellite or several? Clearly its editors, based on Bauer's statement, would have been out of their minds to allow this. But they did, indeed, permit two well respected scientist's, Bevan M. French and S. F. Singer, to publish what can only be regarded as a "misleading" paper that Venus was "disturbed" when either an ancient Moon or Moons, in retrograde orbit around it, crashed into its surface causing the planet's direction of spin

⁵⁴⁵James E. Oberg, *New Earths*, (Harrisburg, 1981), p. 201.

⁵⁴⁶William R. Corliss, *The Moon and the Planets*, (Glen Arm, Md., 1985), p. 303.

⁵⁴⁷*Ibid.*

⁵⁴⁸Irwin I. Shapiro, "Radar Observation of the Planets," *Frontiers in Astronomy*, (San Francisco, 1970), p. 45.

⁵⁴⁹Eric Burgess, *Venus An Errant Twin*, (New York, 1985), p. 132.

to reverse.⁵⁵⁰ If only they had had the opportunity to discuss Bauer's understanding of this matter, they might have saved themselves much grief for misleading their colleagues of the scientific world.

Zdenek Kopal an internationally renowned astronomer on this question, states,

"The first problem concerns the rotation of the planet. What made Venus rotate so slowly and what tilted its axis almost *upside down* to give rise to its retrograde rotation. [Emphasis added] The only probable mechanism would be a very close encounter with another celestial body whose gravitational attraction played havoc with Venus and altered its kinematics beyond recognition."⁵⁵¹

In this case we have yet another astronomer being, as Bauer says, "misleading." Finally, science writer Ben Bova comes out with the unthinkable statement that Venus' rotation was "disturbed" by using the forbidden word "disturbed."

"The rotation of Venus is an even bigger headache to astronomers Venus rotates backward, in the retrograde direction, opposite the rotation of every other planet in the solar system. [save Uranus]

"This anomaly profoundly disturbs astronomers. It is not that they are fussy; they simply find it hard to explain why the Sun and all the other planets could revolve in one direction, and Venus in the other. *If the solar system was created all at the same time, then Venus should have started out moving in the same direction as everything else. **SOMETHING DISTURBED VENUS***, something powerful enough—or violent enough—to 'flip' the planet's direction of rotation. The energy involved in reversing the spin of some 4.5 billion trillion tons of planet is staggering to contemplate" [Capitalization and bold added]⁵⁵²

Were all these astronomers, scientists and science writers being "misleading" as Bauer so clearly proclaims, or is Bauer the one again "misleading" his readers?

With respect to the object that "disturbed" Venus' spin, Hoimar Von Ditfurth has suggested that, on the basis of the *near couple* of Venus' rotation with the Earth at inferior conjunction, (when Earth and Venus line up on the same side of the sun, Venus *almost* precisely shows the same hemisphere to the Earth. It is not a precise resonance.) "[t]he earth must once have exerted a braking or decelerating effect on Venus."⁵⁵³ In this instance, Von Ditfurth is "misleading" us that the Earth and Venus in the past had a braking interaction just as Velikovsky claimed!

There are other papers, books and journals that repeat this evidence over and over. But Bauer is oblivious to all this. I again in every citation above, except that of Eric Burgess, chose references that were published well before Bauer wrote his criticism of Velikovsky. There *is* something "disturbed" in all this, but it is not the suggestion made by Immanuel Velikovsky that Venus' rotation has been disturbed. In this case one is not "misled" to wonder what standards of evidence Bauer is employing. He should have taken even a little time to read the literature on this matter before he burst forth with such an egregious, mistaken statement.

For the third time I must agree with Bauer when he states:

⁵⁵⁰Bevan M. French, S. F. Singer, "How Did Venus Lose its Angular Momentum?" *Science*, Vol. 173, (1971), p. 169.

⁵⁵¹Zdenek Kopal, *The Realm of the Universe*, (New York, 1979), p. 191.

⁵⁵²Ben Bova, *The New Astronomers*, (New York, 1972), p. 163.

⁵⁵³Hoimar Von Ditfurth, *Children of the Universe*, (New York, 1976), p. 115.

"When a man makes a fool of himself by discussing, at ponderous length, a subject of which he is ignorant, I tend to assume that he will act similarly in other fields and at other times—an unwarranted assumption but one that is commonly made."⁵⁵⁴

When Bauer claims Velikovsky is "misleading," an alarm ought to go off to alert the reader that Bauer may not have read the literature on that topic and may himself be guilty of that which he accuses Velikovsky.

Bauer, who is himself an advocate that the Loch Ness monster is a genuine scientific phenomenon, has taken umbrage when sleazy tactics were employed against this phenomenon. He states,

"Further, when an author lumps together Velikovsky and the Loch Ness monster . . . that author's credibility is lowered in my estimation. Just as from personal inquiry I believe Velikovsky to be a pseudo-scientist, so also on the basis of my personal inquiries I believe in the existence of Loch Ness monsters ('Nessies') and of sea serpents. In Loch Ness there is a breeding population of large aquatic animals with powerful flippers, long thin necks, and bulky humped bodies, animals not as yet known to 'science.' I believe that because I myself have examined the evidence of eyewitnesses, and photographs, of sonar observations. So someone who lumps my Nessies together with the case of Velikovsky loses credibility in my eyes; it indicates to me that he probably takes his opinions at second hand or after only cursory reading. And when someone classes Velikovsky (or someone else) as a crank, I expect that writer or critic to have his facts straight"⁵⁵⁵

But it becomes rather clear that Bauer's personal inquiries into Velikovsky indicates that not only does he lack credibility based on his misrepresentations, misquotations, and *ad hominem*s, but it shows that as a writer and a critic, he does not value logic and clarity and appreciate them, and because he does not have his facts straight he loses credibility in my eyes. Misrepresenting Velikovsky's or anyone else's statements by any scholar is the most unforgivable of scholarly sins. Doing so again and again amounts to a smear campaign, especially when it is accompanied by the use of pejoratives based on misrepresentations. Bauer is clearly aware of what such propagandist techniques can accomplish. He tells us,

"There was pronounced resistance by some of the older generation against the new approaches, theories, and discoveries. [Einstein's] Relativity was strongly resisted, as were at least certain implications and interpretations of the new quantum physics or wave mechanics. Indeed, the Nazi attack on 'Jewish' science was instigated by some physicists of the old school: unable to win their battle by reason and evidence within their discipline, they enlisted the aid of government, seizing on the happenstance that some of the best physicists of modern view were Jews."⁵⁵⁶

The very essence of name calling added to misrepresentation in a hostile environment where there is trust of authorities, be they scientists and academics, be it 1930's Germany with a tradition of antisemitism, or be it in the United States where science is *the authority* and greatly respected, is, and never was other than, a smear campaign. Why did Bauer understanding the implications of this behavior do these things?

For one thing, it is well-known that accusations containing pejoratives and misrepresentations which will rarely be investigated by the average reader can do great damage to a person's theories, especially when presented in the reputable, academic press in which Bauer presented his critique. Reporters and public relations people know all

⁵⁵⁴ *Beyond, op. cit.*, p. 94.

⁵⁵⁵ *Beyond, op. cit.*, pp. 138-139.

⁵⁵⁶ Henry H. Bauer (B), *The Enigma of Loch Ness*, (Chicago, 1988), pp. 33-34.

too well that an accusation gets headlines and attention, and that the taint of dishonesty in others may never go away even if later findings show the accusation to be without merit. Haven't we all heard after a sensational trial when a well-known person accused of a crime is acquitted, some say "he got away with murder." If one wishes to make Velikovsky and his ideas and his advocates as unwelcome and distasteful to the educated as some disaffected scientists in Germany wanted to create a negative attitude against Einstein's Jewish science then misrepresentations and pejoratives are an excellent propaganda tool. No matter what the intellectual or philosophical pretense stated as the *modus vivendi* the method at bottom is still to smear, defame and discredit, not to examine fairly or justly.

There is a second aspect, particularly related to the use of pejoratives. Ultimately, one must ask why would a scholar, scientist and academician like Bauer resort to the use of such low language? *Name calling is at bottom, to my view, nothing less than intellectual violence.* Martin Buber, the humanist philosopher, in his book *I and Thou*, suggests that the employment of denigrating terms in describing others and their work is always an attempt to destroy that person by dehumanization. Once Bauer has pinned the labels "crank, crackpot, pseudoscientist, arch dogmatist, ignoramus and charlatan" on Velikovsky based on his many misrepresentations, the vision of the reader of this material becomes prejudiced and tainted. That reader now knows that Velikovsky and his ideas no longer require reasoned understanding since they are the work of an alleged misfit. Such criticism is merely a personal attack disguised as scientific criticism. It is a form of guilt by association, the same as saying on any point of Velikovsky's theory that since "Velikovsky does not know what he is talking about, say with regard to Darwin not being the first to suggest the theory of evolution, as Bauer presented, we needn't take anything he says seriously." We can say "Velikovsky the crackpot believes the Earth was nearly destroyed by a comet 3,500 years ago," or "Velikovsky the arch dogmatist and charlatan has written a crazy book on ancient myths, which he suggests have a basis in reality."

But Bauer then turns around and says he does not wish to have his views of the Loch Ness monster treated in the same way—guilt by association.

"Some subjects are so widely regarded as crackpot that any given phenomenon or idea can be discredited simply by associating it with those typically cranking pursuits. Fringe subjects are commonly found guilty by association in this way, whether or not any evidence is adduced for the association. So Nessie has suffered from the very beginning through linkage with sea serpents, a quite logical and necessary association. Nessie has also been discredited by unsubstantiated smear and gratuitous association" ⁵⁵⁷ with Velikovsky.

What Bauer has done after misrepresenting and classifying Velikovsky as a pseudoscientist, *etc.*, to his own personal satisfaction, claims it is wrong for others to discredit Nessies by unsubstantiated smear and gratuitous association to their own personal satisfaction. However, as Professor Wolfe will demonstrate in the following essay, Bauer repeatedly plays the game of guilt by association with Velikovsky. What standards is Bauer basing this argument on when we have shown that so much of his criticism of Velikovsky is unsubstantiated smear? What this suggests is that Bauer has no standards or two sets of standards, one for Velikovsky and quite another for Nessie.

This brings us finally to the evidence Bauer has presented that he claims discredits Velikovsky from *Cosmos Without Gravitation*. As Martin Gardner pointed out earlier, this is only one of Velikovsky's papers published in 1946. In this early paper Velikovsky attempted to show that gravity is electromagnetic and that certain phenomena are not explained by gravity. This paper had been in large measure dropped by Velikovsky. This fact was written but not published by Professor Lynn E. Rose in one of Velikovsky's later manuscripts, *Before the Day Breaks*. Professor Rose sent me this citation from that manuscript.

"It should be noted that during the last two decades or so of Velikovsky's life, the ideas that had been expressed in *Cosmos Without Gravitation* and in the fourth of the "Four Plans of the Universe" no longer reflected Velikovsky's approach to the extent that they once did. In particular, he backed off considerably from the idea of circumduction as a non-gravitational, non-inertial account sufficient in itself to explain orbital motions, and he also backed off from any

⁵⁵⁷*The Enigma of Loch Ness, op. cit.*, pp. 95-96.

general suggestions that gravity and inertia might somehow be banned from the celestial arena. What he often said in his later years was, not that gravity and inertia played no role, but rather that they did not play the *only* role, that is, that gravity and inertia were not alone responsible for what occurred in the celestial arena: electromagnetic interactions also played a considerable role in cosmic events—especially when celestial bodies were in close approach to each other, but also even when they were far apart.

"The Space Age brought the myriads of artificial satellites that orbit Earth on different planes and in different directions. Clearly Earth with its magnetic field does not 'circumduct' these artificial satellites around itself in a common plane, nor is it able to control their direction of movement. In such cases, some of the lesser variations and perturbations might still be attributed to electromagnetic factors, but gravity and inertia would remain the principal determining factors. It was no doubt such considerations as these that led Velikovsky to change his stance here. In any case, each of "The Four Plans of the Universe" even the fourth, should be taken as a "construction," a working hypothesis for the purposes of discussion, not as a final position. The same is true of *Cosmos Without Gravitation*: not all of the ideas that were formulated in that early monograph were ones that Velikovsky continued to adhere to in his later work.

"Nevertheless, it must be emphasized that Velikovsky did not ever abandon the idea that gravity itself might eventually be interpreted as an electromagnetic phenomenon, nor did he ever abandon the idea that the solar magnetic field might to some extent be responsible for the fact that the planetary orbits are roughly co-planar and for the fact that all nine of the major planets, and apparently all of the minor planets as well, orbit the Sun in a counterclockwise direction. —[Lynn E. Rose]." All this was written before Bauer produced *Beyond Velikovsky*, though he was not aware of it.

Velikovsky's withdrawal from his position on *Cosmos Without Gravitation* was generally known among those of his followers like Rose and Ransom in the late 1970's who were studying the ideas of Velikovsky. That is, during the 1960's and 1970's a reevaluation of this material led Velikovsky to draw away from much of this work. Bauer would have us believe that because Fredric Jueneman's work in 1973 and Ralph E. Juergens' work in 1972, which cite some of this material, proves Velikovsky still adhered, in the latter part of the decade, to all these ideas, is untrue. But this is based on the assumption Velikovsky would never alter his mind about earlier work. It seems clear Bauer has not seen Rose's statement.

I am not arguing in any way that Bauer should not have criticized this work, but I decidedly submit it should not have been made the *centerpiece* of his book. The reason for this has once again to do with standards; that is, with Bauer's own standards, not Velikovsky's.

In his book *The Enigma of Loch Ness*, Bauer has a chapter titled, "What is Relevant" in which he presents what he considers the distinction between worthy or relevant evidence for the Loch Ness monster as opposed to evidence which is unworthy or irrelevant. Bauer argues, "But for the monster hunter the only relevant evidence is the photographic, the sonar, and those eyewitness reports that have not been explicable as cases of mistaken identity or deceit."⁵⁵⁸ What Bauer wishes to do is separate "The Wheat And The Chaff."⁵⁵⁹ That is, Bauer wants those interested in the evidence as to the reality of Nessies to dismiss and ignore evidence known to be dishonest, questionable, irrelevant or unworthy of his thesis. What he wants is that the *questionable* photographs, *questionable* sonar soundings and *questionable* eyewitness accounts be disregarded *in favor of the strongest unquestionable evidence* of photographs, sonar soundings and eyewitness accounts. That is a very reasonable and valid standard.

However, this is not the standard Bauer has applied to Velikovsky. Whereas he wishes Nessie to be evaluated on the most secure evidence presented, he is, nevertheless, ready to evaluate and criticize Velikovsky's entire planetary thesis on one of his least known, least secure, most questionable of papers. Elsewhere in his book Bauer explains how to distinguish worthy criticism from that which is unworthy by discussing Maurice Burton's book, *The Elusive Monster*. He states, "Although Burton's actions and his book are sad, easy to interpret

⁵⁵⁸Henry H. Bauer, *The Enigma of Loch Ness*, *op. cit.*, pp. 41-42.

⁵⁵⁹*Ibid.*, p. 58.

uncharitably. The book is misleading, misquotes and misinterprets . . . Maurice Burton surely exemplifies the dilemmas facing the professional who thinks Loch Ness is worthy of interest."⁵⁶⁰ If this criteria applies to Burton who is misleading, misquotes and misinterprets, using Bauer's own criteria this surely applies to Bauer's book which has amply been shown to mislead, misquote and misinterpret!

However, I will still examine Bauer's criticism of *Cosmos Without Gravitation* not to attempt to prove that Velikovsky is correct—since Velikovsky has rejected much of that work—but to examine how Bauer, himself, handles this evidence. The following analysis of Bauer's critique, therefore, should not, nor cannot be construed as a defense of all of Velikovsky's assertions in *Cosmos Without Gravitation* since this work has been largely disavowed by Velikovsky and by his followers by the late 1970's. The only question I shall appropriately examine is the manner in which Bauer uses scientific evidence to criticize. As Bauer states of Velikovsky:

" . . . I had to search for other ways to reach a judgement [about Velikovsky]. In particular, how sound is his competence in the fields that enter into his discussion—in plain language, does he know what he is talking about? In *Cosmos Without Gravitation* I found clear proof that, in the physical sciences, Velikovsky does not know what he is talking about."⁵⁶¹

In essence Bauer is going to examine *Velikovsky's planetary theory* in terms of *Velikovsky's scientific competence*. The assumption underlying this personal evaluation is that if Velikovsky has a poor understanding of certain areas of science in one paper, then his other major theory may well be wrong or at least incompetent as well, because a person's competence is an acceptable scientific measure in some way of the merit of his theory. That is, if Velikovsky is wrong about some areas of scientific knowledge then he is probably wrong with respect to his major theory. But this approach is entirely personal and without merit, and unscientific, as Tom Van Flandern explained.

"A specific idea under consideration does *not* have a lesser (or greater) probability of being correct because its proposer has a higher (or lower) than normal error rate. THE VALIDITY OF AN IDEA CAN BE DETERMINED SOLELY BY AN EXAMINATION OF ITS MERITS, AND NOT AT ALL BY AN EXAMINATION OF ITS PROPOSER." [Capitals added]⁵⁶²

As Broad and Wade explain, but in a slightly different manner:

"If the originator of a revolutionary new concept either has low standing in the [scientific] elite system of his own discipline or if he comes from outside the discipline, his ideas are particularly likely not to receive serious consideration; *they will be judged on his perceived merits not on their own.*" (Emphasis added)⁵⁶³

Bauer, however, is going to evaluate Velikovsky personally on his perceived merits, on his *scientific knowledge* and on his prior errors, not solely on the merits of *his theory*. This, I suggest, is extraordinarily unscientific, dishonest, and illogical in the sense that the rating of Velikovsky's planetary theory is in some significant measure to be based on Velikovsky's understanding of science, not solely by an examination of the merits of the theory. Bauer is, therefore, going to evaluate the "proposer," not the "proposal," and believes this is *appropriate* as a method of evaluating Velikovsky's theory. It would be the same as evaluating Kepler's three laws

⁵⁶⁰*Ibid.*, pp. 126-127.

⁵⁶¹*Beyond, op. cit.*, p. 97.

⁵⁶²Tom Van Flandern, *Dark Matter, Missing Planets & New Comets*, (Berkeley, Calif., 1993), p. 358.

⁵⁶³William Broad, Nicholas Wade, *Betrayers of the Truth*, (New York, 1982), pp. 135-136.

of planetary motion based on his earliest erroneous view that there were only five regular solids which could be placed between the then known planets to give their distances from each other, which has never been scientifically accepted. Based on Bauer's personal approach, Kepler's laws of planetary motion are the work of a crank, crackpot, and pseudoscientist! One could, therefore, dismiss Kepler's laws of planetary motion because this earlier work was completely wrong!

What Bauer has chosen to do to evaluate Velikovsky's work is examine not Velikovsky's most secure or worthy evidence, but evaluate one of his least secure papers while at the same time he requests that the most secure evidence be employed to evaluate the Loch Ness monster! He further, dismissed Maurice Burton's book on Nessie as unworthy because his book is misleading, misquotes and misinterprets, while at the same time he requests that his own book, *Beyond Velikovsky*, not be dismissed as unworthy in spite of it being shown to be misleading, misquoting and misinterpreting. Finally, Bauer intends to evaluate Velikovsky's planetary hypothesis not solely on its merits but on the scientific merits of its proposer based on another paper on another topic. The standards of evidence that Bauer asks be applied to his sea monster views he does not apply to those of Velikovsky. This is sheer hypocrisy!

Not only, then, does this criteria of judgement fall on Velikovsky based on Bauer's criteria, but it is the very criteria by which I would ask the reader to judge Bauer. In plain language, in his criticisms of *Cosmos Without Gravitation* with respect to the physical sciences, does Bauer know what he is talking about?

What must be kept in mind is that Velikovsky published his analysis in 1946; therefore, we must evaluate this material only in terms of what was known up to and prior to that time, not by what became known thereafter. As Mark Twain pointed out, history "must be judged by the standard of the time and place, not by ours . . ." ⁵⁶⁴ Twain was discussing political history, but the same applies to scientific history. This would be similar to attacking Velikovsky's position against continental drift theory in the 1950's when, in fact, the general consensus and predominant view within the scientific establishment, as Professor Lynn E. Rose once discussed, was decidedly against this concept. Because the scientific consensus based on the evidence swung over in favor of drift theory under the name plate tectonics is, therefore, no adequate basis to castigate Velikovsky's views which were perfectly in accord with the thought of the scientific world of that time. I will analyze Bauer's criticisms on *Cosmos Without Gravitation* based on the scientific understanding and consensus of that distant era.

This becomes especially important with regard to Bauer's criticism of Velikovsky's assertions about ozone wherein he cites *Cosmos Without Gravitation* in which Velikovsky stated:

" . . . Ozone though heavier than oxygen, is absent in the lower layers of the atmosphere, is present in the upper layers and is not subject to the 'mixing effect of the wind' Nowhere is it asked why" [Bauer then states:]

"The question is not asked because it is a nonquestion; there is no paradox or anomaly here. Ozone is subject to the same convection as any component of the atmosphere but, unlike most of the others, it happens to be unstable

"The ozone does tend to spread from the upper atmosphere, where it is formed, to lower altitudes, as a result of the normal self-mixing of gases and whatever convection arises from winds and thermal gradients. But ozone is quite unstable and spontaneously reverts into the normal form of oxygen. Thus ozone is present effectively only in the upper atmosphere because it is formed there and decomposes before it can reach the lower atmosphere. There is absolutely nothing here to cast doubt on the theories of gravity, of gases, or the composition and the circulation in the atmosphere."⁵⁶⁵ (Emphasis added)

Bauer has authoritatively told us that *circulation in the upper atmosphere* where ozone forms caused by self-mixing and *whatever convection arises from winds and thermal gradients spreads ozone downward*. But was this what was understood and accepted by meteorological scientists in 1946? No, in fact, in terms of a mechanism, none of this was known, understood and accepted with regard to thermal gradients, convection and winds where

⁵⁶⁴James M. Lattis, *Between Copernicus and Galileo*, (Chicago, 1994), p. 11.

⁵⁶⁵*Beyond*, pp. 112-113.

ozone forms. On the question of ozone moving by normal mixing via convection, thermal gradients and winds in the upper atmosphere where it forms, the reader will find clear proof that, on this matter of physical science Bauer does not know what he is talking about!

Let us examine this on the basis of what the meteorologists had concluded was the correct evidence to explain this in 1946 when Velikovsky wrote and not from Bauer's unscholarly misunderstanding and misrepresentation of that era. The question, of course is, did meteorologists suggest that there were convection currents, winds and thermal gradients in the upper atmosphere where ozone forms back at that era? In fact, for Bauer's information they claimed exactly the opposite!!! Meteorologist W. J. Humphreys informs us that in the stratosphere where ozone is formed,

"In the region above the troposphere [the stratosphere or upper atmosphere] *where the temperature is constant with height, marked or vigorous vertical convection does not and cannot occur Since active convection here is impossible masses of air forced into this region necessarily would spread out in the horizontal strata*, and indeed there commonly are evidences of existence of just such strata though we seldom if ever are sure of their evidence."⁵⁶⁶ (Emphasis added)

But Bauer has told us there are thermal gradients, convection and winds to mix the ozone throughout the upper atmosphere, while Humphreys, a recognized authority on meteorology at that time, understood and accepted the concept that *there were no thermal gradients* and that *there was no convection or winds* to carry out this mixing.

Another meteorologist of that time, John G. Albright, in analyzing the stratosphere in his book, *Physical Meteorology*, (New York, 1939), page 169, presents a graph which shows that the temperature of the stratosphere is *isothermal* from top to bottom. Therefore, what we have is the establishment meteorologists saying at that time, in total contradiction to Bauer, that the upper atmosphere or stratosphere is at one uniform temperature and, under such conditions, "active convection here is impossible." But Bauer, without ever attempting to learn what was actually known about the stratosphere, speaks out authoritatively with a tone of absolute assurance that it seemingly had to have been known that there was a "thermal gradient" there giving rise to "convection and winds." Had he voiced such an opinion at that time—in the face of the accepted and established meteorological understanding of the stratosphere—he, on the basis of his own criteria of not knowing the established research or failing in attempting to learn about it, would have to be considered all the names he dumped so strikingly on Velikovsky. How could Velikovsky or anyone, for that matter, have known there were stratospheric "thermal gradients" and "convection" and "winds" in the stratosphere when no such measurements had been found to support this concept. Like all the scientists of that time, Velikovsky had faith in the reliability of these measurements. As Auguste Piccard states:

". . . meteorologists, on the faith of their first observations; came to the conclusion that *an ideal stratosphere existed. Along a vertical line of temperature were to be perfectly homogenous and NO MOVEMENT OF AIR ASCENDING OR DESCENDING, WAS TO TAKE PLACE*

*"If the temperature of the stratosphere was perfectly homogenous, there would be no motive power inside the stratosphere creating local air currents."*⁵⁶⁷ (Emphasis and capitalization added)

Air currents of course are "winds." According to Piccard there were no air currents, meaning winds, in the stratosphere.

Without the motive power of a thermal gradient in the stratosphere the meteorologists suggested that gases in it became stratified. As meteorologist G. M. B. Dobson explained later in 1968,

⁵⁶⁶W. J. Humphreys, *Weather Brambles*, (Baltimore, Md., 1937), pp. 58-159.

⁵⁶⁷Auguste Piccard, *Between Earth and Sky*, (London, Eng., 1950), p. 63.

"Some years ago it was thought that the stratosphere would be stagnant enough for separation of light and heavy gases to take place."⁵⁶⁸ (Emphasis added)

On the basis of this evidence, meteorologists thought heavier gases would sink to the bottom of the stratosphere, lighter gases would be at the top. Thereafter, with rocket research in the late 1940's it was found that the stratosphere did have a small thermal gradient, as well as winds and some convection.

Even today it is known that there is extremely little stratospheric mixing of gases. As we were informed in 1984, the date of Bauer's book, "[in the stratosphere] the air is not well mixed at these altitudes"⁵⁶⁹

And inasmuch as Bauer has informed us about the instability of ozone not reaching lower regions of the atmosphere, as late as 1968 we are informed: "The large variations of ozone with heights that are sometimes found in the lower stratosphere, and their cause is still not well understood."⁵⁷⁰ While in 1959 it was stated "For reasons not yet explained completely, there seems to be more ozone in the stratosphere near the earth's poles, than at the equator, and more in spring of the year than in fall or winter."⁵⁷¹ But Bauer tells us that Velikovsky should have known 14 and 22 years after he had raised the question regarding ozone, what the meteorologists themselves did not know and admitted they did not know how to explain the amounts of ozone found in the stratosphere and below it.

Bauer has further stated that "ozone is quite unstable and spontaneously reverts into the normal form of oxygen. Thus ozone is present effectively only in the upper atmosphere before it can reach the lower atmosphere."⁵⁷² Ozone breaks down by interaction with other gases in the atmosphere as well as builds up by reversal of some of these chemical reactions. It also is destroyed if it *makes contact with the ground or dust in the atmosphere*. It was, in fact, presented long ago that the primary cause of ozone breakdown was *contact with the ground*. According to G. M. B. Dobson, writing in 1968 after the stratospheric thermal gradient was discovered, stated:

"In the photo-chemical region there may be as much as one part of ozone to every 100,000 parts of air. The ozone in the photo-chemical regions will be slowly mixed with all the rest of the atmosphere and if there was no destruction of ozone near the ground the whole atmosphere would contain something like this proportion of ozone [I]t is fortunate that *it is so rapidly destroyed at the ground!* Because the ozone is destroyed at the ground there is a gradual drift of ozone from the photo-chemical region downward through the whole of the lower atmosphere."⁵⁷³ (Emphasis added)

Bauer has told us ozone "decomposes before it can reach the lower atmosphere." But, in fact it reaches the ground! On the other hand, Dobson states ozone drifts downward and is destroyed "rapidly at the ground." How long is it suggested that ozone can remain stable in the lower atmosphere? According to Dobson, "Any ozone which comes below a height of 20 km [12 miles] will be protected from the sun's ultraviolet light by the ozone above it and *will have a life of several months*, provided that it does not come near the ground."⁵⁷⁴ (Emphasis added) Humphreys, further tells us that "one-fourth of the ozone is in the troposphere."⁵⁷⁵ The troposphere is the lower

⁵⁶⁸G. M. B. Dobson, *Exploring the Atmosphere*, (Oxford, Eng., 1968), p. 10.

⁵⁶⁹Stephen H. Schneider, Randi Londer, *The Coevolution of Climate and Life*, (San Francisco, 1984), p. 147.

⁵⁷⁰*Ibid.*, p. 168.

⁵⁷¹Clyde Orr, Jr., *Between Earth and Space*, (New York, 1959), p. 28.

⁵⁷²*Beyond, op. cit.*, p. 113.

⁵⁷³Dobson, *op. cit.*, p. 114.

⁵⁷⁴*Ibid.*

⁵⁷⁵Humphreys, *op. cit.*, p. 94.

atmosphere. Does a life span of several months indicate that ozone would have insufficient time to reach the lower atmosphere as Bauer has so authoritatively told us?! And if, as Bauer states, "it decomposes before it can [effectively] reach the lower atmosphere," why did Humphreys state "one-fourth" of all the ozone in the atmosphere resides in the "troposphere" or lower atmosphere or Dobson shows it actually reaches the ground?! Bauer is, in part, correct that ozone does descend, but he is also wrong about the nature of the stratosphere.

In the face of these undeniable facts, it is clear that Bauer, who tells us Velikovsky does not know what he is talking about, does not know what he himself is talking about regarding "thermal gradients," "convection," and "winds" in the stratosphere. Bauer is totally ignorant of the accepted evidence and concepts in the period in which Velikovsky wrote. In 1959 and 1968 meteorologists still did not understand the distribution of ozone in the atmosphere but Bauer expects Velikovsky to have known in 1946 what was unknown in 1959 and 1968. Bauer's criticism is disconnected from the realities of meteorology of the 1940's and this is the basis of Bauer's criticism, part of his "in depth study" of *Cosmos Without Gravitation*.

Perhaps this lack of knowledge on Bauer's part would be excusable if Bauer had not claimed to have read a book on meteorology from that era which specifically dealt with this information regarding meteorological knowledge that the stratosphere was isothermal and on this basis did not possess thermal gradients, convection currents or wind. But Bauer cannot even claim ignorance of this because on page 111 of *Beyond Velikovsky*, Bauer cites W. J. Humphreys' book, *Physics of the Air*, 2 ed., (New York 1929). The 1929 date in Bauer's "Bibliography" for Humphreys' book is to the second edition, while Velikovsky cited the 1940, third edition. If Bauer wished to properly evaluate Velikovsky's work regarding Humphreys' meteorological evidence, he should have at the very least read the edition of the book Velikovsky cited, not the edition which is eleven years older. There are differences between the two editions.

Here, then, is what Humphreys wrote about the concept of "thermal gradients" or, more precisely, about the "absence of thermal gradients" in both the second edition Bauer read and the third edition Velikovsky cited.

"Chapter IV"

"The Isothermal Region or Stratosphere"

"Of all the conditions indicated by the temperature gradients . . . by far the most surprising and most difficult to explain is the approximately isothermal state of the upper atmosphere. Indeed, the discovery of the fact that the temperature of the upper atmosphere changes but little with altitude . . . constitute[s] one of the most important advances in modern meteorology

"Indeed, as a rule the change is so small that the whole region characterized by this approximate constancy of temperature has been called the "isothermal region." At present it is called the "stratosphere."⁵⁷⁶

The very book Bauer cites by an authority of that time showed in plain English that there were no thermal gradients in the upper atmosphere to drive convection or winds but that this entire region was considered to be isothermal. How then could Bauer, in all good conscience have stated, after reading this material, that the mixing of ozone occurs from convection and winds caused by thermal gradients. Bauer expressed this approach as his "wish to make the scientific argument accessible to all, so that the opportunity exists for everyone to reach an individually informed decision."⁵⁷⁷ How can anyone make an informed decision about stratospheric thermal gradients, convection and winds based on Bauer's treatment of this material? How can anyone make an informed decision about Velikovsky's work based on Bauer's failure to find out what meteorological science stated on this subject at

⁵⁷⁶W. J. Humphreys' *Physics of the Air*, 2 ed. (New York, 1929), p. 46. The same statement appears in the 3 ed., p. 51.

⁵⁷⁷*Beyond, op. cit.* p. 112.

that time? Humphreys' suggestion in the 1940 edition that there are winds in the stratosphere is based only on "inference" which was yet unproven, as well as unproven cyclonic movements.

Interestingly, it is in this same chapter Bauer goes on to accuse Velikovsky because:

"He misrepresented theories and facts extant at the time he wrote—and not because he had not read them, for he quotes from authoritative sources: either he did not understand, or he deliberately misrepresented. Whatever the reason, it is plain that Velikovsky's references to scientific work are not to be relied on."⁵⁷⁸

Could anyone believe that Bauer did not misrepresent the theories and facts extant at the time Velikovsky wrote—and not because he did not read about them for he quotes authoritative sources? Either Bauer did not understand or he deliberately misrepresented. Whatever the reason it is plain that it is Bauer's references to scientific work which are not to be relied on. The very things Bauer accuses Velikovsky of doing is, in fact, what he himself is doing. Therefore, I cite the following remarks on the point of isothermal temperature. "Hell must be isothermal for otherwise the resident engineers and physical chemists (of whom there must be many) could set up a heat engine to run a refrigerator to cool off a portion of their surroundings to any desired temperature."⁵⁷⁹

This is not to suggest in any way that Velikovsky is correct on this point, but only to illustrate the deeply flawed nature of Bauer's criticism.

In problem 4 Bauer raises the point that "there is no need to invoke repulsive forces, or as Velikovsky puts it, "forces generated in collisions" of molecules of gases to drive their motion. In this problem Bauer is discussing the kinetic theory of gases. Bauer cites Velikovsky thus,

". . . the motion of the molecule, if effected by a mechanical cause, must subside because of the gravitational attraction between the particles and also because of the gravitational pull of the earth . . . as the molecules as a gas at a constant temperature . . . do not stop moving, it is obvious that a force generated in collisions drives them . . ." (*Cosmos Without Gravitation*, p. 4)⁵⁸⁰

What Bauer has omitted is most significant because it changes the meaning and physical conditions. Velikovsky was discussing not only "gas at a constant temperature" but "in a perfect insulator,"⁵⁸¹ which is an impossibility. Bauer, however, never mentions this parameter which Velikovsky placed on this analysis. He argues that gases do not require "repulsive forces" for motion, that according to Sir James Jeans quantum theory removes any need for these "repulsive forces between the molecules." Does modern physics theory of gases say that there is no "repulsive force between molecules of gas during collisions?" Here then is what George Robert Talbot, a physicist, sent to me from an undergraduate textbook of physics which says with respect to repulsive forces between molecules in gases when they collide:

"Forces between molecules are known to be of electric and magnetic origin. All molecules contain electric charges in motion. These molecules are electrically neutral in the sense that the negative charge of the electrons is equal and opposite to the charge of the nuclei. This does not mean, however, that molecules do not interact electrically. For example, when two molecules approach each other the charges on each are disturbed and depart slightly from their

⁵⁷⁸*Ibid.*, p. 121.

⁵⁷⁹Henry A. Beirut, "Science and Technology," *Humorous Quotations*, ed. Des MacHale, (Dublin, Ireland, undated), p. 61.

⁵⁸⁰*Beyond, op. cit.* p. 115.

⁵⁸¹*Ibid.*

usual positions in such a way that the average distance between opposite charges in the two molecules is a little smaller than that between like charges. Hence, an attractive intermolecular force results. This internal rearrangement takes place only when molecules are fairly close together, so that these forces act only over short distances; they are short-range forces. *If the molecules come very close together, so that their outer charges begin to overlap, the intermolecular force becomes repulsive. The molecules repel each other because there is no way for a molecule to rearrange itself internally to prevent repulsion of the adjacent external electrons. It is this repulsion on contact that accounts for the billiard ball character of molecular collision in gases.* If it were not for this repulsion molecules would move right through each other instead of *rebouncing on collision*.⁵⁸² (Emphasis added)

What becomes painfully clear is that what Bauer was saying about repulsive forces between gas molecules during collisions is dead wrong. This authoritative undergraduate college textbook puts forth precisely the opposite conclusion on this matter of "repulsive forces" between gas molecules during "collisions" causes them to rebound like billiard balls. When gas molecules collide the negatively charged electrons of the molecules must come close together, electrons with like negative charges repel one another. Here Bauer's statements collide with basic scientific knowledge.

What Bauer wishes to argue, however, on pages 116-117, is that Sir James Jeans presented the statement that quantum theory would account for the motions of molecules in gases. What I wish to point out before proceeding is that even today these same "repulsive forces" during collision causing rebound of gas molecules are invoked by physicists. What was Jeans' view of quantum theory and why was it invoked to explain this concept? The history of Jeans' work on the kinetics of gases is important. The following discussion was supplied by a physicist, George Robert Talbott, who gave me permission to use his material, which I do so verbatim).

"*Jeans Dynamical Theory of Gases* was published first. Its First Edition was extensively revised in the Second Edition which appeared in 1916. This major revision was due to advances in quantum theory, new at the time. The Third Edition, published in 1920, extended the quantum mechanical approach, and the Fourth Edition, from 1925, was used later on in 1954, for the Dover paperback publication. Even as late as 1925, Jeans believed that the quantum mechanical explanation of gas behavior was at best pragmatic and partial. Jeans' *Introduction to the Kinetic Theory of Gases* appeared in 1940, and by that time he appears less critical of quantum mechanical explanations, as will be shown in quotations below. What there is of quantum mechanics is given in the earlier 1925 book, the later 1940 one explicitly stating that the matter is not to be discussed. Any reader able to work at this level in physics knows that Bauer's claim that the whole matter has been settled and sealed by quantum mechanics is the stuff of glossy journal "scientific educators," not the considered position of one engaged in physical research. To gain some historical perspective, Jeans was born in 1877 and died in 1946. He was a friendly and argumentative colleague of Eddington. Like other great scientists who followed them, they were aware of how little we know, even with the advent of pragmatic advantages in the new physics. Velikovsky did not believe that the failure of classical physics in the area of kinetic theory was overcome by quantum mechanical strategies. Jeans did not believe it either, and Bauer misreads here as elsewhere

"The failure of classical physics in kinetic theory is stated quite clearly in Jeans' *Dynamical Theory of Gases* (Fourth Edition, 1925, pages 6 and 7) in which the analogy between 'billiard balls' and molecules is used in the usual way.

'One further question must be answered. No matter how elastic the billiard-balls and table may be, the motion cannot continue indefinitely. In time,

⁵⁸² David Halliday and Robert Resnick, "Intermolecular Forces," *Physics for Students of Science and Engineering*, Vol. 1, (1960), p. 500.

the energy of this motion will be frittered away partly by frictional forces, such as air resistance, and partly by the vibrations set up in the balls by collisions. The energy dissipated by air-resistance becomes transformed into energy of internal vibrations of the billiard-balls. What then does this represent in the gas, and how is it that a gas, if constituted as we have supposed, does not, in a very short time, lose the energy of translational motion of its molecules, and replaces it by the energy of internal vibrations of those molecules, and energy in the surrounding ether?

‘The difficulties raised by this and similar questions formed a most serious hinderance to the progress of the kinetic theory for many years. Attention was drawn to them by Maxwell but it was not until the introduction of the Quantum-theory by Planck and his followers in the early years of the present century, that it was possible TO GIVE ANYTHING LIKE A SATISFACTORY EXPLANATION. The explanation supplied by the Quantum-theory will have to be examined in detail in a later chapter of the present book. IT IS AT BEST ONLY PARTIAL, BUT MUST, AS FAR AS IT GOES, PROBABLY BE REGARDED AS SATISFACTORY. The explanation is, in brief, that there is no true analogy between the two cases when we consider questions of internal vibrations and transfer of energy to the surrounding medium. For the motion of the billiard-balls is governed by the well-known Newtonian laws, whereas the internal motions of molecules, and their transfer of energy to the ether, ARE NOW BELIEVED TO BE GOVERNED BY AN ENTIRELY DIFFERENT SYSTEM OF DYNAMICAL LAWS.’ (Emphasis mine)

"Here in 1925, Jeans is very cautious. Later in 1940, in his *Introduction to the Kinetic Theory of Gases*, he states the above word for word, but omits the extreme caution of his earlier book. On page 16, we have the following:

‘The difficulty raised by this and similar questions formed a most serious hindrance to the progress of the Kinetic Theory for many years. Maxwell drew attention to them, and Kelvin, Rayleigh and many others worried over them but it was not until the introduction of the Quantum theory by Planck and his followers in the early years of the present century, that it became possible to give anything like a satisfactory explanation.’

"Jeans now omits, in 1940, the statement, ‘IT IS AT BEST ONLY PARTIAL BUT MUST AS FAR AS IT GOES PROBABLY BE REGARDED AS SATISFACTORY.’ This is the only difference between the account in *The Dynamical Theory of Gases*, (1925), and that in *Introduction to the Kinetic Theory of Gases*, (1940). As I stated earlier, only in the [earlier] *Dynamical Theory* is the mathematics of quantum theory developed. *Introduction to the Kinetic Theory of Gases*, page 16 has this concluding remark:

‘The newer system of laws, constituting the modern theory of quantum mechanics, is beyond the scope of the present book.’

"In neither book is there an implication that quantum theory, has solved all the problems which bothered Velikovsky." [Here ends the verbatim comment of Dr. Talbott.]

In this instance, Velikovsky made it quite clear that he did not accept the explanation of the kinetic theory of gases as outlined in the application of quantum theory to it. Bauer apparently accepts both theories without reservation. Velikovsky did not do so with respect only to gases.

Why didn't Velikovsky accept the concept that quantum theory could account for all the motions of molecules of gases? Here I must hazard guesses.

For one thing Richard Feynman, a Nobel laureate, made the specific claim,

"I think I can safely say nobody understands quantum mechanics."⁵⁸³

If, as Feynman suggests, no one understands how a theory works, should Velikovsky be condemned for questioning its correctness while, according to historian of science I. Bernard Cohen, "Einstein, for example, cannot bring himself to accept the conclusions and premises of present quantum mechanics"⁵⁸⁴ Should Velikovsky then be criticized for holding the same opinion as Einstein about the conclusions and premises of quantum theory?

According to Paul Feyerabend:

"And now take the best theories of modern physics, general relativity in its most recent form and general quantum mechanics. So far it has proved impossible to merge them into a coherent whole—ONE THEORY MAKES ASSERTIONS THAT FLATLY CONTRADICTED THE OTHER. Can we still assert that we get a correct description of reality from either of them? WE CANNOT. WE CAN SAY THAT BOTH THEORIES ARE USEFUL APPROXIMATIONS BUT WE HAVE NO IDEA WHAT THE REALITY THEY APPROXIMATE LOOKS LIKE."⁵⁸⁵ (Capitalization added)

This evident contradictory nature between quantum theory and general relativity was well-known in the earlier part of the 20th century. Velikovskian literature, in fact, discussed this.⁵⁸⁶ What Velikovsky may have decided is that when two of the world's best scientific theories are based on assertions that flatly contradict one another and are really only "useful approximations" but "do not, nor cannot precisely describe the underlying reality of nature," then it was reasonable to attempt to see if other explanations would describe the evidence.

The point I wish to stress is that even if this is not Velikovsky's reason for rejecting quantum theory, there is no solid foundation underlying it because the other theory, general relativity, may turn out to be correct. Bauer seems to assume that quantum theory is correct because it does approximate the energies and motions of gases.

As long ago as 1948, the historian of science, Sir William Cecil Dampier stated, "Bohr's atomic model, with its circling planetary electrons, goes farther from the observed facts than it is safe to venture. We can only examine atoms from outside, keeping note of what goes in and what comes out, . . . Bohr has described one mechanism [*via* quantum theory] which will produce some, at all events, of atomic properties. But it is possible that other types of mechanism might work equally well."⁵⁸⁷ What Dampier is suggesting is that the mechanism envisaged by quantum mechanicians is only an assumptive analysis because one cannot see inside the atom and Velikovsky was proposing that another, "possible other type of mechanism might work equally well." Dampier adds this with respect to heat in such an energy system. ". . . the great science of thermodynamics, which deals

⁵⁸³Richard Feynman, *The Character of Physical Law*, (M.I.T. Press, 1967), p. 129.

⁵⁸⁴I. Velikovsky, *Stargazers & Gravediggers*, (New York, 1983), p. 248.

⁵⁸⁵Paul Feyerabend, *Farewell to Reason*, (London, 1987), pp. 250-251.

⁵⁸⁶H. C. Dudley, "The Personal Tragedy of Albert Einstein," *KRONOS*, Vol. 1, No. 4, (April 1976), p. 66.

⁵⁸⁷Sir William Cecil Dampier, *A History of Science*, (New York, 1948), pp. 411-412.

only with the changes in heat and energy in a system, makes no use of pictures of intimate mechanism such as are given by atomic concepts."⁵⁸⁸

What he means, of course, is that the kinetic theory of gases with application of quantum mechanics is only an outside assumptive projection of events inside the atom. That is why I believe Jeans called quantum theory "only partial . . . so far as it goes, probably regarded as satisfactory" [as an explanation]. That is, quantum theory is merely "a system of computations," as David Bohm thus describes quantum theory. Dr. Talbott sent me this final statement of Bohm's:

"In fact you must engage in logical gymnastics in order to accommodate the present view. The typical reaction of a student who studies quantum mechanics is that at first he doesn't understand it and by a year or two later he says there is nothing to understand because it's nothing but a system of computation. At the same time they've got to say, no, it isn't just that, we're discussing reality. After all, physicists would have no motive for doing the work they do if they didn't believe that these particles are really the building blocks of the universe. So, you see, you have to engage in and become very skillful at mental gymnastics in order to sustain this myth. It's actually not so easy. It takes several years and a lot of skill to train people to be able to do it [*i.e.*, to avoid the above philosophical implications]."⁵⁸⁹

Here a world renowned physicist tells us quantum theory is a "myth" which does not explain what is *really* occurring. In such a theoretical condition Velikovsky had every right to question quantum theory's explanatory power, he was in no way out of touch with real science for questioning its application to gases.

If Bauer is unconvinced by all this, let him present what scientists have proven, that atoms are internally organized, as the theory suggests. This, Bohm stated, had not been done two years prior to Bauer's critique of this matter.

Bauer then moves on to Velikovsky's discussion of water in clouds which does not settle out and fall to the surface of Earth. To explain why water particles do not fall even though they are 800 times heavier than as Bauer states,

"There is a well-known class of systems in which the particles are larger than normal molecules but still sufficiently small that they do not settle out under the influence of gravity. These are called colloidal systems, made up of colloidal particles

"Clouds, mist, and fog consist of particles of water that are of colloidal size. These droplets remain suspended in the air as long as they do not exceed a certain critical size

"This 'fact' of Velikovsky's then serves only to show that he is ignorant of an important and well-understood class of substances."⁵⁹⁰

Let us assume that colloidal systems create clouds and hold them aloft as Bauer suggests; what would ensue? The point Bauer has missed is that if that were the case clouds would rarely if ever form. When a bottle of perfume is opened in a room perfume molecules enter the air as a colloid and diffuse throughout the air in the room. Eventually they would be dispersed everywhere in the room but they would not form a cloud. In order to form a cloud some force is necessary to concentrate them into a particular area. What Bauer has failed to understand is the well understood processes of diffusion which disperses vapor particles and some form of active transport which concentrates particles into a region of space. The energy related to concentrating water vapor particles into clouds is created by *updrafts* and wind motion.

⁵⁸⁸*Ibid.*, p. 412.

⁵⁸⁹K. Wilber, *The Holographic Paradigm*, (New York, 1982), p. 58.

⁵⁹⁰*Beyond, op. cit.*, pp. 113-115.

The question is: How many water particles in clouds are above the "critical size" and why don't these constantly fall to Earth. I am not disputing the colloidal nature of clouds, but is that what keeps clouds aloft? Here then is what a scientist writing in 1933 stated on this question: "Clouds float in the sky because the individual droplets are so minute that the viscosity of the air is very effective in retarding their fall. They actually would gradually settle in still air, and in general there is always this component [updrafts] intermingled with the effects of air currents."⁵⁹¹

In the era in which Velikovsky wrote, the meteorologists made it quite clear that cloud particles of water are always falling; the smaller, less massive the particle, the slower the rate of descent. Therefore, what is the range of sizes of these cloud particles and the amount of fall based on their sizes? According to meteorologists Roger Clausse and Leopold Facy, for the various types of clouds, the size range of these particles is between 2 and 200 microns with average diameters between 8 and 50 microns.⁵⁹² That is, an inordinate number of droplets are above the "critical size" and must be constantly falling. 50 micron particles fall at a rate of 10 centimeters, or almost 4 inches per second, 60 micron droplets fall at 15 centimeters or about 6 inches per second, and 100 micron droplets fall at 27 centimeters or about 10 inches per second.⁵⁹³ Since clouds are made up of untold trillions of droplets, untold billions of the larger particles are always falling out of the cloud. This being the case, the cloud would be diffusing water vapor droplets away from the cloud in outward directions, and untold droplets above critical size would be falling out of the cloud and again the cloud would not form or survive if colloidal systems were all that is working. As Fritz Kahn explains, these larger "Drops of water do not remain suspended for long unless soaring winds lift them up. They tend to fall from sheer weight . . . every cloud is composed of falling drops . . ."⁵⁹⁴ If the air below the cloud has reached the water saturation point the drops *would* reach the ground.

In essence Bauer, in spite of his knowledge of colloids, does not understand the basic nature of cloud physics which he discusses authoritatively as if he knew what was actually occurring. A cloud, although a colloid could not be sustained above the Earth without updrafts. Again this is not to suggest in any way that Velikovsky is correct but to illustrate that Bauer's discussion of these topics shows that he is ignorant of what *was* known. Again all Bauer had to do to learn about this basic area of physics is read any encyclopedia which deals with "Cloud Physics."

But Bauer then turns around to accuse Velikovsky of being "ignorant of an important and well-understood class of substances," namely colloids. According to Dr. Talbott,

"The explicit and specific charge Bauer makes is that Velikovsky is totally ignorant of colloids. This implies the allegation that Velikovsky lacks even a decent high school education. What were Velikovsky's educational credentials? According to Fredric Jueneman, Velikovsky

' . . . formally studied mathematics and languages at Medvednikov Gymnasium, Moscow, graduating with honors and a gold medal; pre-medical studies in the natural sciences were begun at Montpellier, France and continued at the University of Edinburgh, Scotland. During World War One he studied law and ancient history at Moscow's Free University, and later received his medical degree at the University of Moscow.

'In Berlin, with Professor Heinrich Loewe, he founded the publication *Scripta Universitatis*, conceived as the corner stone of what is now Hebrew University in Jerusalem. Professor Albert Einstein edited the mathematical-physical volume of this work. In 1923 he married and moved to Jerusalem to practice medicine, and afterward he studied psychoanalysis in Vienna under a

⁵⁹¹Matthew Luckiesh, *The Book of the Sky*, (New York, 1933), p. 30.

⁵⁹²Roger Clausse, Leopold Facy, *The Clouds*, (New York, 1961), p. 53.

⁵⁹³*Ibid.*

⁵⁹⁴Fritz Kahn, *Design of the Universe*, (New York, 1956), p. 312.

student of Sigmund Freud, returning to practice psychiatry in Haifa and Tel Aviv.⁵⁹⁵

"Now ANY psychiatrist must understand and be able to apply the colloidal gold test for diagnostic support in studies of syphilitic damage to the nervous system. Velikovsky HAD to know the nature of colloids because his medical specialization would demand that knowledge. Velikovsky practiced psychiatry up until 1940 and the colloid gold test was employed since 1912 or when Velikovsky was only 17 years old. Therefore, he would have to have studied it in medical school. It is specifically stated, 'Colloidal Gold Test—Lange's colloidal gold test, introduced in 1912 and now widely used, consists in mixing cerebrospinal fluid in certain proportions with a colloidal solution of gold.'⁵⁹⁶ If a patient is normal the fluid does not change color. However, in cases of syphilis and certain pathological conditions of the nervous system there are changes in the color of the gold colloid solution from red to purple, deep blue, pale blue, etc. Velikovsky as a doctor and psychiatrist had to fully understand this test and how colloids behave as part of his educational requirements and practice."

Here Dr. Talbott's discussion ends.

It is quite clear Velikovsky understood the nature of colloids; what he questioned was, that in spite of the colloidal nature of clouds, since they apparently contained untold billions of water drops well above colloid size that must gravitationally fall, why then did they not do so? He suggested this could be due to electrical or electromagnetic action. After all, clouds do develop a charge and then discharge as lightning during thunderstorms when charged raindrops fall. If Velikovsky's conclusion was wrong, it was a reasonable attempt to try to explain this phenomenon not only in terms of gravity and colloids but also in terms of electromagnetic action. To claim Velikovsky is ignorant of the possible nature of colloids is blatantly dishonest. At best Bauer could have claimed he did not transfer his biological knowledge of colloids to clouds. But since clouds are held aloft by updrafts this accusation is itself without merit.

But let us now turn this type of accusation around and raise the specter that Bauer does not understand the fact that gas molecules on collision are repelled because like charges repel each other. How would Bauer feel if I accused him of not knowing that electrons in atoms of molecules in a gas upon collision repel one another because they contain like charged electrons? Should I say that Bauer does not know that electrons are negatively charged, and does not know that like charged particles repel each other? But this precisely is Bauer's technique in his criticism of Velikovsky. If Velikovsky does not specifically discuss colloids or Brownian movement in his discussion of concepts, Bauer goes out of his way to say Velikovsky does not know these phenomena. Should I also add that Bauer does not know of the existence of dust in the atmosphere which destroys ozone on contact? This kind of criticism on Bauer's part is really mendacious.

But Bauer then goes on to discuss Velikovsky's "facts" 6 through 25, or 19 of the 25 problems raised in *Cosmos Without Gravitation* wherein he cites Velikovsky:

". . . The semidiurnal changes in barometric pressure are not explainable by the mechanistic principles of gravitation and the heat effect of solar radiation . . ." [Bauer then states]

"Velikovsky quotes Lord Rayleigh (1890) and Humphreys (1940) to the effect that these changes are not understood. That does not, however, make them not explainable, not

⁵⁹⁵Fredric B. Jueneman, "The Search for Truth," *Analog*, (October 1974), p. 34.

⁵⁹⁶Todd and Stanford, "Colloidal Gold Test," *Clinical Diagnosis by Laboratory Methods*, (1946), p. 598.

understandable, on the basis of presently accepted physical laws, and Rayleigh and Humphreys do not say that— merely that a full explanation has not yet been given."⁵⁹⁷

True Humphreys states science has no "finding for it [of] a complete explanation."⁵⁹⁸ This is not what Lord Rayleigh had to say as cited by Humphreys. What Lord Rayleigh actually said was "The relative magnitude of the latter [semidiurnal variations] as observed at most parts of the earth's surface is still a mystery, *all* the attempted explanations being illusory."⁵⁹⁹ (Emphasis added) There is no explanation for this phenomenon but Bauer argues that although this problem has existed since its discovery by Dr. Beals around 1664-1665,⁶⁰⁰ a period of over three hundred years, does not mean it is unexplainable, merely unexplained. And it is still unexplained today. Thus Bauer believes a physics problem which has taxed the best minds of the scientific community for over three hundred years must eventually have an explanation in terms of present physical laws which have not solved it. How does he know this is possible or even probable? He does not! By looking into his crystal ball he cannot in any way fathom the solution to this problem. He may hope for such an outcome, but to suggest that the solution will be in terms of present day physical laws is baseless! Bauer argues, "To overturn existing theory, one must adduce facts that contradict that theory, not merely point to facts that have not yet yielded to detailed explication."⁶⁰¹

What Bauer implies is that his *opinion*—and it is only an opinion—is proof of his assertion. But even on this point Bauer runs into contradiction. Sir Fred Hoyle a Nobel Prize winner in science, sees this sort of rhetoric raised by Bauer in a completely different light and comes to exactly the opposite conclusion. Hoyle calls Bauer's type of approach "THE WAGES OF RESPECTABILITY."

"Science is unique to human activities in that it possesses vast areas of certain knowledge. The collective opinion of scientists in these areas about any problem covered by them will almost always be correct. It is unlikely that much in these areas will be changed in the future, even in a thousand years. And because technology rests almost exclusively on these areas the products of technology work as they are intended to.

"But for areas of uncertain knowledge the story is very different. Indeed the story is pretty well the exact opposite, with the collective opinion of scientists almost always incorrect. There is an easy proof of this statement. Because of the large number of scientists nowadays and because of the large financial support which they enjoy, uncertain problems would mostly have been cleared up already if it were otherwise. So you can be pretty certain that whenever problems resist solution for an appreciable time by an appreciable number of scientists the ideas used for attacking them must be wrong. It is therefore a mistake to have anything to do with popular ideas for solving uncertain issues and the more respectable the idea may be the more certain it is that they are wrong."⁶⁰²

It is rather clear that Hoyle is suggesting that Bauer's position is without real merit. But the most fascinating and intriguing aspect of this analysis by Bauer is that this is his *absolute proof* in problems 6 through 25 presented in *Cosmos Without Gravitation* as evidence to dismiss each of these problems. In all these cases Bauer argues, "The same objection applies to Velikovsky's 'facts' 6 through 25."⁶⁰³

⁵⁹⁷*Beyond, op. cit.*, p. 117.

⁵⁹⁸W. J. Humphreys, (1940) *Physics of the Air, op. cit.*, p. 240.

⁵⁹⁹*Ibid.*

⁶⁰⁰*Ibid.*

⁶⁰¹*Ibid.*, pp. 117-118.

⁶⁰²Sir Fred Hoyle, "The Wages of Respectability," *The Origin of the Universe and the Origin of Religion*, (Wakefield, R.I. and London, Eng., 1993), pp. 17-18.

⁶⁰³*Beyond, op. cit.*, p. 118.

This is sheer nonsense! He has not disproved or even dealt with one iota of these 19 problems. What he has done amounts to saying that Velikovsky has 19 problems related to gravitational theory that have defied explanation in terms of that theory. But Bauer suggests this proves absolutely nothing. They should then be explainable in terms of gravity. Can you imagine anyone saying that he will completely explain away the vast bulk of Velikovsky's evidence by offering as counter evidence that all these problems raised by Velikovsky may, perhaps, might be or may be, *explained* at some unknown future date by the same physics that presently does not explain them? And on the basis of this "in-depth analysis," Bauer has shown that problems 6 through 25 prove that Velikovsky does not know what he is talking about.

Derek Gjertsen, the philosopher and historian of science at the University of Leeds and Oxford describes the type of argument Bauer has raised:

"Given the right to call in the outcome of 'many centuries,' almost anything would be possible. The alchemist could equally claim his powder would transform lead into gold once it had been allowed to stand for a few centuries. There could even be a lazarus ointment which when smeared on a corpse would revive it several centuries in the future."⁶⁰⁴

To be sure Gjertsen considers this "future" concept "the weakest of responses, an excuse commonly offered in defense of the fake and the charlatan."⁶⁰⁵ But this is the argument that Bauer presents. And again, I will not call Bauer the names employed by Gjertsen—Fake and Charlatan.⁶⁰⁶ But I will say that Bauer's argument about the future smacks of snakeoil!

Bauer's argument is an example of the logical fallacy called "*Ignotum per ignotius*." It is the defense of an ignorant argument such as "unexplained aspects of scientific phenomena"—by invoking an even more ignorant general principle "can be explained in the future by the same concepts," which is even less plausible and subject to criticism. Since these aspects of gravitational theory are inexplicable based on gravitational theory, how can one, as a principle, suggest that in the future, answers based on gravity will solve them? In this sense, Bauer exhibits the arrogance he accused Velikovsky of possessing. He suggests that he can see into the future and knows in some way what the future research will show regarding each and every one of these problems. Like a magician, he is clairvoyant about future scientific development.

But such an approach is without one shred of substantive evidence to prove anything at all. It is pure speculation masquerading as analysis. It is not so much that Sir Fred Hoyle, completely contradicts what Bauer is saying, but it is that such an analysis can be applied to almost any concept. Anyone can say that, in the future, solutions to unsolved problems based on present-day thinking will be found; but this proves absolutely nothing. One can just as easily suggest that in the future, evidence will be found that completely proves just the opposite of what Bauer is presenting and defending. I am doing neither. What I am doing is showing that when Bauer cannot disprove the evidence that Velikovsky presented in *Cosmos Without Gravitation*, he produced mindless rhetoric as his evidence and then used this rhetoric to dismiss the bulk of Velikovsky's evidence. To suggest that he has answered these problems with evidence or proof or even research is simply absurd!

But let us remember that Bauer has argued: "To overturn existing theory, one must adduce facts that contradict that theory, not merely point to factors that have not yielded to detailed explanation."⁶⁰⁷ But this fact *has indeed been presented* by Velikovsky. He showed "facts that do indeed contradict the theory of gravity," but Bauer then completely ignored these facts. For example, in problem ten, Velikovsky states in *Cosmos Without Gravitation*,

⁶⁰⁴Derek Gjertsen, *Science and Philosophy*, (London, Eng., 1989), p. 107.

⁶⁰⁵*Ibid.*

⁶⁰⁶*Ibid.*

⁶⁰⁷*Ibid.*, pp. 117-118.

"Over the oceans the gravitational pull is greater than over the continents, **THOUGH ACCORDING TO THE THEORY OF GRAVITATION THE REVERSE SHOULD BE TRUE**; the hypothesis of isostasy also is unable to explain this phenomenon (Vening Meinesz; see Fleming *Terrestrial Magnetism*, p. 33). The gravitational pull drops at the coast line of the continents. Furthermore, the distribution of gravitation in the sea often has the peculiarity of being stronger where the water is deeper. 'In the whole Gulf and Caribbean region the generalization seems to hold that the deeper the water, the more strongly positive the anomalies.' (The Navy-Princeton Gravity Expedition to the West Indies in 1932)

"As far as observations could establish, the sea tides do not influence the plumb line, **WHICH IS CONTRARY TO WHAT IS EXPECTED**. Observations on reservoirs of water, where the mass of water could be increased and decreased, **GAVE NONE OF THE RESULTS ANTICIPATED ON THE BASIS OF THE THEORY OF GRAVITATION**. (A. Berget, Paris C.R. 116, (1893), pp. 1501-03)"⁶⁰⁸ (Capitalization added)

Here, Velikovsky *has presented* "facts" that obviously contradict the theory of gravitation just as Bauer requires; but Bauer, to dismiss this contradiction studiously, ignores this "factual evidence" as it pertains to this argument, for Bauer mentions that in another connection.

Gravity is only related to mass. The more mass in a region the greater must be its gravity. This is so simple and so basic that anyone can understand this concept. Therefore, gravity measurements over continents made up of rock which is about three times more massive than water, according to the fundamental tenets of gravitational theory, should show stronger gravitational pull than over oceans. However, it shows just the opposite. The high tides over the ocean should be more massive than low tides, and therefore, pull a plumb line toward the gravitational center of the high tide. But the greater water mass of high tide shows no such agreement. This is a further fundamental contradiction to gravitational theory. Finally, reservoirs can be filled with water and thus, be more massive than the same reservoir when it contains much less water and hence much less mass. Based on the fundamental tenets of gravitational theory, the full reservoir should affect the gravimeter and pull the plumb line more than the less full or nearly empty reservoir, but contrary to what gravitational theory categorically demands, no such agreement is found to be exerted.

The point I wish to emphasize is that Bauer raises the specific point that for Velikovsky he requires that "one must adduce facts that contradict the theory" leaving the impression that Velikovsky did not do this, when, in fact, that is precisely what Velikovsky did! Then Bauer raises a rhetorical argument but never ever mentions the "fact" that in point 10 of *Cosmos Without Gravitation* Velikovsky documented three contradictions to gravitational theory. In other words, when Bauer cannot present evidence that Velikovsky is in error on points 6 through 25, he argues from rhetoric and not from evidence. Even when he argues from evidence for Velikovsky to adduce facts that contradict the theory, and Velikovsky does so, Bauer simply ignores these contradictions. Velikovsky is damned if he does and damned if he doesn't play by Bauer's rules. Because of this behavior and the many misrepresentations presented above, I strongly suggest that Bauer plays by no rules of scholarly evidence or argumentation.

For example, Bauer on a similar point argues, "He [Velikovsky] does not distinguish between a situation in which some influence is calculably or observationally so small that it can be neglected, and a situation in which a principle, law, or force is not considered at all. Yet the making of precisely such distinctions is crucial to and fundamental in scientific activity."⁶⁰⁹

But isn't this precisely what Velikovsky showed in problem 10. He showed that the mass of water in the oceans or land over continents, and water in reservoirs can be calculated, especially in reservoirs and thus cannot be neglected in analyzing gravitational theory and what the theory demands. And this is precisely what is crucial to and fundamental in scientific activity, but Bauer refused to play by the rules he sets up for Velikovsky on this very point.

⁶⁰⁸I. Velikovsky, *Cosmos Without Gravitation*, (1946), p. 7.

⁶⁰⁹*Beyond, op. cit.*, pp. 118-119.

Bauer further argues, "Forces other than gravitation evidently exert an effect that is negligible compared to that of gravity and consequently they are not taken into consideration."⁶¹⁰ Here Bauer is discussing "planets and . . . their satellites."⁶¹¹ How do we know that this is correct? The only evidence for this statement is once again Bauer's word. Are the effects, say, of electromagnetism so negligible that "they are not taken into consideration"?

In 1981 three years prior to the publication of Bauer's book on Velikovsky, B. M. Vladimirkij, *et al.*, measured the behavior of the Earth's magnetosphere as it relates to the oscillations of the sun which pulsates up and down with a period of 160.01 minutes. What these researchers discovered is that the geomagnetic field also oscillates in its electrical components with a 160 minute micropulsation.⁶¹²

In 1984, the year Bauer published his book, G. P. Pil'nik measured the rotation of the Earth with atomic clocks and his measurements showed that this solar oscillation was reflected in the speeding up and slowing down of the Earth's rotation every 159.56 minutes.⁶¹³ But Bauer has told us such forces are so negligible they are not taken into consideration. If that is so, why did these Russian scientists take them into consideration and measure them precisely? Apparently, they did not know that Bauer would say they were doing precise scientific measurements in which this force's "influence is calculably or observationally so small that it can be neglected."⁶¹⁴ But these scientists were not paying attention and found that the force could be both calculated precisely and should not be neglected! The force affected the Earth's rotation!

Not only did these scientists measure these effects of solar electromagnetism on the Earth but so did others who also did not *heed Bauer's* pronouncements on this matter. In 1981 prior to Bauer's criticisms of Velikovsky, D. Djurovic measured this same relationship and showed a direct correlation of the Earth's rotation with well-known solar variations of periods of 0.5, 3.3, 6.6 and 11 years.⁶¹⁵ That is, the Earth's rotation speeded up and slowed down in precise relation to the behavior of the sun's cycles. But Bauer says these electromagnetic findings are neglected. Perhaps he should have a good talk with Djurovic and explain to this scientist that he ought to neglect this finding! In 1982, again prior to Bauer's publication on Velikovsky, F. Carter, *et al.*, measured the sun's electromagnetic activity and emphasized that the data showed a possible relationship with the Earth's *seasonal rotation*.⁶¹⁶ What Carter was showing is that the seasonal rotational variations assumed to be caused by wind patterns were fairly closely correlated with solar activity. Finally, in 1983 prior to Bauer's work, Djurovic measured three and four month georotation periods and solar activity and showed that indeed there was a correlation between these phenomena⁶¹⁷

All of these precise measurements of planetary rotation were carried out and presented before Bauer published his criticism and each confirmed in complete contradiction to Bauer that these forces are calculable, precisely measurable and not neglected at all by scientists. James S. Trefil, in a popular book, admits "the solar wind [the sun's electromagnetic field] can alter the length of the day."⁶¹⁸ Again, Bauer has not done his homework; he presents his ignorance as fact, then tells us yet again that this "misleading" argument, given here by Velikovsky,

⁶¹⁰*Ibid.*, p. 118.

⁶¹¹*Ibid.*

⁶¹²B. M. Vladimirkij, *et al.*, "Oscillations in the Magnetosphere of the Earth with a Period of 160 Minutes Caused by the Pulsation of the Sun," *Bulletin of the Crimean Astrophysical Observatory*, Vol. 64, (1981), as abstracted in *Astronomy and Astrophysical Abstracts*, Vol. 31, Part 1, (1982), p. 296.

⁶¹³G. P. Pil'nik, "Multiple Waves in the Earth's Diurnal Rotation," *Soviet Astronomy*, Vol. 28, No. 1, (1984), pp. 112-114.

⁶¹⁴*Beyond, loc. cit.*

⁶¹⁵D. Djurovic, *Astronomy & Astrophysics*, Vol. 100, (1981), pp. 156-158.

⁶¹⁶F. Carter, *et al.*, "A Comparative Spectral Analysis of the Earth's Rotation and Solar Activity," *Astronomy & Astrophysics*, Vol. 114, (1982), pp. 388-393.

⁶¹⁷D. Djurovic, "Short-Period Geomagnetic Atmospheric and Earth Rotation Variations," *Astron & Astrophys*, Vol. 118, No. 1, (1983), pp. 26-28.

⁶¹⁸James S. Trefil, *Spacetime Infinity*, (New York, 1985), p. 159.

is again typical of much of his writing."⁶¹⁹ The reality is that this "misleading argument, given here by Bauer, is again typical of much of his writing!!"

Undaunted, Bauer goes on.

"Another example of Velikovsky misleading in this manner occurs when he quotes 'H. N. Russell' and comments upon the quote: 'An atom differs from the solar system by the fact that it is not gravitation that makes the electrons go around the nucleus, but electricity' [Velikovsky adds] 'Different principles are supposed to govern the motion of the planetary bodies in the macrocosm and microcosm' Scientists do *not* suppose that 'different principles' are involved. Within atoms electrical forces outweigh gravitational ones to the extent that the latter can be neglected for the purpose of even very exact calculation and vice versa in the case of solar systems."⁶²⁰

Bauer has again accused Velikovsky of being MISLEADING!

Again, what is the evidence that scientists neglect the gravitational forces in atoms because they are outweighed by electromagnetism? Again this is based only on Bauer's word. But do scientists neglect these forces in analyzing the motion and behavior of the gravitational subatomic particles in atoms? Once again, Bauer is dead wrong because these forces are measured, calculated and taken into account in atoms, as I was informed by Mr. Joel Canepa. T. Uzer, *et al.*, in the journal *Science* in an article titled "Celestial Mechanics on a Microscopic Scale," tell us not only that classical gravitational, celestial mechanics explain motions in atoms but that they do so more precisely in some cases than does quantum theory based on electromagnetic forces.

"Classical and semiclassical [gravitational] methods are unrivaled in providing an intuitive and computationally tractable approach to the study of atomic, molecular and nuclear dynamics. An important advantage of such methods is their ability to uncover in a single picture underlying structures that may be hard to extract from the profusion of data supplied by detailed quantum calculations."⁶²¹

These scientists go through a litany of cases wherein gravitational, classical mechanics is employed in analyzing the behavior within atoms. Why aren't these scientists neglecting classical, gravitational, celestial mechanics inside the atom as Bauer says they do? Why did the journal *Science* and its editors not neglect these forces, since they are so small compared to electromagnetism that they are, as Bauer suggests, usually neglected? The reason they are not neglected is that they provide important explanations of the behavior inside atoms, in some cases, more accurate than quantum theory based on electromagnetism. Although this paper was published after Bauer wrote this criticism of Velikovsky, in the footnotes these researchers provide a list of papers on this and related subjects, going back in time well before Bauer published his book. Yet Bauer accuses Velikovsky of being "apparently ignorant of the existing description of these forces, although the relevant work predates his suggestion by a decade or two."⁶²² But Bauer is, in fact, the one who is ignorant of the existing description of the force of gravity operating in atoms, although the relevant work predates his criticism by several decades!

Here once again Bauer may wish to speak to these scientists and the editors of the journal *Science* to show them that, like Velikovsky, they are not only misleading the public, but the scientific community as well, by

⁶¹⁹*Beyond, loc. cit.*

⁶²⁰Bauer, *op. cit.*, p. 119.

⁶²¹T. Uzer, David Farrelly, John A. Milligan, Paul E. Raines, Joel P. Skelton, "Celestial Mechanics on a Microscopic Scale," *Science*, Vol. 253, (July 5, 1991), p. 42.

⁶²²*Beyond, op. cit.*, p. 120.

presenting information that gravitational forces, which Bauer claims are neglected in such work, has not been neglected by them. With respect to atoms and solar systems, Bauer states:

"Moreover, electrons are not comparable to or analogous with planets. [and] . . . there is nothing in the behavior of planets that can—let alone needs to—be described by wavelike equations or calculations."⁶²³

Bauer now suggests that scientists do not offer the idea that planets in the solar system are comparable to electrons in atoms or that they describe the behavior of planets by wavelike equations and calculations. Has Bauer presented a single citation to support his claim? No, not at all. Again he asks his readers to take this information on faith in his word. But what does the scientific literature actually say about the question of planets being similar to electrons in atoms. A. E. Caswell in the journal *Science* did not see this prohibition by Bauer and had the gall to write:

"The writer has discovered another simple relation between the planetary distances, [other than that of the Titus-Bode law] and so far as he is aware this relation has not been reported hitherto. It suggests the possibility that the orbits of the planets may be 'quantized' somewhat after the number of electronic orbits in the Bohr atom. For this reason it may prove to have some theoretical importance."⁶²⁴

William M. Malisoff later improved on and corrected Caswell's work and then had the unmitigated nerve to say that the motions of the planets exhibit a wave of velocity contrary to the proclamation Bauer has presented on this matter,

"The writer would point out a relation that depends *strictly* on the square root of the distance of a planet from the sun or a satellite from its planet. It is the velocity which varies inversely as the square root of the distance from the axis of revolution. For the planetary system one could then state as a law. *The velocities of the planets are inversely in proportion to simple integral numbers* (Malisoff's emphasis)

". . . we may conceive the propagation as A WAVE OF VELOCITY at the initiation of revolution to follow the law of a logarithmic spiral."⁶²⁵ (Capitalization added)

J. B. Penniston thereafter showed that all the planets and their satellites obey these "quantized" mathematical relations outlined by Caswell that reflect the Bohr atom.⁶²⁶

Petr Beckmann, in his book *Einstein Plus Two*, has also derived equations from physical laws that "lead . . . to agreement with observations for both electrons and planets, that is regardless of whether the field propagating from its source is electric or gravitational" applies to atoms and solar systems.⁶²⁷

⁶²³*Ibid.*, pp. 122-123.

⁶²⁴A. E. Caswell, "A Relation Between the Mean Distances of the Planets from the Sun," *Science*, Vol. 69, (1929), p. 384.

⁶²⁵William M. Malisoff, "Some New Laws for the Solar System," *Science*, Vol. 70, (1929), pp. 328-329.

⁶²⁶J. B. Penniston, "A New Law of Satellite Distances," *Science*, Vol. 71, (1930), pp. 512-513.

⁶²⁷Petr Beckmann, *Einstein Plus Two*, (Boulder, Colo., 1987), p. 184.

In addition, John P. Bagby has actually written an equation that not only derived the Titius-Bode Rule of planets and their satellite distances but can also "accommodate the atomic Bohr Law as well . . . [His results] show how the Constant of Gravitation, Planck's Constant and Bohr integer squared relationship can be combined with the mass and density of any primary body to define electrical and gravitational orbit[al]s. This should go a long way towards the ultimate goal of a unified Field Theory."⁶²⁸

J. Banorthy, in an article in *Nature*, has written an extremely provocative equation regarding the motions of the planets. It contains the fine structure constant of the Bohr atom and, significantly, the Pauli Exclusion Rule which states no two electrons can have the same numerical values unless they rotate in opposite directions. When Banorthy applied this equation to the planets he found Saturn and Uranus have the same quantum numbers which fit Uranus' retrograde rotation and Saturn's prograde rotation. The same principle applied to the Earth and Venus. Since the Earth rotates in the prograde direction, Banorthy's equation predicted that Venus rotates in the retrograde direction, which was confirmed almost two decades later.⁶²⁹

In the journal *Nature* there were also presented two articles on this concept both titled "Parallelism Between Atomic and Planetary Properties."⁶³⁰ Each of these many researchers seems to have missed Bauer's boat which states "electrons are not comparable or analogous with planets" [and] "there is nothing in the behavior of planets that can—let alone need to—be described by wavelike equations and calculations."

Dennis Overbye further informs us that Stephen Hawking stated "he wanted now to really quantize gravity . . . In a recent controversial paper, he and [Gary] Gibbon had extended the notion of Hawking radiation beyond black holes to the expanding universe." Hawking, one of the world's most famous physicists, is also applying quantum physics which is used to explain the nature of atoms to the bodies of the universe. Scientists do believe that, at a fundamental level in the future (as Bauer would

say) we will discover, explain and understand the unified law that applies to atoms and solar systems. As Albert Einstein wrote in *Sidelights, Geometry and Experience*, page 22:

"Of course it would be a great advance if we succeed in comprehending the gravitational field and the electromagnetic field together in one unified conformation. Then for the first time the epoch of theoretical physics founded by Faraday and Maxwell would reach a satisfactory conclusion. The contrast between ether and matter would fade away and through the general theory of relativity, the whole of physics would become a complete system of thought, like geometry, kinematics, and the theory of gravitation."⁶³¹

Einstein, in his final years, sought to work with gravity and/or relativity and electromagnetism to unite these forces. To do so would lead to a single set of laws that would govern atoms and solar systems. Why should Velikovsky be condemned for presenting views which Beckmann, Hawking, Einstein, and others are applying to atoms and solar systems? Are they all being misleading in doing this?

But let us proceed with more of Chapter 7. Bauer cites Velikovsky's paper, "The Velocity of Light in Relation to Moving Bodies." I will cite the two paragraphs from which Bauer drew his data and then quote and analyze Bauer's attack on Velikovsky's statement.

"The Michelson-Morley experiment performed in 1886 demonstrated that a beam of light that issues from a terrestrial source and travels in the direction of terrestrial motion, East-West-East or West-East-West, needs the same time to traverse a laboratory distance as a beam that

⁶²⁸John P. Bagby, "A Comparison of the Titius-Bode Rule with the Bohr Atomic Orbitals," *Speculations in Science and Technology*, Vol. 2, (1979), p. 173.

⁶²⁹J. Banorthy, "Quantization of the Solar System," *Nature*, Vol. 157, (1946), p. 808.

⁶³⁰*Nature*, Vol. 161, (1948), p. 780 and *Nature*, Vol. 162, (1948), p. 409.

⁶³¹J. J. Callahan, *Euclid or Einstein*, (New York, 1931), P. 261.

travels at right angles to that motion (North-South-North or South-North-South). *The undulatory theory of light transmitted by waves in the ether* anticipated detection of a difference in the velocities of the two beams due to the orbital velocity of the earth through absolute ether-filled space. Half a year later, when the earth was on the opposite side of its orbit, the same experiment again disclosed no difference in the velocities of the beams; any possible compensatory motion on the part of the solar system or the entire galaxy thus was excluded.

"The explanation first offered was the supposition that any material object (also a measuring rod) travelling through the ether is shortened by a very small amount; the East-West distance in the laboratory apparatus (interferometer), being shorter, is traversed by a beam of light travelling a little slower in the same time that the North-South distance is crossed by a swifter beam (1), Einstein, however, generalized this idea by assuming that the velocity of light in vacuum is constant in relation to all bodies, whether in motion or at rest. *The ether was discarded in the Special Theory of Relativity and Einstein embraced the quanta theory of light.* Both space and time lost the attribute of constancy and light (its velocity) acquired it."⁶³² (Emphasis added)

On the basis of these paragraphs by Velikovsky, Bauer claims "This implies that the 'undulatory' and 'quanta' theories were antithetical" according to Velikovsky.⁶³³ On what basis could anyone ever suggest this is so? Velikovsky never suggested that because Einstein dropped the idea of an ether in which light was believed to travel, for the concept that light travels in a vacuum that the wave or undulatory light theory and quantum theory are contradictory. In fact, Velikovsky never mentions quantum theory anywhere in these paragraphs with regard to ether theory or vacuum concepts. But Bauer accuses him of suggesting that quantum theory contradicts the undulatory theory. Bauer is putting an idea into Velikovsky's writing which is simply not there and then attacking that strawman! The reader will find that the *italicized* parts of Velikovsky's two paragraphs are all that Bauer quotes to misrepresent Velikovsky.⁶³⁴ Please read only the words in italics to see what Bauer has done.

The sentences are separated into two distinct paragraphs and are not in any way related together to what Bauer is suggesting. It is very clear that Velikovsky did say Einstein rejected the ether concept for the vacuum concept in which light travels, but he never implied, suggested or even claimed that undulatory and quanta theories are antithetical. Therefore, Bauer did not infer this from Velikovsky text; he has misrepresented Velikovsky's text. To attack Velikovsky for something he never stated in this citation is simply dishonest.

This is exactly what Janet Malcolm did in a report on Jeffrey M. Masson. She pieced together different parts of Masson's material to misquote him, which is clearly dishonest. Geoffrey Stokes, in *The Village Voice*, concluded that:

"The sort of Selective (mis)quotation calls Malcolm's good faith into question, and it is only on an almost preternatural amount of good faith that readers can rely when reporters start creating composite quotations. The net effect of what Malcolm has done . . . seems to me far worse than the comparatively minor atmospheric details . . ." ⁶³⁵

Sanskrit scholar, Robert Goldman, put the case more bluntly:

⁶³²Immanuel Velikovsky, "The Velocity of Light in Relation to Moving Bodies," *Pensée*, Vol. 3, No. 3 (Fall 1973), p. 16.

⁶³³*Beyond, op. cit.*, p. 129.

⁶³⁴*Ibid.*

⁶³⁵Jeffrey M. Masson, "Preface to the Penguin Edition," *The Assault on Truth*, (New York, 1985), pp. XVIII-XIX.

"Her article provided Masson's critics both with the precedent for ignoring his discoveries (while focusing on his personality) and with the ammunition for attacking him—his credentials, his competence, and his motives—without the need to conduct any independent inquiry of their own. To be blunt: Malcolm's account . . . is a tendentious, dishonest, and malicious piece of character assassination, all the more pernicious because of its studied tone of . . . detachment."⁶³⁶

As I pointed out earlier, Bauer decided to evaluate the *proposer*, Velikovsky, and not, by and large, his *proposal*, as the mechanism of his critique on these materials. But, in fact, *his* "composite quotation" of Velikovsky on ether theory again shows his account is also a tendentious, dishonest, and malicious piece of character assassination, all the more because of its studied tone of detachment. Goldman's assault on Malcolm applies in full to Bauer's work on Velikovsky. It focuses on his personality, competence and motives and misrepresents his words by selective editing. All through his criticism I have shown this to be the case over and over again. Who can trust such a critic?!

But let us continue with more of Chapter 7.

Bauer attacks Velikovsky's statement, ". . . in the deposits of the Gulf of Mexico the age of oil is measured in thousands of years not millions . . . This destroys the main argument the geologists have raised against the theory of exogenous origin of some deposits of oil. (*Worlds in Collision*, pages 53-58, 369)." To this Bauer replies "Not so!"⁶³⁷

Now in the rest of Bauer's discussion which we will get to below, he never deals with the fact of this young carbon 14 date for the oil, although he does mention the 9,000 year old date cited by Velikovsky. If oil is millions of years old then after thousands of half lives of radioactive carbon breakdown there should not be any measurable carbon 14 left in oil at all! The rest of his discussion simply evades squarely facing this fundamental contradiction to his argument. Bauer instead argues,

"One of the chief arguments against Velikovsky's suggestion that oil plummeted down and then seeped into deposits is that oil, lighter than water, has in fact been pressed *upward* through porous formations to be held in basins under non-porous rocks, and could not have seeped *down* through the latter."⁶³⁸

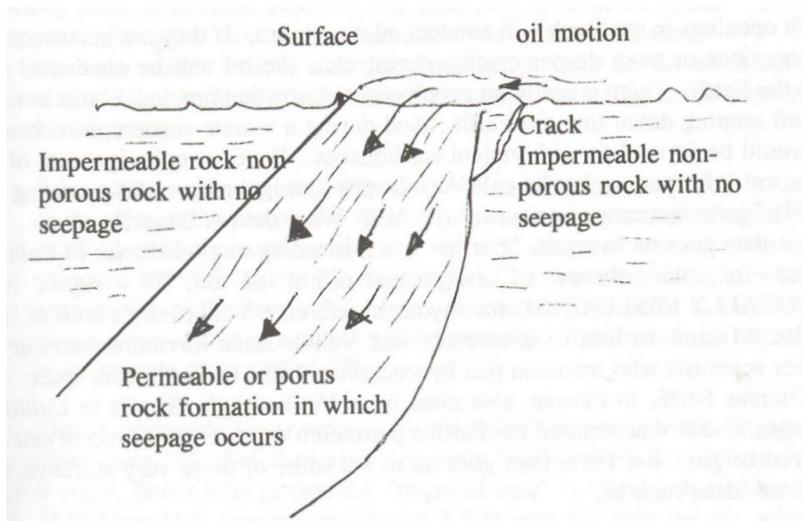
Bauer has cited a book review by K. S. Latourette, *et al.* in the *American Journal of Science*, 1950, page 588 for this item of evidence. But this is utter nonsense and any third rate geologist could see through the transparency of such egregious ignorance. Oil like water is a liquid and even Bauer claims that it can be "pressed *upward* through porous formations" or porous rock. What Bauer is ignorant of is the nature of artesian formations and how liquids move through them. This, in fact, is *junior high school level work*. Porous rock often lies in such a way that only one area of the formation is at or near the surface while the rest of the formation slopes down and away from the surface. See Figure 1.

⁶³⁶ *Ibid.*, p. XIX.

⁶³⁷ *Beyond, op. cit.*, p. 130.

⁶³⁸ *Ibid.*

Figure 1



When it rains at the surface the water will seep downward through the porous rock to great depths. The very same thing will also happen to other liquids, including oil! To suggest on the one hand that porous rock formations will allow oil to seep upward but not downward is not valid geology. And if Bauer thinks that this will not, nor cannot occur, then I suggest he read about artesian formations and the movement of liquids in them from a good junior high school Earth Science book!!!

Velikovsky claimed that "some oil" was recently brought to the Earth and that in the past, other comets brought "some oil" to other regions of the Earth. In each time period oil can certainly penetrate and pass downward through porous artesian rock strata much deeper into the Earth, and in time such strata could be buried beneath newly formed layers of non-porous rock. All Bauer had to do to learn about this was consult with some honest geologists who were not bent on assassinating the character and work of Velikovsky.

In addition, there are thousands upon thousands of cracks in the Earth that are far too small for humans to enter, but run deep into the Earth. According to Corliss,

"It turns out that the Earth beneath our feet is not so solid after all. Some 40,000 caves are known in the United States alone. There are thought to be ten times that number [400,000] that have no surface opening and therefore escape spelunking census takers. And besides caves big enough for humans to crawl into, there exists an immensely greater continuum of cracks, crevices, channels and pores which circulate air, water and chemicals in solution. This 'crevicular structure' may be continuous for thousands of miles possibly around the world."⁶³⁹

All this information about cracks and crevices in the Earth has been known for decades. Bauer's argument on this point again shows he doesn't know what he is talking about!

Such openings in the Earth will conduct oil downward. If they are in contact with artesian formations or even deeper cracks, pores, etc., the oil will be conducted even deeper into the Earth. There is really no geophysical reason that precludes large amounts of surface oil seeping down into the Earth. And during a violent catastrophe enormous crevasses would be formed as with violent earthquakes. Bauer is quite ignorant of this basic geological information, but he calls Velikovsky "an ignoramus masquerading as a sage" (p. 94) "quite ignorant of science." (p. 319) What then is Bauer?!

⁶³⁹William R. Corliss, *Science Frontiers*, (Glen Arm, Md., 1994), p. 177.

Bauer then goes on to argue, "Further it is misleading to give *Worlds in Collision* as reference for 'the' theory of exogenous origin of oil."⁶⁴⁰ Again it is UNEQUIVOCALLY MISLEADING for anyone to reference Velikovsky's book as "the" reference that oil came to Earth via comets. But Velikovskian literature does cite the work of other scientists who maintain that hydrocarbons came to Earth from space. For example, Thomas Ferté, in *Pensée*, also cites from Velikovsky's *Worlds in Collision*; Naphtha, pages 53-58, that some of the Earth's petroleum deposits are of recent date and extraterrestrial origin. But Ferté then goes on to list some of those very scientists who presented these ideas such as,

"A. T. Wilson claimed an extraterrestrial origin for *all* the Earth's oil. "Origin of Petroleum and the Composition of the Lunar Maria," *Nature*, 196, (October 6, 1962), pp. 11-13; J. Oro and J. Han describe how petroleum can be formed through the interaction of comets and planets. "High-Temperature Synthesis of Aromatic Hydrocarbons and Methane," *Science*, 153, (September 16, 1966), pp. 1393-5."⁶⁴¹

No one among the Velikovskians would doubt that in 1877 Mendeleev, the great Russian chemist who devised the periodic table of elements, published a work in which he gave his reasons for believing oil came from deep within the Earth.⁶⁴² The one who is misleading his readers is again Bauer who clearly must have seen discussion of Oro and Han's work in this book, *The Age of Velikovsky*, by C. J. Ransom page 122 which he cites, but fails to discuss, the fact that all these scientists who contributed to the view of the extraterrestrial origin of oil are presented in Velikovskian literature.

Bauer then chides Velikovsky again for being "misleading in implying that a date of 'thousands of years' make his theory in any way more plausible: the dating was about 9,000 years, whereas Velikovsky's scenario *would require* a date of about 3,500 years."⁶⁴³ (Emphasis added)

But, now it is known in the Guaymas Basin in the Gulf of California that oil has been found which is only about 4,240 years old. However the dating leaves open the possibility that the actual date of the oil may be even 500 to 3000 years younger than that for very specific reasons. First, the organic material that was carbon 14 dated apparently took many years to become mixed in and incorporated with the strata. Second, dating may appear to be older from older material mixed in that sediment.⁶⁴⁴

Does the reader believe that this date, which falls within the 3,500 year date "required" by Bauer, will satisfy this man? Of course not, as Bauer perhaps anticipating such a finding argues,

"The method (carbon-14) used for measuring the age [of oil] is based on the premise that the oil was formed from once-living material (vegetation) under normal terrestrial conditions—a very different history from that proposed by Velikovsky. No date obtained by this method could provide support for Velikovsky's hypothesis unless he first gives a calculation showing the amount of carbon-14 to be expected in oil formed and deposited in the manner he described."⁶⁴⁵

This is neurotic! Bauer tells us "Velikovsky's scenario *would require* a date of about 3,500 years."⁶⁴⁶ But then after requiring Velikovsky's scenario to have a date of 3,500 years, Bauer tells us that the "required date" is of

⁶⁴⁰*Ibid.*

⁶⁴¹Thomas Ferté, "A Record of Success," *Pensée*, Vol. 2, No. p. 15.

⁶⁴²Thomas Gold, *Power From the Earth*, (London, Eng., 1987), p. 3.

⁶⁴³*Beyond, op. cit.*, pp. 130-131.

⁶⁴⁴Jeff Hecht, "Youngest Oil Deposit Found Below Gulf of California," *New Scientist*, (April 6, 1991), p. 19.

⁶⁴⁵*Beyond, op. cit.*, p. 131.

⁶⁴⁶*Ibid.*

no value! One then is led to ask, if Velikovsky's scenario requires a 3,500 year old date for oil, why is such a "required" date obtained by this method worthless? Yes, indeed! The logic is that Velikovsky must provide a 3,500 year old date for oil, but at the same time, no date by this method could support his scenario. Then why does Bauer "require" a 3,500 year old oil date if it means absolutely nothing?! Because, Bauer is looking for impossible situations with which to confront Velikovsky. He requires a 3,500 year old date for oil and at the same time claims no date could support Velikovsky's hypothesis! To require something as validation which is at the same time of no value is "neurotic"!

But let us return to the oil with a 9,000 and 4,240 or younger carbon 14 dated age. This oil could not be produced millions of years ago and still present such a youthful date no matter what illogical arguments Bauer raises. What Bauer is not willing to face squarely is that the youthful date is in complete contradiction to the concept that the oil formed from vegetation is millions, or if formed from cometary materials—it should be billions of years old and again could never exhibit a youthful age. That is the crux of the matter related to the origin of oil. Oil millions or billions of years old should not give a radiocarbon date at all, and this does suggest it has a different origin and perhaps a recent origin!

Now to be fair to Bauer, Velikovsky did make mistakes in this paper which we touched upon. This cannot be denied! The kinetic theory of gases explains the mixing of the gases in the atmosphere; winds further do explain the mixing of gases in the atmosphere and gases do not separate into layers in the atmosphere gravitationally based on fundamental scientific evidence which Bauer, to his credit, did present. But Velikovsky's failings in these areas are more than matched by Bauer's falsehoods. And ultimately this has nothing to do with Velikovsky's historical and cosmological theory. Velikovsky did, in fact, say that his theory could also be explained by gravitational theory although he questioned it as the only force in celestial motion.

If someone now wished to attack Bauer's concept that the Loch Ness monster was a reality based on Bauer's own criteria, all he would have to do is use Bauer's worst errors and misrepresentations pointed out above and say this proves Bauer's incompetence and dishonesty in science. Therefore, his statements about Nessie should be seen as probably suffering from the same kind of problems. But Bauer and his supporters of Nessie would be the first ones to say this does not prove anything one way or the other about the reality of the Loch Ness monster. Yet this is Bauer's general approach to Velikovsky's theory.

What Bauer has argued for is that there must be precisely known phenomena which can then be rigorously calculated before a concept can be taken seriously by scientists and the scientific community. This point is made with regard to Velikovsky's advanced claim that Venus is hot. Bauer raises this argument with other logical statements to confront this issue.

"Velikovsky was correct that Venus is hot. But the high temperature of Venus can also be explained in terms of conventional wisdom; at the very least, no scientist has concluded that it cannot be."⁶⁴⁷

What does Bauer's statement mean? If a phenomenon can be explained by conventional wisdom then what may be implied is that one cannot take Velikovsky's assertion as possibly being correct. But what is also implied is that neither can one take conventional wisdom as correct. There are indeed explanations of phenomena accepted within the scientific establishment that can also be given by other explanations of the data, not only that of the accepted, favored explanation. As Paul Feyerabend the late philosopher of science, in complete contradiction to Bauer's statement, pointed out,

"For every statement, theory, point of view believed (to be true) with good reason *there exist* arguments showing a conflicting alternative to be at least as good or even better."⁶⁴⁸

⁶⁴⁷ *Beyond, op. cit.*, pp. 86-87.

⁶⁴⁸ Paul Feyerabend, *Farewell to Reason*, (New York, 1993), p. 76.

In fact, in science there are many areas in dispute wherein most scientists advocate a particular interpretation of the evidence. So how does one derive the correct interpretation? Here Bauer put his finger on the nub of scientific interpretations of evidence and the reason for preferring one explanation over those of its rivals such as Venus' high temperature,

"Velikovsky's explanation was offered in a manner that is not conducive to testing. In order to test it, one would ask: with what energy was Venus expelled (or fissioned off) from Jupiter? What was its temperature then? What is the average heat capacity (how much heat does it absorb or lose for a given change in temperature)? By what mechanisms and at what rate (based on those mechanisms) did it cool? If more heat was generated when it encountered the earth, how much? How was the heat distributed between Earth and Venus? And so forth.

"This is the argument of those who have said that Velikovsky's hypothesis cannot be tested because it is too vague, purely a qualitative and descriptive account. The argument does not prove Velikovsky wrong, but it does underline the fact that he has not presented a conclusive, testable chain of reasoning to support his prediction."⁶⁴⁹

I agree with Bauer and those who raised this argument, except that I can and will now point out a concept that is well established by the scientists within its discipline that underline the fact that this established concept is also too vague to be tested and cannot be presented by a conclusive, testable chain of reasoning to support the predictions of it. And, in fact, the predictions are contradicted by the findings at many levels but are nevertheless well accepted as correct.

The origin of comets is precisely one of these concepts. It has become a scientific establishment model that is extremely well respected offering that comets come from what is termed the Oort comet cloud surrounding the solar system. For example David Morrison and Tobias Owen in their textbook on astronomy state,

"The first such satisfactory theory for the origin of the comets was proposed by the Dutch astronomer Jan Oort in 1950 Oort noted that in all [known] cases where the orbits of new, nearly parabolic comets had been carefully determined, the orbits indicated on aphelion at a distance of approximately 50,000. A.U., a thousand times more distant than Pluto. Very few comets seemed to come from greater distances, and none showed evidence of originating outside the solar system in interstellar space. He therefore suggested the existence of a comet cloud associated with the Sun but existing far beyond the known planets. This grouping of comets is now called the Oort comet cloud."⁶⁵⁰

Robert D. Chapman and John C. Brandt state, ". . . the true mechanism of the [Oort] clouds origin may never be identified unambiguously."⁶⁵¹

Now one would wish to know if this generally accepted hypothesis is also testable or if it "was offered in a manner that is not conducive to testing." In order to test it one would ask, what is the precise chemical make up of the materials that accreted to form the comets, and how can we test this? By what precise manner were these comets in the inner solar system then ejected to this distant cloud surrounding the solar system and how can we test this?

Are there precisely known numbers of passing stars nearby to perturb this cloud and send comets into the solar system, and how can we test this? Is the method of comet capture by Jupiter and the other gas giant planets precisely known to be capable of capturing these comets, and how can we test this? Each of these questions has been shown by rigorous research to be fraught with serious unresolved problems. These have been discussed and outlined in many books and papers over the past decades; see for example Donald K. Yeomans' *Comets*, (New

⁶⁴⁹*Ibid.*, p. 87.

⁶⁵⁰David Morrison, Tobias Owen; *The Planetary System*, (Reading, Mass., 1987), p. 115.

⁶⁵¹Robert D. Chapman, John C. Brandt, *The Comet Book*, (Boston, 1984), pp. 53-54.

York, 1991) pages 313 to 344. Yeomans, a specialist in comet research, suggests on page 340 that because of these problems further research on the Oort cloud, the "generally favored . . . hypothesis . . . [may in time show] this area of study changes rapidly and today's favored hypotheses are often tomorrow's historical footnotes."

Now if Velikovsky's theory for Venus' heat is a chain of assumptions untestable by rigorous physical models, then the same clearly applies to that of the Oort comet cloud, in spite of all the theoretical material written about it. But, have the astronomers applied Bauer's standards and refused to "seriously" consider its possible validity as they have Velikovsky's explanation for Venus' heat? Of course not; the Oort comet cloud is a highly respected, and to a great measure, highly accepted concept.

The astronomers have been attempting to rigorously examine the Oort cloud model theoretically and see how well the theoretical evidence supports the theory, without success over the past 40 years. This does not mean that the theory is too questionable to be taken seriously, as Bauer suggested, for Velikovsky's theory for Venus' internal heat, "but it does underline the fact that [the scientific community] has not presented a conclusive, testable chain of reasoning to support [its] predictions" based on the Oort cloud model.

We do not know how the solar system formed, yet the Oort cloud model is based on an unproven assumption that the solar system began as a cloud that accreted into planets as well as comets. What we have is an unproven theory of the formation of the solar system upon which a second unproven theory, the one regarding the Oort comet cloud, is built. Why then haven't the scientists concluded that since the model is so highly hypothetical, it is not worth the bother of investigating but instead have raised its status to their favorite theory for the origin of comets? The reason is that the constraints Bauer has placed on Velikovsky's model do not pertain to how real science works and operates. If scientists are interested in a theory they will bring to bare all their measurements, physics etc. to investigate it. That is what has been done with the Oort cloud theory in spite of the presently highly hypothetical, untestable nature of it. The scientists *believe* it is useful but this is a very far cry from having "a conclusive testable chain of reasoning to support it."

The very same kind of untestable considerations exist for the origin of the solar system, the origin of the moon, and in several other areas of scientific research. But this has not stopped papers on these concepts being published in reputable journals and these theoretical proposals finding acceptance within large segments and even among majorities of the scientific community.

If, in the future, Velikovsky's hypothesis for the source of the heat of Venus became the favorite model of the scientists, wouldn't scientists tackle each of the questions Bauer has posed for Velikovsky's theory of Venus with the same sorts of theoretical tools? Again, no matter how well the models they created may fit Velikovsky's concept, it would be no different than what they are doing with respect to the Oort cloud model. In the future, it may be found from probes *beneath Venus' surface* that the heat of Venus *is* coming from its subsurface just as Velikovsky predicted. Since Bauer claims there is no "conclusive chain of reasoning" to support Velikovsky's theory, another theory will have to be found to explain the heat. It may be that it will be presented that an immense asteroid struck Venus and turned the planet inside out to generate its subsurface heat. But even this theory will be fraught with questions of rigor and testability. How big was the asteroid? How fast was it moving? How long ago did it strike the planet? etc., etc. Now none of these questions can be answered with direct knowledge or known precision, but they can be explored in much the same manner as is the Oort cloud model.

Science does not work in the manner Bauer has presented. If it did the Oort cloud theory would be placed in the same category as Velikovsky's theory for Venus' heat. Concepts that are not fully testable do find their place in science contrary to everything Bauer has suggested. Will Bauer present a conclusive chain of reasoning to support the Oort Cloud Theory? Of course not. Conclusive evidence for this theory does not exist.

The basic underlying fallacy of Bauer's approach in his criticism is that although he pays lip service to rationality and open-mindedness in science and admits one cannot define science, he believes he understands the rules for scientific research and how one must approach a scientific concept. This fallacy becomes transparent in his advice to someone in Velikovsky's position.

"Consider the courses of action open to a man in Velikovsky's position. In 1940 he conceived the idea that the plagues of Egypt were actually physical occurrences, rather faithfully described in the *Bible*. Searching in various places, he came upon a translation of the Papyrus Ipuwer and was struck by how similar the events described there were to those in the biblical

account. Could these be the same event? On the face of it, no—because the papyrus predated the Exodus by several hundred years. Could the dating of the papyrus be wrong? At this stage our imaginary scholar would have attempted to find some independent evidence to alter the dating of the papyrus. But Velikovsky did not do that. He *postulated* the different date and then proposed yet further thorough going revisions of accepted chronology."⁶⁵²

Here Bauer is again omitting evidence! What Velikovsky did was indeed attempt to find some independent evidence not only to alter the dating of the Egyptian dynasty purported to be earlier than the Exodus catastrophe he posited, he also attempted to find independent evidence for the reconstruction of ancient history. And this evidence he presented in his *Ages in Chaos* series of books on the chronology of the ancient world. Whether or not one agrees with Velikovsky's reconstruction of this chronology, he put most of his effort and research into this revision. To say as does Bauer that he merely postulated a different date is thoroughly misleading. He did much more than just present a new date; he spent most of his life thereafter researching this material and explaining in book after book the correlations he had found that supported his thesis. Bauer once again, without citation or a shred of support for his assertion, states that the Ipuwer "papyrus predates the Exodus by several hundred years." How does he know this when as Velikovsky showed in 1952, thirty-two years prior to this statement by Bauer, that scholars do not agree on the date of the Exodus completely contrary to Bauer's statement? As Velikovsky distinctly pointed out in one of the books Bauer claims to have read, *Ages in Chaos*: At the very beginning of the book, Velikovsky presented an entire chapter to deal with the arguments historians have had with dating the Exodus. But Bauer acts as if this lengthy evidence does not exist.

Now what did a scholar of the *Bible* and ancient history have to say about Velikovsky's use of the Ipuwer Papyrus as a description of the Exodus? Robert H. Pfeiffer, late Chairman of the Department of Semitic Languages and History of Harvard University, wrote:

"Among the impressive findings which the exhaustive research [by Velikovsky] has brought to light is the story of the plagues which preceded the Exodus."⁶⁵³

Was Pfeiffer so ignorant that he did not understand, as did Bauer, that Velikovsky's research was inept or in some way inappropriate? Pfeiffer knew that the history of the ancient Near East was not settled and that Velikovsky's identification was a reasonable postulation. He also felt that Velikovsky's exhaustive research to recast ancient history was neither eccentricity nor pseudoscience, as Bauer implies. According to Velikovsky,

"Professor J. Garstang, excavator of Jericho, read an early draft of the first chapter. It was his opinion that the Egyptian record of the plagues, as set forth in this book, and the biblical passages dealing with the plagues are so similar that they must have a common origin."⁶⁵⁴

Was Garstang so ignorant that he did not understand, as did Bauer, that Velikovsky's research was inept or in some way inappropriate? Garstang, a researcher into these periods of ancient history, came to exactly the same conclusion as Velikovsky! Velikovsky also cited Dr. Walter Federn of Asia Institute in New York. He came to the conclusion "that conventional history is not built on unshakable foundations."⁶⁵⁵ Were all these men incapable of seeing that, as Bauer appears to imply, the foundations of ancient history are truly settled? Charles Pellegrino, admittedly not in the same class as Pfeiffer, Garstang and Federn, has come to the conclusion that,

⁶⁵²*Beyond, op. cit.*, p. 90.

⁶⁵³Robert H. Pfeiffer, in Immanuel Velikovsky, *Ages in Chaos, op. cit.*, blurb front cover.

⁶⁵⁴*Ibid, op. cit.* "Acknowledgments," p. XIII.

⁶⁵⁵*Ibid.*

"Ipuwer echoes throughout the Book of Exodus, sometimes with such spine-chilling fidelity that I am tempted to believe both accounts were copied from the same texts." [He adds after presenting much the same material as Velikovsky did, sadly without giving Velikovsky credit for this work] "the events described here [in Ipuwer and Exodus] and the language used are virtually identical."⁶⁵⁶

The final point on this issue that must be emphasized was presented by Lewis M. Greenberg in 1973. He showed that a well respected historian, John Van Seters, in the *Journal of Egyptian Archaeology*, Vol. 50, (1964), pp. 13-23, carefully analyzed the Ipuwer Papyrus and "arrived at a date identical with Velikovsky's."⁶⁵⁷ Lewis M. Greenberg then goes on to cite Van Seters' work wherein the historian feels so strongly about the dating of this document that he claims that, "it seems that the burden of demonstration rests on those who would still maintain an early date" for the Ipuwer document. Now, why didn't Bauer, in discussing the Exodus and the Ipuwer Papyrus, tell us that an historian dated the document precisely to the time Velikovsky dated it? Why did he attempt to suggest that there was no evidence in modern historical literature that fully supports the dating Velikovsky offered? The exclusion of this material by Bauer is once more the same miserable, ignorant misrepresentation found in so much of his other criticism.

What this shows is that Velikovsky was not hasty or wildly incorrect in concluding that neither the Exodus nor the Ipuwer Papyrus were properly dated within the framework of Egyptian chronology. Does this careful research show that he made his decision without careful, in-depth analysis of the historical literature that existed? Bauer's approach, as has been demonstrated repeatedly, is to make an unsubstantiated pronouncement about the evidence, in this case to the dating of historical events about which recognized authorities express agreement with Velikovsky; Bauer suggests his pronouncement is unquestionably correct. But this is not what the many authorities cited by Velikovsky suggest.

Bauer's approach is that of an authoritarian who assumes the textbooks are absolutely correct. As Gerard Gilmore explains:

"Everything I learned I learned by reading journals, so I learned from the bottom up what a great deal of dissention and disagreement there is about most things . . . rather than learning from textbooks, which give you a completely wrong impression that answers are precise and well understood."⁶⁵⁸

Again, the question of Bauer's dating of the Ipuwer Papyrus is based on the assumption that Egyptian history is also firmly and precisely dated. Here, also, Velikovsky did his homework to evaluate this aspect of the historical equation. He presented strong evidence in the supplement to his book, *Peoples of the Sea*, (New York, 1977), pages 205 to 244, that the foundations of Egyptian chronology are in error. In fact, one of these foundation stones criticized by Velikovsky, namely—astronomical Sothic dating—has already been abandoned by certain Egyptologists.

"Work on chronology has clearly arrived at a crisis. The reason for this is in part adoption of dogmatic [Sothic] scientific facts without testing their applicability to Egyptian material and the reliability of this material."⁶⁵⁹

⁶⁵⁶Charles Pellegrino, *Return to Sodom and Gomorrah*, (New York, 1994), pp. 214-215.

⁶⁵⁷Lewis M. Greenberg, "The Papyrus Ipuwer," *Pensée*, Vol. III, (Winter 1973), p. 36.

⁶⁵⁸Ken Crosswell, *The Alchemy of the Heavens*, (New York, 1995), p. 147.

⁶⁵⁹W. Helck, "Zur Lage der Ägyptischen Geschichtsschreibung" (resume) in S. Schoske, ed. 4, *Internationaler Ägyptologenkongress*, 26, 8-1, 9, 1985, *München [sic] Resumés de Referate*, Munich, Germany, 1985, p. 95.

Though other historians debate this conclusion, it does indicate Egyptian ancient chronology is not solidly established contrary to Bauer's suggestion. While Willy Ley writes of the Sothic Period for dating Egyptian chronology, "It was all a later invention; the Egyptians did not use such a period."⁶⁶⁰ And Lynn E. Rose has only recently confirmed this by showing elsewhere in this volume that an almost complete set of Sothic dates occurs only after 687 B.C., the time of Velikovsky's last upheaval. That is, Rose has confirmed that Sothic dating is a later invention which does not go far back into early Egyptian history. But Bauer is completely oblivious to Ley's statement which casts extreme doubt on the establishment chronology of Egypt. Rose's work is so important that the *Journal of Near Eastern Studies*, published by the University of Chicago, presented this material, which distinctly shows that what were accepted by historians as firm, astronomical anchors of Egyptian chronology do not exist. As he states, "No one seems to have been able to establish an early second-millennium chronology for the Twelfth Dynasty by calculating exact placements both for the Sothic date and for the El-Lahun lunar documents."⁶⁶¹

Anthropologist Patrick Pender-Cudlip makes this assessment quite clear:

"Most historians . . . share certain ideas about possibilities, and it is these ideas, more than anything else, that determine how they distinguish between myth and history . . . no story has an inherent quality which makes it historical; it becomes historical not by being true but by being accepted as true. Conversely an unhistorical story or 'myth' . . . is not necessarily an untrue story, but simply a story which is regarded as untrue. Neither myth nor history has any 'objective' existence from society . . . historians in different societies reconstruct the past in different ways for different reasons, using different criteria to distinguish between [criteria of] fact and fiction which are a product of their cultural environment."⁶⁶²

The historical validity of the Exodus has been called a myth by historians in terms of the events it describes, while the Ipuwer document has been called history in terms of the events it describes. As shown above, some very well regarded historians, anthropologists, and others, say they both refer to the same events and, therefore, should be seen as contemporary. Bauer acts as though all the evidence Velikovsky presented on this matter does not exist, as though the historians, and others who do not concur with him on this point have no veracity and that the whole matter is cut and dry and settled.

Where Bauer has gone wrong is his assumption that certain matters in science and history are settled and correct. But this is far from the case. Neither the placement of the Exodus nor the chronological validity of Egyptian history have been established and settled. Bauer has merely mouthed the general accepted assumptions in these areas of research as if they were facts with which to lambast Velikovsky. But Velikovsky not only questioned these assumptions, he showed that they had not been correctly drawn by examining the facts underlying them in his writings. Bauer has indeed read these, as his bibliography shows, but has simply ignored them as if they did not exist to raise this point. That is, Bauer attacks Velikovsky's methodology for not accepting standard historical interpretations of ancient chronology; but these have been shown to be incorrect based on research of those who work in these areas. How, then, could Bauer ethically have raised such an issue? I suggest that this, once again, shows that Bauer's moral commitment to examining Velikovsky's work dispassionately is a sham and a fraud! To argue procedurally that the Exodus and the ancient Egyptian chronology are established fact, but never to mention, as part of this criticism, the point Velikovsky specifically raised about them, and then proceed as if Velikovsky did not work honestly in dealing with these difficulties when he in fact did, is misrepresentation at its worst. To sweep these matters under the rug in order to attack Velikovsky's methodology is nothing less than dishonest. And that is the insidious nature of Bauer's criticism. When Bauer raises an attack on a point against Velikovsky, he does not show in that same place that Velikovsky answered it. To place Velikovsky's response to the attack elsewhere in the

⁶⁶⁰Willy Ley, *Watchers of the Skies*, (New York, 1963), p. 15.

⁶⁶¹Lynn E. Rose, "Astronomical Evidence for Dating the End of the Middle Kingdom of Egypt to the Early Second Millennium: A Reassessment," *Journal of Near Eastern Studies*, Vol. 53, No. 4, (October 1994), p. 261.

⁶⁶²Patrick Pender-Cudlip, "Oral Traditions and Anthropological Analysis; some contemporary myths," *Azania*, Vol. 7, (1972), pp. 3-24 as cited by Richard Heinberg, *Memoirs and Visions of Paradise*, revised ed., (Wheaton, Ill., 1995), p. 183.

book is a tactic that will fool even the careful reader who will not remember it, especially when the answer is presented in another connection. Again and again, too many of his bald statements of criticism are not backed up by citations or any research. But when these are examined, Bauer's criticisms turn out far too often to be in stark contradiction to the facts. Bauer is not one to be trusted to play by the very rules he has created. Far too much of his criticism is hypocritical, since he does not follow his own dictates, and many of his criticisms turn out to be downright falsehoods, no different in value from those Carl Sagan and others presented.

Bauer's criticism is not really very different than that of Carl Sagan, James Oberg and the rest. It is only larger and more subtly misrepresentative of Velikovsky's work. As Martin Gardner stated, he found Bauer's critique similar to those that went before and welcomed him into the Velikovsky debunking club. It is a great pity that Bauer should have devised such an approach to Velikovsky's research. What is most deplorable is the cleverness underlying Bauer's criticism. The criticisms are formulated in such a way that one must not only re-examine precisely Velikovsky's works, but also that of the scientific establishment to unravel Bauer's manipulations and evasions. It is a strategy to degrade Velikovsky the man and Velikovsky's ideas. My final evaluation of his work is that Bauer's criticism is as Goldman stated above, a "tendentious, dishonest and malicious piece of character assassination, all the more pernicious because of the studied tone of detachment."

There is a great deal more that can be said on many other issues, as Professor Wolfe will present next. I feel safe in suggesting that Bauer's critique suffers from too many serious problems to make it helpful in evaluating Velikovsky. It is, in fact, an empty polemic masquerading as solid research. It is an attempt to undermine Velikovsky's credentials by attacking his competence and also his motives. Bauer is attempting to stop others from conducting their own evaluation of Velikovsky. His work, like Carl Sagan's, puts him among those in the infamous gang of the Velikovsky Debunking Club. "Welcome."

Every accusation Bauer lays at Velikovsky's door he is himself guilty of doing repeatedly. There is no excuse for such behavior. His criticism of Velikovsky, as seen in the mirror of this analysis, shows him to be either incompetent or dishonest or both.

* * * * *

BENEATH BAUER, By Irving Wolfe

In 1984, Henry Bauer the chemist published *Beyond Velikovsky*, a book which purports to deal with the events and implications of the Velikovsky Affair. It is divided into three parts which, in my opinion, have decreasing merit. The first, "The Story of the Velikovsky Affair," is the most useful because it is a fairly-objective annotated narrative of the major events, whose data, some of it new, covers three decades. This makes it valuable as raw material for any study of the Affair, not merely (or, as we shall see, not particularly) Bauer's. In the second part, "An Analysis of the Velikovsky Affair," Bauer does not analyze the Affair but attempts to assess the validity of Velikovsky's planetary and historical theories. Here he finds himself on much shakier ground, ending up thoroughly bungling the job, as Ginenthal has shown in the previous chapter. The title of the third part is "Beyond the Velikovsky Affair," and here Bauer attempts to answer two questions: Why do people still believe in Velikovsky, and How should science behave if this sort of situation ever arises again? This is the largest section of the book, and it turns out to be Bauer's central point. If, therefore, we discover that Bauer does not succeed here, not at all, this is a death blow, for then he would have failed in his grand objective, which would make the book *as a whole* a failure. I will show that this is just what occurs, because Bauer is totally out of his depth as a pop-psychologist and would-be

philosopher of science, to the point where the final section of his book, the very *raison d'être* of his endeavor, becomes not merely wrong but almost meaningless, and ends up being merely a paean to and advertisement for Bauer himself.

1. Why People Like Velikovsky

The last section of the book, as I said, contains three sub-sections, the first of which attempts to find reasons for Velikovsky's continuing popularity. This prologue is intended by Bauer to set the stage for the grand conclusion, his description of science and then of the Affair, but it gets things off to a disastrous start, doing much more damage to himself than Velikovsky.

Let us look at the first reason Bauer offers. It is called "The Wish to Believe," and Bauer argues "that it is characteristic of human beings to wish to believe."⁶⁶³ I had felt, upon first picking up his book, that Bauer's style was a welcome relief after Asimov, whom I will criticize in a later chapter. That is to say, where Asimov is an ill-spirited boor, Bauer appears to be polite, respectful and modest. A closer look at how he writes, however, has led me to be much more suspicious, for it reveals that he is guilty of a *number* of unscientific, unintellectual and actually dishonorable types of error in his writing.

For instance, note how he introduces this first point. If Velikovsky's theories are "incredible, obviously misguided," one "is then faced with task of explaining how otherwise intelligent people can be seduced by such fallacious stuff."⁶⁶⁴ What a sneaky way to put the question, for the impression slyly given by his malicious choice of words is that people who believe Velikovsky are "intelligent" whenever they do *not* believe in Velikovsky, that Velikovsky's "stuff" is entirely "fallacious" and that those who believe in it were "seduced." That is the first category of error I call attention to in Bauer—rhetorical manipulation. I have termed it "sly" because the passage contains a number of loaded, subliminally influential words (as indicated by quotation marks), words which are *entirely unnecessary* if Bauer were really trying to be objective, for then his question would have simply been something like "If Velikovsky is wrong, why do people believe him?"

That fault is quite repugnant, but the presence of these sneaky put-down adjectives is not Bauer's major weakness here. What is worse is that he tosses off the statements in which they appear *with no proof whatever for them*. We are not told who these "people" are, or if they are identical (which alone would allow Bauer to characterize them *all* in one lump), or how they were *all* judged to be "intelligent," or what acumen they displayed in reaching decisions where Velikovsky was not involved, or just how their belief in Velikovsky was *always and only* the result of "seduction," or whether this statement applies to everyone who believes in Velikovsky. (Are all his supporters "intelligent"? Were they all "seduced"? Is there no variation at all between them?) There is no proof offered here for these preposterous over-generalizations, no reference to independent studies, no statistics *and no discussion*. All we have is Bauer's bald assertion, and we are urged only to take his word for it. That is the second of Bauer's major faults on our list—a pattern of argument which is unsupported and extremely unconvincing (and even deceitful) because he does not even try to prove what he says. He merely blurts it out (loaded, of course, with sneaky adjectives), and then acts as if it is taken for granted. That is how Bauer starts the all-important third section, and it is a weakness in argumentation of which he will be guilty throughout.

From this unsupported platform (nothing but his mere opinion), Bauer leaps to a series of subsequent related assertions about how people believe, each one equally unsupported by any proof whatsoever. Despite that, they are presented by Bauer in a tone of *ex cathedra* authority as if they were absolutely true, and as if they applied to everyone everywhere.

⁶⁶³Henry H. Bauer, *Beyond Velikovsky: The History of a Public Controversy*, (Urbana: Univ. of Illinois Press, 1984), p. 185.

⁶⁶⁴*Ibid.*

". . . we do believe that what we see, hear, read, is true, just as long as there are not definite and severe obstacles to believing."⁶⁶⁵

"The child, learning from adults, finds that most of what he is told is in fact true."

"As life becomes more complicated, we recognize that the truth is sometimes shaded. But still, on the whole, it is quite safe to believe almost all that we are told."⁶⁶⁶

"In adolescence and early adulthood, our readiness to believe encounters our desire to comprehend the world . . . we choose among the various philosophies and religions to whose propaganda we are exposed And, having once embraced a belief, we are prepared to ignore all sorts of evidence that points to the inadequacy of that belief."

"Commonly, as adults, we continue to be more likely to believe than to disbelieve."⁶⁶⁷

These are very broad low-specificity social science concepts, for nowhere does Bauer support them with any data or studies or statistics or independent evidence. Do these patterns, we wonder, apply equally to everyone? *Can* they? If not, to what portion of the population *do* they apply, and how is that portion characterized? Where are the studies which support this? Bauer does not tell us. To continue, who is the "we" to whom he refers? Does it include scientists? Furthermore, does our desire for comprehension always encounter our beliefs in our adolescence? Is this true of all the 5 billion people in the world? Are we *all* prepared at the same time to ignore counter-evidence to our beliefs? None of these questions is answered.

What can Bauer possibly mean by offering these vague over-generalizations? Are they to be taken as an argument? He never says that these are only his *opinions*, that he feels they *might be* true, or (even worse) that they may not apply to everyone in every society at every time. He merely orates this supposed ideological history of humankind with a casual air of certitude, as if it were absolutely typical like the growth of a tree or the formation of an organic compound. From this observation, we can perceive the source of Bauer's error—Bauer the chemist is treating *social* life as if it were a chemical process, uniform and predictable. That is the kind of scientific mistake he will continue to make throughout the section, which will render it meaningless.

I have gone carefully right from the beginning into the rhetorical sleights-of-hand employed by Bauer (not to mention his deficiencies in argumentation), because I am following an evaluative model *which Bauer himself insists upon*, and which therefore must be as valid when applied to himself as to Velikovsky. It is very strict, and can be found for instance in Bauer's response to the alleged success of Velikovsky's prediction that the planet Jupiter emits radio signals. Bauer admits that "Velikovsky was right about this,"⁶⁶⁸ but he denies that the cause is as Velikovsky stipulated because Velikovsky's explanation provides no evidence, lacks substance *and is therefore* (in his own words) *meaningless*. (In the following quotations from Bauer, the notation is mine.)

- a. ". . . he has not made clear how he reached this conclusion. Only when he does that can the significance of the successful prediction be gauged."⁶⁶⁹

⁶⁶⁵*Ibid.*

⁶⁶⁶*Ibid.*, pp. 185-186.

⁶⁶⁷*Ibid.*, p. 186.

⁶⁶⁸*Ibid.*, p. 87.

⁶⁶⁹*Ibid.*

- b. "Velikovsky's prediction was . . . useless in . . . its *lack* of detail—where to look in the radio spectrum."⁶⁷⁰
- c. "Velikovsky failed to predict the existence of radio noises from Venus."⁶⁷¹
- d. "Velikovsky failed to suggest a mechanism for the emission of the radio signals and did not even specify the type of emission."⁶⁷²
- e. "He did not suggest at what frequencies we might look for the signals, which is . . . unlikely to be a productive endeavor."⁶⁷³
- f. "As it stands, there is as much reason to class him as wrong as to class him as right in relation to Jupiter."⁶⁷⁴

These criticisms of Velikovsky's argumentation indicate how Bauer expects a serious scientific statement to be supported. They imply his model. Should anyone object that Bauer is being overly fussy or picky, he would no doubt respond that important assertions have to be able to be verified and used or they are mere empty utterance. He certainly said this often enough about Velikovsky, and, therefore, when we come to analyze Bauer, must we not apply this model to Bauer himself, especially here in his all-important third section? Mustn't we be as fussy and picky as he is? To put it differently, doth it not behoove us to do unto Bauer as he hath done unto Velikovsky?

It certainly doth, and therefore let the fun begin.

What we find here, of course (as we will all along), is that, when we apply Bauer's models to his own writings, he comes out much worse than Velikovsky. Consider, for instance, the real significance of what Velikovsky did in his statement about Jupiter we discussed a moment ago. So certain was he that his *overall* theory was correct that he laid its entire validity on the line, staking its correctness on a prediction that no scientist in the world would have seconded and for which, as Bauer himself admits, there was no hint in the data at the time Velikovsky made the prediction. We could therefore say for a start that Bauer's response to Velikovsky's daring gamble is a trifle ungenerous (to put it mildly), but that is not important now. What we are concerned with specifically at this moment are the *intellectual* and not moral defects of Bauer's reaction. To illustrate how extensive and self-destructive these are, here is the result when we apply Bauer's evaluation model to his own theory about believers (to be compared with the noted quotations on the previous page). Assessing his ideas concerning why people are inclined to believe in the light of how he assessed Velikovsky, we *have* to say the following:

- a. He has not shown how he reached his conclusions, and therefore their significance cannot be gauged.
- b. His statements are useless because they lack detail and precision.

⁶⁷⁰*Ibid.*, p. 88, quoted from *Pensée*, VIII. p. 42.

⁶⁷¹*Ibid.*

⁶⁷²*Ibid.*, p. 286.

⁶⁷³*Ibid.*

⁶⁷⁴*Ibid.*, p. 87.

- c. He failed to describe any parallel phenomena in any related areas of human behavior.
- d. He did not specify a mechanism for the adoption of belief, nor its force nor its strength.
- e. He did not indicate how one could verify his assertions.
- f. There is therefore as much reason to dismiss his statements as to accept them.

This is how we *must* judge Bauer to have begun his third section, with a point that is unspecified and unproved and therefore pointless, and we reach this decision by applying to his argument about believers the standard he has applied to Velikovsky. This failure does not bode well for Bauer (that almost everything he says in the first part of the third section, the prelude to his great disquisition on science, is an unsupported opinion masquerading as a provable assertion), for we shall find that it is a weakness of argumentation to which he will fall victim again and again.

To describe what Bauer does to himself, there is a phrase we might borrow from Shakespeare. It comes from *Hamlet*, and is to be found when the protagonist contemplates his enemy Claudius destroying himself through his own efforts. The expression Hamlet uses, which is extremely well-known, employs the analogy of soldiers trying to dig under a fortress wall to blow it up and who are themselves blown up—the "enginer" (digger) is "hoist" (blown up) with his own "petar" (explosive). That is what occurs again and again to Bauer: he will hoist himself with his own petar, to the point where what he says becomes not merely valueless but self-demolishing.

In addition, we need a term to describe not merely *how* Bauer destroys himself, but the meaninglessness of what he produces as a result, and, as usual, we need only turn to Bauer himself to find it. (We shall find, to our amusement, that he almost always provides the rope to hang himself.) The term I refer to here is to be found in Bauer's analysis of Velikovsky's explanation of the miracle of the parting of the waters at the Sea of Passage. Velikovsky spoke of immense tidal waves being built up and then suddenly receding as the result of a giant electrical interchange between the comet-like Venus and the Earth, and Bauer asked

"What current at what voltage for how long is needed to push down how much water, by what mechanism? On what law is such an assertion based?"⁶⁷⁵

(This, as the reader will perceive, is the model I followed a moment ago when I asked very precise questions about Bauer's vague statements on the behavior patterns of believers.) Bauer decides that this kind of theorizing by Velikovsky is "so vague that it cannot even be discussed, let alone disproved or proved,"⁶⁷⁶ and he therefore judges that this is what "the scientific community calls 'hand-waving.'"⁶⁷⁷ (Note the sneaky implication that Velikovsky, who is guilty of it, cannot therefore be part of the scientific community, while Bauer, who recognizes it, is.) That is how Bauer describes Velikovsky's Sea of Passage theory—because Velikovsky has not supplied the kind of data called for in Bauer's query quoted above, then "Without answers to these questions we have only hand-waving, not science . . . it is gibberish."⁶⁷⁸ We must thank Bauer for this expression because, by his own standard, most of what

⁶⁷⁵*Ibid.*, p. 124.

⁶⁷⁶*Ibid.*

⁶⁷⁷*Ibid.*

⁶⁷⁸*Ibid.*, pp. 124-125.

he says in this first part of the third section will be found to be hand-waving and gibberish, "so vague that it cannot even be discussed" (*i.e.*, he will be hoist with his own arrogant petar). I would go further and, applying his own analogy to himself, have to call his ideas *finger*-waving, for, in comparison to the large amount of specificity and evidence in most of what Velikovsky says (prediction as the result of theory, to train of thought, to individual insight), most of Bauer's observations are merely chit-chat. They are *pinkie*-waving. The majority of the third section, as we shall see, the very heart of Bauer's book, is merely pinkie-waving.

Following upon his first set of unsupported platitudes about belief, uttered in a tone of absolute assurance, Bauer then begins to insert more propaganda.

"We must begin by recognizing that humans are ready to believe almost anything, *unless* there are very high, well-nigh insurmountable obstacles to belief."⁶⁷⁹

This is stated not as a casual remark, nor as his opinion, nor as something that might be true sometimes, but as a virtual law of nature which is true of everyone everywhere at every time, and it *has* to be put that way by Bauer (dogmatically and without exception like a chemical law), because it becomes the foundation of his attempt to entirely dismiss the *value* of Velikovsky's ideas. (His object is to equate Velikovsky with the "almost anything" in the above passage.) Here is how he tries to do it:

"In the present context we should not ask how it is possible that people could give credence to Velikovsky"

—after all, "people" or "humans" are inclined to accept "almost anything"—

"We must begin by recognizing that many people, for various reasons of background and personal inclination, will be ready to believe such ideas as those of Velikovsky Then we need only establish that these ideas are not, in fact, obviously impossible."⁶⁸⁰

Bauer presents this as no less than a syllogism, an invariable and universal truth: people are always ready to believe ideas *like* Velikovsky's as long as they are not "impossible," Velikovsky's ideas are not impossible, therefore people believe them. That is that.

The errors in Bauer's thinking are manifold. First, notice that two of the three passages I have just quoted start with the same phrase "We must begin by recognizing." Must we, Henry? Not if we follow your models of evaluation, for we recall how you treated Velikovsky's prediction about the parting of the waters at the Sea of Passage. To remind the reader, it was this:

What current at what voltage for how long is needed to push down how much water, by what mechanism? On what law is such an assertion based?

Well, Henry, where is *your* evidence for what we "*must* recognize"? You offer no proof whatever for your universal assertions, no *independent* psychological statistics about what sorts of ideas people are most ready to believe, no *independent* ranking of the eagerness which different ideas provoke, no sociological or demographic subdivisions of the category "believers," *nothing*, only your *ex cathedra* dogmatic insistence that, despite your total absence of

⁶⁷⁹ *Ibid.*, p. 186.

⁶⁸⁰ *Ibid.*, p. 187.

proof, "We must begin by recognizing." What would *you* call this, Henry, in your status as expert on science? Is it sound argumentation, or is it (to use your memorable term) hand-waving?

It is, of course, a wholly tendentious argument, and, if we desire a model to tell us *how it should have been done*, why, Bauer himself inadvertently gives it to us, as usual. Here is what he says about the weakness of Velikovsky's predictions and how Velikovsky *should have* proceeded when he first came across his ideas.

"Consider the courses of action open to a man in Velikovsky's position."⁶⁸¹

To indicate the correct procedure, Bauer wonders what an "imaginary scholar" who followed the rules of science might have done in Velikovsky's place. If he felt, for instance, that the Ipuwer Papyrus and the Biblical plagues describe the same event (one of Velikovsky's major arguments for the historicity of the Exodus), then he must take steps to discover if it could be true.

". . . our imaginary scholar would have attempted to find some independent evidence to allow the dating of the papyrus. But Velikovsky did not do that. He postulated the different date."⁶⁸²

The "imaginary scholar," says Bauer, should have written an article describing his hypothesis, to provoke discussion on it by his peers, and sought external corroborative data. "Our imaginary scholar would seek independent evidence But, again, Velikovsky did not do this."⁶⁸³ *Can* one make general statements derived from legends? "Again, Velikovsky did not consider this question independently."⁶⁸⁴ *Could* the legends be true? "To establish the possible correctness . . . he would seek independent evidence. But Velikovsky, of course, did not do that."⁶⁸⁵ We are told that Velikovsky merely "postulated,"⁶⁸⁶ he "assumed" and he "accepted." Worse than that,

"Velikovsky had not built an objectively strong case . . . because he used as a test of his evidence only whether it accorded with his premise and did not *adduce* independent proofs He did not show that his interpretations were the only possible ones, or even that they were the most likely ones; he simply did not discuss possible alternatives."⁶⁸⁷

Well, Henry (we might say if Bauer were here), is this not what you have just done? Why have you not, in your own argumentation, followed your own advice? You have just accused Velikovsky five times of failing to provide "independent" support. Where is *your* independent proof for your *ex cathedra* generalizations about belief, my good fellow, or your "objectively strong case" to support *your* interpretation as the only possible (or even likely) one? Why should we accept what *you* say without independent evidence? Haven't you too merely "postulated" and "assumed" and not discussed any alternatives because you are sure your ideas accord with your premise? Haven't *you* failed to consider your ideas independently? Haven't you (to be specific) failed to do *any* of the things you insisted your "imaginary scholar" should do, and aren't you therefore plainly guilty of pinkie-waving, judged by your own dogmatically-asserted standards, and haven't you once again shot yourself in your own foot?

⁶⁸¹ *Ibid.*, p. 90.

⁶⁸² *Ibid.*

⁶⁸³ *Ibid.*, p. 91.

⁶⁸⁴ *Ibid.*

⁶⁸⁵ *Ibid.*

⁶⁸⁶ *Ibid.*, p. 90.

⁶⁸⁷ *Ibid.*, p. 96.

Feeble and self-destructive argumentation, however, is perhaps the mildest of Bauer's sins in this section, for he is guilty of a much more heinous offense—character assassination, or sly *condemnation by innuendo*, the very thing he said he abhorred most in Velikovsky's previous critics. (If it turns out that Bauer is guilty of the same thing, of course, then he demolishes his pretension to be an objective critic.) Let us therefore look carefully at the next point he tries to establish about belief.

"... the human wish to believe places the onus of proof—or rather disproof—on the unbeliever. That is not what we take to be the norm, say in philosophy or in scientific activity."⁶⁸⁸

Notice what is being implied, that the average (or merely "human") person will believe anything (especially if it is foolish) because of a desire to believe, but that is not true of the scientist or philosopher, two realms of activity known above all to produce *truth*. Bauer is therefore super-"human," in that he is a scientist writing a philosophical book (*i.e.*, he is a member of the "we" in the above passage), which sneakily implies the following series: Bauer the philosophical scientist can easily perceive the errors in Velikovsky which the merely "human" reader cannot, Bauer is therefore fully qualified to report them to the "average" public because he unlike them places the onus of disproof on Velikovsky, and Bauer should, therefore, be believed over Velikovsky or his supporters because he is doing a service to those merely "human" people unable to fend for themselves in such matters. As he warns,

"There were no great barriers to believing Velikovsky."

—not among the average unphilosophical unscientific humans—

"except for the specialists in various fields,"

—as if they *all* reacted in the very same way—

"who recognized his wholesale repudiation of satisfactory and well-established concepts."⁶⁸⁹

The chain of Bauer's reasoning seems to be this: science consists of "specialists," and specialists are not easily fooled by foolish ideas as "humans" are, but are always right (and always in unanimity), and Science to a man condemns Velikovsky, and Bauer is a scientist and therefore he too is completely right and Velikovsky is wrong, and all the merely "human" readers who believe him are wrong. Believe only in Bauer.

This, of course, is as usual an unsupported, over-general and therefore false argument, judged by Bauer's own standards, moving slyly and self-servingly from non-point to non-point, but it softens the reader for the next innuendo, which is even more insidious.

"Beyond that, the fervor shown by many of Velikovsky's supporters demands some attention."⁶⁹⁰

⁶⁸⁸ *Ibid.*, pp. 186-187.

⁶⁸⁹ *Ibid.*, p. 187.

⁶⁹⁰ *Ibid.*

He finds their behavior "ardent," "religious," "that of converts to a new religion."⁶⁹¹ That is all he says on this point, but, from this minuscule assessment, which is wholly unscientific, he slides sneakily into his next allegation, "that contemporary society offers fertile ground for the appearance of new cults."⁶⁹² This new concept, however, that belief in Velikovsky is a cult phenomenon (*i.e.*, that there is no intellectual reason for it), is not presented by Bauer as the conclusion of a solid and well-worked-out argument. No chain of reasoning is displayed, no independent evidence is adduced, it is never even stated by itself as a point, it is merely tossed off by Bauer *in the middle of a sentence*, without further support or elaboration, and is then in the next instant treated as a proven datum from which to launch an analysis of cults. It is a preposterously non-intellectual procedure. Bauer does not know how to think.

Bauer's objective, of course, is to argue that Velikovskianism is a cult and that is why people (apart from scientists) are attracted to it, but we have seen that he does not lead up to it intelligently, and we will find that the rest of his argument on that topic is as non-rational as its beginning. For instance, having simply introduced the cult slur without proof and then taken it for granted without justification (thereby allowing him to condemn all of Velikovsky's supporters as extremists and fanatics *without having to discuss the issue adequately*), he slides into his next attack (*why* cults exist), by building up what he wants to say from separate quotations, as if these different speakers had acted in concert. That is to say, having leaped casually from "fervor" to "converts" to the idea of "cults" without any proof or argumentation whatever, he will now embark on an analysis of the *function* of cults, but again without proof, as a way to explain the popularity of Velikovsky. The result is that this next issue too is not argued carefully, academically, with *independent* proof, but is merely thrown at the reader naked, so to speak, in an artificially-constructed set of quotes, and in a take-it-or-leave-it fashion (although we can guess that Bauer, having had recourse to his standing as an expert, expects by now that his innuendoes will be accepted without question). That is his technique.

Here is how it is put:

" . . . the cults, while . . . insubstantial and occasionally eccentric to the point of being purely funny"

—(like the Velikovskians)—

"nevertheless do their level best to fill a serious vacuum."⁶⁹³

Without ever saying so himself, he has now gone from Velikovskians as cultists to catastrophism as a cult to *all* cults as merely funny (none of this with even the slightest shred of "independent" discussion or proof), which will leave him free to make his next point (once again not even formally stated in his own text, but merely tossed off in the midst of someone else's words), that cults like Velikovsky are popular because they provide answers to questions which science and religion cannot handle. (See the next quote.) These questions (Bauer prepares us to see), are not reasonable ones like those of science, but arise from the unreasonable desires of "human" people who are not scientists like Bauer (*i.e.*, Velikovskianism is unscientific and founded on passion). That is the vacuum cults fill, and the only reason for their existence.

" . . . unorthodox theory usually promises people something they want"

—(notice "people")—

⁶⁹¹ *Ibid.*

⁶⁹² *Ibid.*

⁶⁹³ *Ibid.*

"whereas conventional science seems to close the door for the ordinary guy."⁶⁹⁴

(Notice "science" opposed to "ordinary" and "conventional" opposed to "unorthodox," as if science is non-ordinary and only ordinary "people" prefer the "unorthodox.") What Bauer is implying by means of this quote (but never states or develops in his own text) is that science knows what can be discovered or determined, but ordinary people set their requirements too high, to which only cults (being totally non-scientific) can respond. It is a neat and very self-serving argument, and Bauer to our surprise actually offers some illustrations here of what he means. This does not indicate, however, that Bauer has suddenly learned how to argue more intelligently, for notice how slyly the list which Bauer quotes of things people want from cults is used to link Velikovsky with some very obviously impossible desires.

"People wish to believe that there are easy, sure ways of foretelling the future, surviving death"

—(obvious impossibilities)—

"or confirming the literal truth of the Bible."⁶⁹⁵

Haha! Gotcha, Velikovsky, as if Velikovsky's sole purpose were to confirm the Bible, which would make those who believe in him as silly as people who seek to surmount death. That is the sneaky point of the quote. This is not legitimate or courteous discussion, for no statistics or evidence on cults are offered, nor on what are the most popular subjects they believe in. He never tries to substantiate his outrageous over-generalizations. (Are all cults the same always and everywhere? Do they all desire precisely the same sorts of things?) Nor does he ever discuss the topic of "cults" in the abstract. He merely takes his naked assertions for granted as soon as they are said and then uses them in a sneaky, dishonest attempt at *guilt by association*. Following that, the next sentence of the quote which Bauer has chosen to offer here in lieu of an intelligent discussion even more sneakily condemns Velikovsky by implication.

"If a half-plausible-sounding method of fulfilling these fervent desires is found"

—(meaning Velikovskians, of course, who are "fervent," and Velikovsky, whose ideas are "half-plausible")—

"millions will flock to it"⁶⁹⁶

like all of those "ordinary guys" who are not scientists like Bauer.

That is Bauer's point. Notice how cleverly and slyly the whole sneaky business is done. Bauer does not present a fully reasoned argument leading step-by-step to the inevitable reasonable conclusion that *all* Velikovskians (or even those who believe in him mildly) are cultists who seek the impossible. There is no reference to independent studies on what cults are or what they want or how they are formed or even how they may be legitimately distinguished, no reference to the history of opinions on that topic, nor (most of all) to the differences of opinion on

⁶⁹⁴ *Ibid.*, p. 188.

⁶⁹⁵ *Ibid.*

⁶⁹⁶ *Ibid.*

that topic among analysts of Velikovskianism. Bauer appears to have done *no* research on the issue at all. There is not even a plain statement from him that he feels unquestionably that Velikovskians constitute a cult. On the contrary, he simply begins talking about the subject *as if* it had already been adequately argued, and then offers quotes to let *them* do the dirty work—cults are funny but fill a vacuum, cults supply the foolish wants of "the ordinary guy," cults are "half-plausible" but "millions" flock to them. He says nothing, but tries to make it sound like something. This is precisely the great fault which Bauer had found in Asimov's "CP" article, that Asimov had failed to look into the topic adequately and did not understand the complexity and difficulty of the issue and merely orated things *ex cathedra*, yet here he does the very same thing, treating cults as lightly and ignorantly (and over-confidently) as Asimov had treated crackpots. (It is, of course, pinkie-waving.)

Bauer is trying to make a serious point here in what purports to be a serious book, yet it is all manipulation and deceit, innuendo and sly implication, vague generalization without evidence, presented by Bauer-the-scientist to be accepted solely, it would appear, because of his tone of scholarly authority. That is the next great fault of Bauer's we will note, and, when we seek for a technical description of the deceitful thing he is doing, we find it as usual ironically in Bauer's own words, when he berates Sagan of all people for attacking Velikovsky without proof.

". . . his utter conviction on the specific issue leads him to try to obtain assent to his opinion even though he cannot prove his opinion to be correct."⁶⁹⁷

What according to Bauer does Sagan do in lieu of proof? He tries

". . . to invoke at least implicitly his standing as a scientist in seeking assent."⁶⁹⁸

That, sad to say, is also all that Bauer offers, and for the same reason. No evidence, no careful argumentation, no awareness of complexity, no proof, merely the arrogant assertion that he is an expert and knows what is right. Once again, he has ironically described his own fault.

The reason that Bauer's dicta on belief have been evaluated so thoroughly is that this is the opening argument in the third section of his book (the culminating movement of his opus), and it is from this first part of the third section that he wishes to launch the concluding segments, his analysis of science and of the Velikovsky Affair in that light. We find, however, that Bauer's argument here is of little value, for he has failed to say anything valid. The section is actually an insult to the reader, as if the "ordinary" person is always susceptible to the "unorthodox," which is always wrong, while only the scientist or "specialist" like Bauer is clever enough to keep from being "seduced" by temptations like Velikovsky and will always choose the truth, that is, the "orthodox." Do we agree that *all* average people are gullible and *only* scientists are not? That the average person always wants to believe foolishness and only the scientist is able to withhold assent? Bauer writes in a calm, superior, self-confident tone, implying that what he says is both momentous and true, but most of it turns out to be simply a trivializing put-down of what he disagrees with, with no proof, no judicious qualification of universal assertions, no restraint, merely untestable generalization and waffling, written with an air which appears objective and uninvolved but is very patronizing. In the end, therefore, his "stuff" is quite "fallacious." It is little more than his bare opinion, unsupported by any evidence, and is not objective, but subjective and very involved, as we will see later in this chapter. I call this work *punditizing*—to act like a pundit, to issue opinions supported only by one's arrogance, without evidence of any sort. In my opinion, Bauer is pontificating, patronizing, punditizing, hand-waving, all the faults of which he accused Velikovsky.

If we wish to assign a value to these pontifications, why, Bauer of course has once again provided us with the correct method and standards. We find them in how he routinely assesses the predictions of Velikovsky. Virtually every explanation by Velikovsky is valueless, says Bauer, because it "cannot be tested because it is too

⁶⁹⁷ *Ibid.*, p. 307.

⁶⁹⁸ *Ibid.*

vague, purely a qualitative and descriptive account . . . he has not presented a conclusive, testable chain of reasoning."⁶⁹⁹

"The material he presented was not testable, or even discussable . . . [It is] speculation that cannot at present be tested . . . The book [*Worlds in Collision*] gives no specific quantifiable proven . . . data . . . it is not scientific."⁷⁰⁰

". . . in such complex situations . . . we are dealing with matters where the laws are not sufficiently well-established, or the detailed parameters sufficiently well-known, as to make possible statements like that."⁷⁰¹

"This is a collection of genuinely meaningless statements . . . There is nothing here that [we] can use . . . to think about in a meaningful way."⁷⁰²

". . . arguments . . . replete with wishful thinking, uncritical commentary, preconceived notions."⁷⁰³

It is in these terms that he dismisses Velikovsky. If Bauer is an expert on science and pseudoscience (as he tells us often enough), then we naturally have to accept his standards as authoritative, and we would therefore inevitably apply them to any writing on science or pseudoscience that we come across, especially his own. When we do so here, however, the results (as we expect by now) are hardly what Bauer would have liked, for what we have found in his analysis of why people believe are nothing but "meaningless statements" on the topic, made in ignorance of the reality that he treats "complex situations . . . where the laws are not sufficiently well-established . . . as to make possible statements like that." We must keep in mind that these are the first major points which Bauer tries to establish in this, the main part of his book. He says he will be objective and scientific about it, but we see that (in his own words) most of what he offers is "speculation", unquantified and "uncritical", that it has not been well-argued or well-established but is mainly "wishful" and "preconceived", and as a result we have to say as he said of Velikovsky that this stuff is not merely "untestable" but "useless," undiscussable, because it is not able to be "thought about in a meaningful way." Even though, however, Bauer in the first part of the third section condemns himself once again out of his own mouth, nevertheless he thinks that this hodge-podge of chit-chat disguised as analysis can serve as the underpinning of his major philosophical point. Because it is obviously a failure, full of valueless unsubstantiated nonsense, I am therefore afraid that we must apply to Bauer here one more model which ironically he applied to Velikovsky.

"When a man makes a fool of himself by discussing, at ponderous length, a subject of which he is ignorant, I tend to assume that he will act similarly in other fields."⁷⁰⁴

⁶⁹⁹*Ibid.*, p. 87.

⁷⁰⁰*Ibid.*, pp. 92-93.

⁷⁰¹*Ibid.*, p. 96.

⁷⁰²*Ibid.*, pp. 124-125.

⁷⁰³*Ibid.*, p. 318.

⁷⁰⁴*Ibid.*, p. 94.

Must we not decide that, if Bauer *himself* is so foolish here, then everything else he will say in the third section will be "similarly" ignorant? (After all, this is how he approached Velikovsky, and we do have a duty to accept Bauer's model as authoritative, since he tells us he is an expert on science.) Let us see.

2. Bauer on The Nature of Science

Bauer's next step in the third section is to try to define what science is, in order that he may then interpret the Velikovsky Affair in that light. He presents a very serious formulation which is critical to the entire third section, and, therefore, if he fails here, the whole section collapses. We shall find that this is just what occurs, that he bungles things as badly here as he did earlier, and consequently that he cuts the legs out from under his entire analysis.

The beginning of the section is quite clever, in that Bauer starts by saying all the right things and giving the impression that he will be objective, accurate and universal. (In practice, however, he will be none of these.) First, he proclaims that he will clear the ground by eliminating incorrect ideas, which is necessary because "misconceptions about science are held not only by nonscientists but also by scientists."⁷⁰⁵ The impression given is that, even though he is a scientist, he is not one of those guilty of harboring misconceptions and is therefore qualified to enlighten everyone of every stripe, even fellow scientists less astute than himself. (He is one of the few who can clear the ground because he is a *bona-fide* expert on science.)

The principle he as an expert will follow in instructing everyone else about science is to define the object and stick to the data, letting it virtually speak for itself.

"I shall proceed on the basis that science comprises all those matters that are commonly called "science," and that we are interested in realities about that."⁷⁰⁶

Nothing unreal will be offered to the reader, nothing idealized or romanticized or falsified, just the reality.

"I shall attempt a purely descriptive approach as the only way to avoid misconceptions."

Why must one strive to proceed only in this way?

"When one tries, as many have done, to construct a model or theory of what science is, one isolates and focuses on certain characteristics that are regarded as "the" important ones. But scientific activity is so diverse that inevitably exceptions can be found to any generalization."⁷⁰⁷

It is best, therefore, to stick to description, which is what he says he will do, and avoid theorizing, for this could lead the analyst away from reality.

"The danger of the theoretical construct is that one may get so enamored of it as to come to believe that the model describes how science should be carried on or what science should be."⁷⁰⁸

⁷⁰⁵ *Ibid.*, p. 251.

⁷⁰⁶ *Ibid.*, pp. 251-252.

⁷⁰⁷ *Ibid.*, p. 252.

This must be avoided at all costs because it could seduce the analyst into preferring the model over reality, with negative results.

"Such an attitude inevitably clashes at some point with what actually takes place, and thereby leads to frustration, anger and unresolvable arguments."⁷⁰⁹

It sounds like a noble idea. The only problem, unfortunately, is that Bauer will then do precisely the opposite. His analysis will not be objective and concrete, but will begin with misconceptions, will isolate chosen characteristics "as "the" important ones," will present a desired theoretical model and will dictate "how science should be carried on," all in total defiance of his own taboos. That is to say, it will be *prescriptive* rather than *descriptive*, idealized rather than real, constructed rather than actual, and, as a result, it will most jarringly clash "with what actually takes place in science." In the end, therefore, Bauer will betray all his values and will say little about science as an institution and as it functions. Contrary to his expressed hope, he will ironically convey only his preference, his ideal, "what science should be" according to a model with which he is "enamored." (Once again, he will be hung by his own words.)

Let us examine the things he does wrong. The most evident of Bauer's faults here, as usual, is that he frequently contradicts himself. For instance, when discussing scientific knowledge, he tells us that the most desirable sequence for attaining it in science is fact-law-theory, and that science prefers theory because each one is "shorthand for a vast array of facts."⁷¹⁰ The facts come first, and theories are believed "because of the overwhelming evidence that those ideas . . . reflect an important part of reality."⁷¹¹ Later, however, he seems to say the opposite. Scientists, he tells us, have trouble perceiving that no scientific theory can describe fundamental reality.

"Since science does not deal in absolute truth, it has no answer to the fundamental questions that begin with 'why?'"⁷¹²

As a result

". . . science is thereby taught as something other than it actually is. From this approach, I believe, stems the typical unthinking acceptance by scientists of established views."⁷¹³

How is one to tell which of these contradictory descriptions of scientific belief by Bauer is correct? Do scientists believe theories for the good reason that they appear to be "reliable," or for the bad reason that (in Bauer's own words), they do not know that science has "no direct means of experiencing whatever absolute, external truth and reality might be,"⁷¹⁴ or that "scientific truths are limited ones, applicable in limited ways,"⁷¹⁵ which makes their

⁷⁰⁸ *Ibid.*

⁷⁰⁹ *Ibid.*

⁷¹⁰ *Ibid.*, p. 261.

⁷¹¹ *Ibid.*

⁷¹² *Ibid.*, p. 265.

⁷¹³ *Ibid.*

⁷¹⁴ *Ibid.*, p. 254.

⁷¹⁵ *Ibid.*, p. 255.

acceptance of "established views" merely "unthinking"? Which is it? Has science been properly taught to scientists, or mis-taught? We are offered no way to choose between these contradictory evaluations, for Bauer himself is unaware of them, yet it is on this sort of thinking (or "unthinking") that he seeks to construct a definition of science. It is not a good beginning.

A second fault of Bauer's is also evident here, and that is his tendency to employ vague terminology whose (unscientific) low specificity renders many of his sentences virtually meaningless (pinkie-waving). In the passage above, for example, what does "typical" mean? That "*unthinking acceptance*" by scientists occurs all the time? Or by most scientists at some time? If either of these assertions is true, then Bauer has just demolished most of his third section in advance, for the major fault which the supporters of Velikovsky charged against his critics was precisely that their intensely hostile reaction was the product of their unthinking acceptance of orthodox views. Second, what are we to make of the noun "views" in his term "established views"? Are the ideas of science merely "views"? Are they not *knowledge*? Or is all knowledge merely perspective? These are major issues, and they occupy much of the attention of philosophers of science today, but Bauer spends very little time discussing this topic because he does not seem to be aware of its importance (*i.e.*, he is very much out of date on science). As a result, the vague ignorant words he uses are not able to be thought about definitively. It is a fault of which he accused Velikovsky, but which he himself will commit again and again.

The water gets murkier for Bauer the deeper he tries to go, as in his discussion of the relation in science between orthodox and radical ideas. To him, every discipline sticks only to the tried and true, and is extremely slow and very reluctant to accept innovation which departs from orthodox dogma because of its extreme faith in that dogma or "*consensus*." For example, the ancient alchemists believed that elements could be transformed chemically, but, since no chemist has ever seen that happen, "The discipline of chemistry teaches alchemy to be impossible."⁷¹⁶ As a result,

"One who persists in asserting that elements have been transmuted in other ways than those known to be effective would be classed as a crackpot."⁷¹⁷

Bauer applauds this sort of reaction and explains that the cause of such a response by organized chemistry is the presence in the discipline of a "dogmatism" or "consensus" which must be believed by every "practicing member of a disciplinary community" and which controls everything that is done, believed and expected.

"Consensus reigns on such major issues as types of apparatus or modes of procedure . . . and theories that are regarded as fundamental, virtually axiomatic for that particular discipline at its particular stage of development."⁷¹⁸

All in all, says Bauer, "the consensus . . . serves to define the discipline,"⁷¹⁹ and it is only by obedience to it that the discipline can do good science. That to him is what science is.

This is an inadequate description, however, because, if it were true, we must ask how any revolutionary progress could ever occur. If a discipline is and should be restricted only to methods and experiments and principles allowed by the dogma, and forbids the search for new forces or processes or substances or laws, it would never move forward but would spend all of its time forever refining traditional activities. Such a definition, that is to say, totally leaves out innovation. A rather different description of science than Bauer's is offered by famed sociologist Thomas Kuhn, who sees each discipline as ruled by a "paradigm" which dictates to the practitioners which problems are to be attacked:

⁷¹⁶*Ibid.*, p. 288.

⁷¹⁷*Ibid.*, pp. 288-289.

⁷¹⁸*Ibid.*, p. 289.

⁷¹⁹*Ibid.*

". . . it informs them of the questions that may legitimately be asked about nature and of the techniques that can properly be used in the search for answers to them."⁷²⁰

Most practitioners within the disciplines are therefore merely problem-solvers, and to that extent Bauer follows Kuhn, who agrees that problem-solving is what most "scientists necessarily do most of the time."⁷²¹ Where Bauer falls significantly short of Kuhn, however, is in his discussion of the non-routine episodes *which also are a legitimate part of any science*. As Kuhn has written,

". . . if the normal puzzle-solving activity were altogether successful, the development of science could lead to no fundamental innovations at all."⁷²²

Bauer insists that "a chemist would get nowhere if he tried to keep his mind constantly open to the possibility that" processes could occur "according to some unknown principle or law of nature."⁷²³ Such a definition, however, leaves almost no room for work on the frontier, which can *only* occur (contrary to Bauer), if scientists are prepared to accept the possibility that substances, processes, relations and laws different from "those known to be effective" can and will be proposed and discovered. Bauer is therefore quite wrong about what science is. Imagine for instance the revolutionary work of Ilya Prigogine concerning the alleged universality of Newton's second law of thermodynamics: had he submitted to Bauer's dictum, his ideas would never have come into existence. What Bauer describes is therefore only a *part* of what science is. It is what Kuhn calls "normal" science and what de Bono terms digging existing holes deeper.⁷²⁴ But there is also "revolutionary" science (Kuhn), or the innovative leader who begins digging a new hole (de Bono), and most of these innovators (contrary to Bauer), go *against* the consensus and reject all or part of it, taking radical or "forbidden" paths. Such people cannot simply be dismissed as "crackpots," (as Bauer would have it), for this would make every radical innovator in chemistry a crackpot. On the contrary, we must say that innovators are the *very lifeblood* of a discipline. As Kuhn has written,

"Almost no one, perhaps no one at all, needs to be told that the vitality of science depends upon the continuation of occasional tradition-shattering innovation."⁷²⁵

Kuhn is wrong, for Bauer apparently needs to be told, badly, and that is why his description of scientific activity is so inadequate and misleading. It paints a picture of each discipline as no more than an inflexible set of arbitrary rules, what Kuhn calls a "pre-established game,"⁷²⁶ which every specialist slavishly follows. Yet even Bauer perceives that this dogmatic game may not be the only pathway to truth.

"Note that this does not *necessarily* mean that the alchemist or crackpot is wrong; it merely means that he is not a chemist and is not practicing chemistry. He is not pursuing work

⁷²⁰Thomas S. Kuhn, 'Scientific Paradigms,' in Barnes, 1972, p. 93.

⁷²¹*Ibid.*

⁷²²Kuhn, *op. cit.*, p. 98.

⁷²³Bauer, *op. cit.*, p. 295.

⁷²⁴Thomas S. Kuhn, *The Structure of Scientific Revolutions*, 2nd ed., (Chicago: Univ. of Chicago Press, 1970). Edward de Bono, *The Use of Lateral Thinking*, (Harmondsworth, Middlesex, England: Penguin Books, 1967, 1971).

⁷²⁵Kuhn, *op. cit.*, p. 102.

⁷²⁶Kuhn, *op. cit.*, p. 98.

within the discipline of chemistry, even though he may be working with matter and looking for reactions that take place."⁷²⁷

Is this what chemistry really is, a rigid and narrow "dogma" which labels all deviants "crackpots" and produces work which solves no new puzzles, while the "crackpots" are also doing work on *the very same tasks* but are not "chemists"? Are all innovators crackpots? Are there two (or more) kinds of chemistry? Or has Bauer merely painted himself into a corner? We must remember that Bauer himself is a chemist, and, if he cannot describe even his own discipline, we can have little faith in what he might say about alternate disciplines, and less in what he might say about science as a whole.

In my opinion, a much more adequate (and tolerant and comprehensive) definition of science than Bauer's is offered by Kuhn. As far as "normal, or paradigm-based, research" is concerned,

"The scientist engaged in it does not at all fit the prevalent image of the scientist as explorer or as inventor of brand-new theories."⁷²⁸

When, however, normal science is not successful, a very different but still legitimate type of science comes into existence (one which Bauer has completely failed to consider or describe), which has a separate ethos and a different vision.

"Nevertheless, this image of scientific research as puzzle-solving or paradigm-matching must be, at the very least, thoroughly incomplete Though successful research demands a deep commitment to the *status quo*, innovation remains at the heart of the enterprise. Scientists are *trained* to operate as puzzle-solvers . . . but they are also *taught* to regard themselves as explorers and inventors."⁷²⁹

It is this critical innovative dimension of *every* science that Bauer has failed to handle and seems almost unaware of, yet he believes that he is giving us a true and *complete* description of science.

Where Bauer is even weaker, to the point of appearing not to understand science at all, is on the question of how and why changes of ideas occur in science, and what they constitute. He refers to it merely as "incorporation," and argues that the slowness with which new ideas are "incorporated" into science is planned and beneficial.

"In a nutshell, 'orthodoxy acts as a kind of filter for new ideas,' thereby assuring that order rather than chaos shall prevail most of the time and that new paradigms . . . will have greater utility and reliability than the old ones they replace."⁷³⁰

This bland generalization gives the impression that orthodox or normal science *is set up* in its everyday functioning to routinely incorporate radical, new ideas smoothly and safely, *i.e.*, that what happens when new ideas are "incorporated" is a standard, orderly and welcome process. It is here that we discover that Bauer in effect knows almost nothing at all about what happens in science, for his description of what takes place at times like this betrays his utter ignorance both of the phenomenon of radical change and of the recent thinking about it.

⁷²⁷Bauer, *op. cit.*, p. 289.

⁷²⁸Kuhn, *op. cit.*, p. 95.

⁷²⁹Kuhn, *op. cit.*, pp. 97, 102.

⁷³⁰Bauer, *op. cit.*, p. 297.

To Bauer the change in ideas is simply an infrequent but routine occurrence:

"Occasionally a paradigm has to be replaced."⁷³¹

This is like saying "Occasionally a light bulb has to be replaced." That may be where he gets the notion, quoted above, that "the new paradigm . . . will have greater utility and reliability." Technology (which Bauer confuses with science), is always advancing, and therefore in his mind everything today is better than yesterday, and therefore, when yesterday's light bulb or paradigm is "replaced", today's will be better than the old one. That, astonishingly, is about *all* that Bauer has to say on this immense topic, except for such small phrases as: there is "revolutionary replacement of paradigms," and it occurs when "we are forced at some time to acknowledge the uncertainty of the prevailing paradigmatic knowledge."⁷³² Yet he sees no reason to discuss *how* "we are forced . . . to acknowledge" the inadequacy of prevailing dogma, or *how* "revolutionary replacement" occurs. These vast subjects, which should form the very heart of any competent analysis, are merely tossed off by Bauer as-is, in the middle of unrelated sentences, and left like that. Our conclusion is unavoidable: when, in an analysis of science which takes 123 pages, Bauer devotes almost all of it to orthodoxy and *less than two paragraphs* to the unorthodox, we see not merely a bias and a gross distortion but evidence of what he is familiar with and what he is not. In a word, he displays an almost total ignorance of all the new thinking not merely in the philosophy but also in the history, psychology and sociology of science in the past 40 years, a period of intense re-thinking, rejection of traditional ideas and of controversy. He seems to have no awareness of developments in the post-Popper and post-Kuhn era which have transformed these disciplines and propelled them along pathways not merely unenvisioned by the old *consensi* but forbidden by them. All he offers is a digest of the old thinking, with a token reference to the new here and there to give the (false) impression that he is up to date, which he sadly isn't. He has no idea of the vast field of thought and debate that lies behind his naïve and casual reference to "replacement," or the reverberations that accompany the notion of "uncertainty," but treats the change of ideas in science, in a "consensus," as simple, ordinary and actually minor. We must consequently judge that Bauer's discussion of "new discoveries", which has direct bearing on the question of Velikovsky, is not merely inept or ignorant, but very misleading and wrong, and we will have to take this defect of Bauer's concerning science into powerful account later, when we will come to assess his depiction of the Velikovsky Affair, because his portrait and analysis of that phenomenon will be conducted strictly in the light of such misconceptions as this, of how science as an orthodoxy responds to radical ideas.

Bauer's next error is to insist that science is not "a coherent monolithic structure."⁷³³ There is, he says, no "scientific establishment."

"The only factor that seems to characterize science as a whole is that, in the long run . . . , untruths are weeded out and what remains becomes more reliable."⁷³⁴

That *alone*, in his opinion, is what happens in science, and it all occurs in a sort of perfect democracy.

"I think the best short description of this reality is Polanyi's phrase, the 'republic of science.' . . . In science, there is 'self-coordination by mutual adjustment'—tacit agreements and assumptions are shared and individuals voluntarily . . . accept the consensus. There is 'discipline under mutual authority.'⁷³⁵

⁷³¹*Ibid.*, p. 296.

⁷³²*Ibid.*, p. 297.

⁷³³*Ibid.*, p. 301.

⁷³⁴*Ibid.*

⁷³⁵*Ibid.*, p. 302.

Best of all, there is in this wonderland called science perfect egalitarianism and cooperation and respect such as is not seen in any alternate segment of society:

"... there is no permanent set of accepted leaders, authorities, or an 'establishment'; the authority resides in the tacit 'constitution' of the republic."⁷³⁶

It is like ancient Athenian democracy, a paradise of equality without coercion. That, believe it or not, is actually what Bauer thinks science is, *and in the 1980's, too*.

How are we to respond to this dream fabrication, which sounds like it was written by a public-relations firm? One hardly knows whether to laugh or cry. First, let us look at how he himself contradicts it.

"Every specialist makes judgements . . . Those who make good judgements fairly consistently . . . become the leaders, the authorities."⁷³⁷

A moment ago he had said that "there is no permanent set of" leaders or authorities in science, yet here in his own words he refers to "leaders" and "authorities." What then can his qualifying term "permanent" indicate? That, because the leadership is allegedly not "permanent," science is therefore an egalitarian "republic"? Is America a country composed entirely of equals merely because its President is not permanent? Or the major CEO's? This is either Jesuitical or Bauer is confused. In my opinion, his evasion is very weak, but his error is greater than that, for the "authorities" he has just described are merely *one class* of leader in science. They are individuals, but science also contains *groups* of leaders. It is above all a *social institution*, but Bauer is extraordinarily feeble in his understanding and analysis of that dimension of science (as we shall see just ahead), and there is consequently almost no discussion of it in his portrayal of the wondrous equality of Science.

For instance, what about the various official, organized *centers of power* in science? Are they not "leaders"? How far would an individual scientist get if the topic of his experiment or the principles involved or the instruments used or the result expected clashed with the prejudices of the huge and powerful AAAS (American Association for the Advancement of Science)? Would the average biochemist ever venture to defy the implicit or explicit attitudes toward what is permitted and what is forbidden held by the monolithic National Institutes of Health, which control grant-awarding and grant-sharing in the amount of hundreds of millions of dollars? Either Bauer does not know of, or he is lying about, the immense and dictatorial power that these institutions of science have, or that each is dominated by a small administrative group, or that these cores of power, as a great many recent studies have shown, tend to perpetuate themselves by selecting like-minded new "authorities" to replace those who leave. *None* of this appears in Bauer's description, which renders what he says meaningless. He simply does not understand science.

To get closer to the topic of Velikovsky, has Bauer forgotten that the majority of astronomers who jumped on the anti-Velikovsky bandwagon in 1950 did so without ever having read Velikovsky's book, as Bauer himself notes?⁷³⁸ Is this a mere coincidence (imagine the probability of all of these simultaneous similar responses being coincidental), or could it have been a group phenomenon having something to do with the fact that the opposition to Velikovsky came from the very prestigious *Harvard Observatory*? More pointedly, with regard to Bauer's naive belief that science has no dictators, was Harlow Shapley, senior American astronomer, director of the Harvard Observatory, officer of the AAAS (the American Association for the Advancement of Science) and even of that *second* AAAS at Harvard (the Academy of American Arts and Sciences), member of the board of *Scientific American* and *Astronomy Today* and *Science News* and the Hayden Planetarium, author of more than half a dozen

⁷³⁶ *Ibid.*

⁷³⁷ *Ibid.*, p. 277.

⁷³⁸ *Ibid.*, p. 223.

important astronomy books and the person who led the anti-Velikovsky crusade, a man *without* "leadership" and "authority"? Did all the American astronomers who ridiculed and abused Velikovsky at the same time and in almost the same words merely just happen to fall into line (in Bauer's quote, to undergo voluntary "self-coordination by mutual adjustment"), with the attitudes and actions of the man who, more than anyone else in their field, held literal life-and-death control over their careers? What on Earth is Bauer talking about? What *can* he mean when he calls science a "republic . . . under mutual authority"?

To contradict Bauer's astonishing assertion that science is mutually organized and exists without permanent leaders or institutional coercion or the arbitrary imposition of dogma, many testimonies are available. I will quote from one because it deals with a further center of power unnoticed by Bauer, the journals, and it comes from a practicing Senior Engineer, James McCanney, with many years of personal experience in science and industry. On the topic of routine imposition from above in science, he has this to say:

"A renowned British physicist wrote to me concerning his confrontation with editors of a popular astronomy magazine. He stated that 'there is far too much evidence of suppression of papers which do not take the . . . orthodox . . . model as an axiomatic starting point.'⁷³⁹

Thus, to satisfy those who *control publication*, the consensus must be observed even by renowned physicists, and the same thing happened in McCanney's own career.

"In 1980, in a letter of rejection to a paper submitted for publication . . . a journal editor . . . explained that one must start with accepted theory as presented in the respected journals . . . and add any new work to this framework."⁷⁴⁰

As many new studies have shown, this is entirely true: what scientists in any field are allowed to read or even think about is controlled by a small group of journal editors, who wield a powerful influence. This is hardly republicanism. To relate this more specifically to Velikovsky, it is McCanney's belief, for instance, that astronomy is erected upon a small number of received axioms and that all "'accepted' theories of Astronomy . . . have been developed to conform with these axioms."⁷⁴¹ When data appears which conflicts with these dogmas, Astronomy can either re-consider the axioms (the best path), or try to rationalize away the difficult data (which is less admirable), or (worst of all) sweep the problem under the rug. "Unfortunately," says McCanney, "the peer editing system of our scientific journals only supports the last two possibilities."⁷⁴² Anomalous data is force-fit to the dogma or ignored. The result, naturally, is nothing resembling republican openness (as Bauer would have us believe), but the dominance of the axioms enforced by the power-centers.

"The strict hierarchy of advancement (from graduate student to aged tenured professor) forces underlings to comply with this protocol. Dissenters and original thinkers have no place in this structure. One is only granted permission to build on this pre-existing caste system."⁷⁴³

⁷³⁹James McCanney, "The Axioms of Astronomy," in *KRONOS*, Vol. X, No. 3, (Summer 1985), p. 109.

⁷⁴⁰*Ibid.*

⁷⁴¹*Ibid.*

⁷⁴²*Ibid.*

⁷⁴³*Ibid.*, p. 110.

It is McCanney's conclusion, therefore (wholly contrary to Bauer's naive picture of "incorporation,"), that "Change cannot be expected, however necessary or justified, within this framework" because "the axioms are never questioned" by the power structures that control the discipline.

"... these groups reign as 'the experts,' generally controlling the flow of publishable material to their journals and the popular media. They . . . control the new personnel who enter the field and those who seek employment in their field. They are approached by the mass media to inform the public, and form core groups such as NASA and the American Association for the Advancement of Science. They control who is allowed to referee articles to be published; they receive the government grants to perform new research, and they present the results of their research in the journals which are controlled by their power groups. It is a closed system."⁷⁴⁴

Someone like Bauer might retort that, even though all of this is true, it is a good thing, for it represents responsible leadership of the discipline in terms of reliable concepts and methods. To say that, however, would be to miss the point, for we are not debating here whether the leadership is good or bad, right or wrong, but *whether it exists*. If it does, and all of the latest studies confirm it, then there *is* dictatorship. As the sociology of science has shown, there *are* leaders and there *are* power centers in science, powerful and entrenched and authoritarian ones, and there is imposition from above, and Bauer is therefore totally wrong on this point.

In McCanney's opinion, these centers of control do most certainly exist, and they have not only enormous *public* influence but also the punitive power within the discipline to keep the average practitioner in line.

"Anyone outside the system who dares to contradict it is easily labelled a . . . crackpot, pseudoscientist, etc. . . . Insiders who defect can be singled out and eliminated. No one will offer public support since the same fate may befall them."⁷⁴⁵

This is dictatorship, not republicanism, and McCanney's practical observations correspond with what every recent historical and sociological study of the thorny issues of institutional power, grant allocation and peer review in science has found, which together present a compelling portrait of rigid institutional control, yet Bauer appears to be singularly unaware of this powerful and important dimension of science. Once again, therefore, he simply does not know what he's talking about. Science *as it functions* is not remotely as Bauer describes it.

The next of Bauer's critical faults we will discuss is his tendency to express himself in "wishy-washy" language which destroys beneath the surface what he says on the surface about science. Bauer tries to express things definitively so that he may then use what he has said as a measure by which to evaluate Velikovsky. However, when his utterances slide again and again into a bog of vagueness, this renders what he says useless as a standard of measurement, for it then means almost nothing, and that I'm afraid is what happens to Bauer all too often. He may not see that it is funny because he is not aware of his unscientific vagueness, but it is funny to us for two reasons. First, here is a man attempting to attain precision through imprecise words and he does not know it, and, second, this is the very man who in the same book vigorously attacks others for this fault. This is one more instance of self-contradiction (one of many), which sadly characterizes Bauer's attempt to talk learnedly about science.

For instance, at one point he mocks Martin Gardner for being wishy-washy in his style of writing, a characteristic which, Bauer observes, renders Gardner's argument "unsure." (O that Bauer had been able to see the beam in his own eye!)

"'Since the turn of the century . . .,' wrote Martin Gardner, ' . . . widespread opposition by scientists to a legitimate theory, based on verifiable evidence and cogent reasoning, is an

⁷⁴⁴ *Ibid.*

⁷⁴⁵ *Ibid.*

increasing rarity’ There is internal evidence here that Gardner was not so sure of the point he was asserting.”⁷⁴⁶

The evidence, in Bauer's opinion, is the presence in the passage "of unquantifiable adjectival judgements: ‘widespread,’ ‘legitimate,’ ‘verifiable,’ ‘cogent,’ ‘increasing.’" Because these words step back from certainty (as the reader may easily see), "the statement is qualified to virtual meaninglessness."⁷⁴⁷ To Bauer, *it says nothing*.

Yet here is how Bauer expresses one of his major points, the resistance of science to new ideas.

"The reassuring thing is that resistance, when ‘unwarranted,’ is eventually overcome by the merits of the case. Ultimately science deals in observables, in concrete events, and in the long run theories are accepted or rejected on the basis of their success in explaining data If resistance to a new discovery is not objectively sustainable, the resistance eventually withers away.”⁷⁴⁸

This is literally how he seeks to explain the process by which new ideas are "accepted" by science, and it is crucial to his overall argument. Since he presents himself as an expert in the analysis of scientific writing, we are naturally obliged to measure him by his own rod. The moment we apply Bauer's own model to himself, however, we see unfortunately that he too is unsure of the point he asserts.

The clue is the presence in Bauer of the same sort of "escape clauses" which he found in Gardner, which render his description of science equally meaningless: "eventually" and "ultimately" and "in the long run" and "objectively" and once again "eventually." Such words *do not allow us to make anything of his sentences*. When is "eventually"? How long is "in the long run"? Or "ultimately"? Does science *not* deal in observables before "ultimately" is reached? *Do* "unwarranted" theories become warranted? How does resistance to them "wither"? What on Earth can "wither" mean scientifically? Does it take a year or a century for "eventually" to be reached? How can one tell regarding any particular concept in science if it is "withering"? Or waxing? Or in an in-between stage? Does Bauer have a litmus test for this? If not, how does he *know* that resistance "withers" "eventually"? What furthermore is the *rate* at which theories "wither," and is the rate for "eventually" different from that for "in the long run"? (Remember that Bauer had insisted that Velikovsky quantify his predictions or they would be unusable.) Apparently Bauer does not feel it necessary to apply the same stringency to himself.

To continue: who can tell which theories that are unwarrantable today will become warrantable tomorrow? Is Bauer such a prophet, or, if not, are there recognized and warranted prophets in science who *know* when the state of warranty will be attained by a theory? I ask this because it is Bauer *himself* who asserted that the consensus which he so admires in each discipline determines not merely the problems and methods and theories which are permissible, but even the very future of the discipline, offering judgements

". . . about which of the presently accepted concepts and theories may need revision at some time and which others will ‘never’ need revision”⁷⁴⁹

What is Bauer saying? That each consensus knows *in advance* (and absolutely) just when new ideas or revisions will appear (not to mention where new ideas will "never" appear)? Did everyone in physics in 1899 know not only what Planck would say in 1900, but even when he would say it? We must wonder how this astonishing prescience occurs in the same way in every discipline. Also, Bauer does not tell us how this astonishing ability functions, he

⁷⁴⁶Bauer, *op. cit.*, p. 295.

⁷⁴⁷*Ibid.*

⁷⁴⁸*Ibid.*, p. 299.

⁷⁴⁹*Ibid.*, p. 290.

merely asserts it in unquantified language. (We wonder, for instance, if it has an accuracy of 100%, or 99%, or less.) Is this a reasonable picture of science? Can anyone "use" such statements? What on earth can this cluster of meaningless utterances mean?

Bauer commits the same fault of wishy-washiness later, again when he is seeking to define the essence of science (as if he is unsure each time of what he is saying). This time, however, the vagueness is coupled with Bauer's frequent fault of self-destruction by self-contradiction. Let us see how both of these are to be found in one passage.

Earlier in his book, Bauer had severely castigated Carl Sagan for offering his readers a much-too-laudatory and incorrect picture of science in his attack on Velikovsky.

"Historians, philosophers, and sociologists of science could well take exception to his statements that 'science is . . . self-correcting . . . This self-questioning and error-correcting aspect of the scientific method is its most striking property.'⁷⁵⁰

"That description," says Bauer, is excessive—"These statements all need qualification"⁷⁵¹—yet, when Bauer himself comes to describe science, he says *virtually the same thing as Sagan*:

". . . in the long run the unreliable is weeded out, and scientific truth is self-determining, emerging from disciplined activity . . . after much trial and error, the only criterion over the long haul being reliability."⁷⁵²

This is delightfully ironic, for, if Sagan is wrong about science's ability to keep itself continually free of error, then, because Bauer *has said the same thing, he must be wrong too*. Once again, he has contradicted himself and is not aware of it. A greater fault in this passage, however, is the presence of the "escape clauses" which he so abhors in writing not his own. Notice "in the long run," "trial and error" and "over the long haul." Are these scientific descriptions? Can we "do" anything with them? Bauer here is trying to define the *central* concept of this section of the book, but what he offers (by his own standard) is non-scientific, undiscussable, unverifiable and therefore useless. How long is "the long run"? What events precisely are contained in "much trial and error"? What on Earth can "over the long haul" mean scientifically? Is it longer than "in the long run"? By how much? If we apply Bauer's model to himself, we see that once again he is tarred by his own brush. The unscientific lack of quantification, and therefore uselessness of his attempt to describe science, forces us to decide, in Bauer's own words, that what he has said here "is once again mere hand-waving, or worse—from a technical standpoint it is gibberish."⁷⁵³ It is, as he said of Velikovsky, "a collection of genuinely meaningless statements"⁷⁵⁴ because, again in his own words, "There is nothing here that [we] can use, can attempt to think about in a meaningful way."⁷⁵⁵ Poor Bauer is once more hoist with his own petar.

I must add a note here about a third fault of Bauer's to be found in his criticism of Sagan, one which is utterly unscientific and which I feel is the deepest reason for his animosity towards Sagan. Even though in my opinion almost all criticism of Sagan is richly deserved because of the many scientific errors he commits (see the chapter on the AAAS Symposium and Ginenthal's book on the subject),⁷⁵⁶ faults which justify Bauer's assault

⁷⁵⁰*Ibid.*, p. 224.

⁷⁵¹*Ibid.*

⁷⁵²*Ibid.*, p. 302.

⁷⁵³*Ibid.*, p. 125.

⁷⁵⁴*Ibid.*, p. 124.

⁷⁵⁵*Ibid.*, p. 125.

⁷⁵⁶Charles Ginenthal, *Carl Sagan and Immanuel Velikovsky*, (Tempe: New Falcon Press, 1995).

scientifically, I suspect that Bauer may also have a *hidden* motive for attacking Sagan as often and as severely as he does. I think that Sagan is singled out in Bauer's book for special demolition not primarily because he is wrong scientifically (which he is frequently), but because he has dared to cast scorn upon Bauer's pet obsession, the Loch Ness Monster. I feel that Bauer takes a special joy in humiliating Sagan for this reason, which is hardly the appropriate emotion for the sort of man Bauer purports to be. This is less than scientific.

There is, next, Bauer's totally inadequate analysis of "Science as an Institution."⁷⁵⁷ This should have been the core or linchpin of his argument, because science responded to Velikovsky mainly as an institution, but Bauer demonstrates that he has completely failed to understand this. (For an analysis of the Velikovsky Affair as an irrational *tribal* phenomenon, see my final chapter of this book, especially the section "Phase One: The 1950's," where the collective, club-like nature of the response of American astronomy to Velikovsky is presented.) Bauer in this section does not refer to *any* of the historical or sociological studies of the Affair which were available when he wrote (published both by supporters *and* detractors of Velikovsky), as if he did not know they existed. All we get from him, as *the sole content of this section*, is an analogy between science and the army, which in my opinion was selected to allow him to say only certain (positive) things about science and to not have to speak of negative elements. *That's all there is*. It is quite astonishing, for to handle what should have been so important a section in this lightweight way is not merely feeble and inadequate, it may be deceitful, for it ignores (or rather trivializes) a major element of the Affair to make it look as if this element played no role. This should have been a most important point, but, because Bauer does not really understand the Affair (as we shall see in a moment), it is quite unimportant to him. The section is therefore useless.

Then Bauer does something else in this section which in my opinion demonstrates a very deep ideological uncertainty beneath the tone of certainty he employs. It may explain why he dared go no further than comparing science to the army. The error I refer to (of which he is guilty not only in this section) is this: after he has finished the bulk of his analysis, which says one thing, he then throws in at the end, *in only one sentence*, the counter-evidence which says the opposite. To be specific, after several pages trying to prove that "Most of the time the army—like science—is wondrously powerful and effective,"⁷⁵⁸ that each activity "has a set of ideals," that for both "the performance does not fall all that far short of the goal,"⁷⁵⁹ and therefore that "The performance of the institution transcends the limitations of the individuals who comprise it,"⁷⁶⁰ we suddenly get this:

"This institution of science, then, differs from other human institutions in those details that pertain to its specific task and subjects of concern but not in any other way."⁷⁶¹

Then the section ends.

What on Earth can this last-second turnabout mean? If Science differs from "other human institutions" only in its task "but not in any other way," then Bauer seems to be acknowledging (whether he intends to or not) that science can be as *corrupt* as every "other human institution," for every kind of human institution that we know of (governmental, political, religious, educational, military) has historically been riddled with selfishness, bias, coercion, manipulation, self-interest, greed and duplicity. Is this what Bauer means? If he wants to uphold the integrity of science, these are hardly effective comparisons, for the final sentence *destroys* what he had tried to establish before. Furthermore (to carry Bauer's idea to its absurdity), garbage men have a different *task* from science, but does it mean that, apart from that difference, the doing of science is no different "in any other way" from the collection of garbage? Is this what Bauer means? Once again we are left wondering what Bauer's true opinion is, for these statements make no sense. They are "without meaning," of "no use," "gibberish," yet this is all that Bauer offers on "Science as an Institution." Worst of all, after having admitted that the "institution of science" is human, he just drops the issue there without following it through. This should have been the point at which he would then

⁷⁵⁷*Ibid.*, pp. 302-308.

⁷⁵⁸*Ibid.*, p. 304.

⁷⁵⁹*Ibid.*, p. 309.

⁷⁶⁰*Ibid.*, pp. 305-306.

⁷⁶¹*Ibid.*, p. 306.

begin to explore the debate on this topic, for it is a central issue in the philosophy and sociology of science, but he does nothing more, as if he has not even understood the importance of the problem he himself raised with his last-minute turnabout. The whole analysis is therefore grossly unsatisfactory, and makes us very dubious about his ability to discuss science.

There *is* a reason why Bauer deep down is unsure of the objectivity and republicanism of science, but we have to dig it out for ourselves, for Bauer seems unaware of its importance. It occurs, significantly, in the section "Scientific Activity" and shows once again that Bauer fails to understand how science actually occurs. After arguing for several pages that science is neither monolithic nor authoritarian but proceeds pluralistically and rationally (which allows him to assert that science's rejection of Velikovsky was objective and unforced), we suddenly find Bauer, at the *end* of the section, contradicting himself again. It is the same syndrome as before. He refers to an article which appeared in *Harper's* in 1963, more than a dozen years after the first phase of the Velikovsky Affair, written by Hadley Cantril, Chairman of the Board, Institute for International Social Research and past president of the Society for the Psychological Study of Social Issues, in which Cantril as a social scientist sums up *his* opinion of scientific activity.

"In tones of indignation Cantril concluded that 'in theory, science's "reception system" is based on the objective, rational evaluation of submitted evidence. In practice, however, . . . it operates much like other social institutions, complete with hierarchy, dogma and coercive power . . .'"⁷⁶²

What is this, we ask ourselves. Is Bauer going to destroy at the end everything he had said before about the impersonality, fairness and non-rigidity of science? Won't he even try to deny that science is riddled by "hierarchy, dogma and coercive power," (or *leaders, led, and pre-existent belief*), as he has done to this point? (Does he not foresee that, if he admits that these elements exist in science, he topples his entire argument?) Here are his words in reply to Cantril's assertion that science is hierarchical and coercive:

"But of course—how could it be otherwise? Science is an activity carried on by human beings, so all these elements are inherent in it."⁷⁶³

Whoa, now, Henry, just a moment! Which is it? Is science mainly subjective, as Cantril says, or is it mainly objective, as you think? It can't be both. Bauer tries, however, to back away from the extremity of his own admission by qualifying Cantril's judgement about science:

"But there is democracy as well as hierarchy, openness as well as dogma, lack of coercion as well as coercion."⁷⁶⁴

Each time, however, that he uses the damaging phrase "as well as," his attempt to reconcile himself and Cantril ends with the plain acknowledgement (totally contrary to what he had said before) that any adequate definition of science has to make a place for the human (and therefore corrupt) element in its description.

". . . science is a human activity and . . . you have, therefore, to take into account the properties of human beings when you are assessing facts and theories . . ."

⁷⁶²*Ibid.*, p. 303.

⁷⁶³*Ibid.*

⁷⁶⁴*Ibid.*

to which he adds,

"or any other facet of science."⁷⁶⁵

Every dimension of science, he is saying (including presumably its response to new ideas), as well as every power center, is open to pressure and manipulation by human emotions.

What are we to make of this turnabout, which constitutes a virtually complete denial of the definition of science he had offered up to that point? Does Bauer make any sense? Some readers might not notice his reversal, however, because of a major fault in Bauer's *form* of argumentation (which we have seen before), which in this: after some 30 pages of trying to establish a certain description of science (that it is impersonal and objective and task-driven), he then tosses in the counter-evidence (that it can be very personal and passion-driven), only at the end, almost as an afterthought, and *then does nothing with it*. Had he been arguing like a true scientist, then, in the face of such a damaging admission, what should have followed it was a complete new section debating the merits of these two views, for they are in direct conflict, followed by his carefully-reasoned choice. This should have been done because the issue raised at the end powerfully affects any concept of the nature of science. It is no less than what every post-Modernist sociologist and psychologist and historian of science has been discussing for the past three decades. Bauer, however, gives it almost no weight. Rather than recognizing the importance of the issue he raised at the end and making it the center of a solid piece of subsequent analysis, as every more modern researcher would, he passes over it pointlessly, stating it but then running off to a different point. It is as if he simply failed to grasp the meaning of what he said. Because this counter-point comes at the conclusion of his long analysis of science and is nevertheless *made nothing of*, we have to conclude (alas) that Bauer's knowledge of what science is is not merely incomplete or theory-derived, or even self-contradictory, but distorted, biased and ultimately false. It is preposterous. He tells us A, he argues B, he asserts C and D, and then, quite casually and without further development or adequate integration, he mentions the counter-evidence E, but rushes on. In the end, therefore, we have to conclude that in each of these instances he has said nothing of value.

If we seek a model by which to evaluate Bauer on science, then all we need do here, as we have done before, is to turn to Bauer himself, who as usual will provide the brush with which he himself must be tarred. We will simply take his condemnations of Velikovsky and substitute his own name and topic at the appropriate point and he will be most suitably (and deservedly) destroyed. For instance, he says this about Velikovsky's theory of gases:

"It is surely reasonable to expect that one who purports to discuss . . . the behavior of gases should be familiar with these concepts."⁷⁶⁶

The same is true, my dear Henry, of one who sets out to discuss the behavior of science, yet your weaknesses on the subject reveal *you*, as you said of Velikovsky, "to be ignorant of some of the most elementary ideas and approaches."⁷⁶⁷ You are also guilty of a second fault of which you accused Velikovsky:

"Most strikingly, this ignorance is coupled with a readiness to discuss these subjects with an air of expertise."⁷⁶⁸

⁷⁶⁵ *Ibid.*

⁷⁶⁶ *Ibid.*, p. 108.

⁷⁶⁷ *Ibid.*, p. 94.

⁷⁶⁸ *Ibid.*

How perfectly you have described your own pompous dogmatism, Henry, and we will therefore have to heed your own warning concerning "how much credence to give to an expert's judgement."⁷⁶⁹ You explained that the expert is familiar with the problems in "the tiny area of science over which he has contemporary mastery . . . and his ego is not threatened as he confesses ignorance and paradox."⁷⁷⁰

"But take a man just a little out of his field, a little out of his depth, and he is likely to become more dogmatic. He is less certain of himself because he is not familiar with all aspects of the subject, and so he may become even more dogmatic."⁷⁷¹

To which we may add that, if he knows almost nothing about the field, he becomes as dogmatic as Bauer. As we did at the end of the previous section, therefore, we must also find Bauer guilty here of a further fault he attributed to Velikovsky:

"When a man makes a fool of himself by discussing, at ponderous length, a subject of which he is ignorant, I tend to assume that he will act similarly in other fields."⁷⁷²

Right on, Henry! You have just spent *123 pages* vainly failing to describe how science actually occurs, and, because you have made a fool of *yourself* (being so wrong on so many essential points), must we not assume as we did before that your entire book must be similarly foolish and worthless? (That *is* after all how you assessed Velikovsky, and, since you yourself tell us that you are an expert, are we not obliged in all reason to follow your lead?)

This is Bauer's Pollyanna vision of science. Unfortunately for him, the reader may remember that Bauer had promised at the beginning to reject misconceptions, stick to reality, be purely descriptive and avoid theory which would clash with the concrete. What he has provided for us, however, is the opposite. It is full of misconceptions and is derived only from a theoretical model with which he appears to be "enamored," and as a result it does just what he said it would not do—it "clashes . . . with what actually takes place" in science. The aim is skewed, and the performance is ludicrous.

There is a discernable cause for Bauer's errors, and it is to be found in the model of scientific behavior put forth by sociologist Robert K. Merton of Harvard as early as 1942 and echoed for example by Michael Polanyi at the same time. In these models we can discover most of the romantic things Bauer had to say about science, especially its republicanism and self-correctiveness, its universalism and disinterestedness, and so on. Merton proposed these in his attempt to formulate the standards and also the very ethos of science, and his views, soon to become known as the "Mertonian norms," served as the foundation for the newly-emergent sociology of science. Because he was alone in his field (and worked at Harvard), his norms reigned unrivalled for decades, being "for a long period the only theoretical approach available . . . the only maturely developed framework for the sociological study of science."⁷⁷³ The reason why Merton's portrait was so quickly accepted by mainstream science (and taught as a dogma to all graduate students) was that it was extremely flattering, for in it scientists were pictured as literally the finest class of people in the world, the most sharing, the most honest, the least selfish or greedy, having as their common goal (unlike the typical selfish greedy individual or group) only the pursuit of knowledge for its own sake. It is true that, by the 1960's and 1970's, not all *sociologists* of science accepted Merton wholly, but, as for the *scientists*, two generations of American researchers were raised on this Harvard religion because it is so attractive

⁷⁶⁹*Ibid.*, p. 294.

⁷⁷⁰*Ibid.*

⁷⁷¹*Ibid.*

⁷⁷²*Ibid.*

⁷⁷³Norman W. Storer, 'Introduction,' in Robert K. Merton, *The Sociology of Science*. Chicago: Chicago Press 1973, p. xi.

and seductive, and it is this pap which Bauer echoes in his book, as do Sagan and Asimov, because they were all fed it and unthinkingly accepted it as part of their ordination as scientists. (As Bauer himself has written, philosophical debates about science, such as are carried out by philosophers or sociologists, are "seen by most scientists as irrelevant.")⁷⁷⁴ They simply believe what they are told, and that includes Bauer. We may deduce, therefore, that Bauer simply sees the world of science through Merton-colored glasses and recites the dogma dutifully like a catechism. Merton's *theoretical* construct, with which Bauer the scientist is "enamored," is the origin of most of his erroneous perceptions. This is all that Bauer's attempt to describe science comes to.

Bauer's mere parroting of the dogma, however, exhibits his most disastrous weakness regarding science, which is that he appears to be ignorant of almost everything that had been published in the philosophy, history and especially the sociology of science since Merton, and this renders his account of science useless. He is totally unaware, for instance, of the work of Barnes, Hagstrom and Mulkay, or of Feyerabend and Lakatos, or of Popper, Laudan and Habermas (to mention only a few), and only marginally of Kuhn (all of which had appeared before he wrote),⁷⁷⁵ but writes as if the Harvard dogma of 40 years prior to his book still prevailed. Bauer is therefore ironically the best example of his own observation that

"Scientists are very rarely exposed, as part of their training, to the history or philosophy of science; when they think about those matters, they readily do so in terms of an ideal, making the naive assumption that actual behavior in science has progressed toward that ideal as the substantive body of scientific knowledge has grown."⁷⁷⁶

It is an appalling ignorance, which leads me to feel that Bauer could only have been willing to say what he did about science in public, in his book, because he did not know what was going on in the field in which he set himself up as an expert. No one forced him to discuss this topic but, once he has, we see that he "does not know what he is talking about." We must, therefore, judge that on this topic, as on the first one we analyzed, he is (as he said nastily of Velikovsky), "an ignoramus masquerading as a sage." He should have done much more research.

3. Bauer's Version of the Velikovsky Affair

We had found in the first section of this chapter that Bauer's explanation of why "people" believe Velikovsky was inadequate, unsubstantiated and sneaky. He did not know what he was talking about. In the second section, we discovered that he was ignorant about science. His attempt to provide a "description of what science really is" turned out to be uninformed, reductive and ignorant of current thought. There too, he did not know what he was talking about. Here in the third section, we will assess his attempt to explain the Velikovsky Affair in terms of what he had set up before. He will try to establish that, while the average "human" might believe Velikovsky, the (more than human?) scientist does not, and therefore that the Velikovsky Affair was an unfortunate difference of opinion between *all* scientists, every one of whom disbelieved Velikovsky, and the unwashed public, *all* of whom would not accept the expert judgement of science. If, however, Bauer is embarrassingly and pathetically wrong in the steps leading to this new section, then unfortunately we should expect (applying his own dictum that a man who makes a fool of himself on one topic "will act similarly . . . at other times"), that this new section will be as

⁷⁷⁴Bauer, *op. cit.*, p. 251.

⁷⁷⁵Barry Barnes, ed., *Scientific Knowledge and Scientific Theory*. London: Routledge and Kegan Paul, 1971; W. O. Hagstrom, 'Gift-Giving as an Organizing Principle in Science,' in Barnes, 1972, pp. 105-120; Michael Mulkay, 'Cultural Growth in Science,' in Barnes, 1972, pp. 126-142; Paul Feyerabend, *Against Method*. London: NLB, 1975; Imre Lakatos and Alan Musgrave, eds., *Problems in the Philosophy of Science*. Amsterdam: Reidel, 1968; Karl Popper, *The Logic of Scientific Discovery*, London: Hutchison, 1959; Laurens Laudan, *Progress and its Problems: Towards a Theory of Scientific Growth*. California: Univ. of California Press, 1977; Jurgen Habermas, *Knowledge and Human Interests*. Transl. Jeremy Shapiro, Boston: Beacon Press, 1971.

⁷⁷⁶Bauer, *op. cit.*, p. 296.

ignorant as the ones upon which it draws, and Bauer does not disappoint us. (He is always the best example of his own criticisms.) We shall find that Bauer's analysis of the Velikovsky Affair outdoes the previous two sections in incompetence, for it is not simply incorrect on its own terms, but much worse: it *completely misses the mark* and ends up not talking about the Affair at all. Bauer's version of the Velikovsky Affair turns out to be an inversion, or, more properly, a non-version, to the point where it is almost meaningless.

Let us look at what he says. According to Bauer, the Velikovsky Affair was a disagreement between "the public" and "science" about a specific matter which the public did not understand. The sort of controversy that arose, therefore, "must be expected when technical matters become the focus of public debate."⁷⁷⁷

Bauer quotes one scientist as saying that, in the Velikovsky Affair, "The nonscientific public came close . . . to demanding the right to pass judgement on scientific questions."⁷⁷⁸ As a result, matters became "messy, confused . . . because there was no substantial area of tacit agreement between the arguing parties."⁷⁷⁹ As far as Bauer is concerned, however, there is nothing to complain about regarding the verdict of science on Velikovsky.

". . . there was nothing unfair, no injustice, in the complete dismissal of Velikovsky's *ideas* by scientists, even after only quite cursory acquaintance with them . . . after some 30 years there is still no sign that the scientific community believes Velikovsky's ideas to deserve any attention at all; the original scientific judgement still stands."⁷⁸⁰

I will argue later that the picture which Bauer presents here is incorrect on its own terms because the response by "science" was not a "complete dismissal" and the reaction by "the public" was not unanimously in favor (*i.e.*, his parameters do not describe the phenomenon). We will overlook this for the moment, however, in order to continue presenting Bauer's version of the Affair. To Bauer, the main reason for the "complete dismissal" was the extreme radicalness of Velikovsky's ideas.

"In science the speculative ideas will be ignored or rejected unless they connect in some acceptable manner with existing knowledge."⁷⁸¹

Velikovsky, however, "showed no inclination to attempt to fit his ideas with any part of established knowledge,"⁷⁸² and therefore "there are sound scientific reasons for ignoring Velikovsky's ideas."⁷⁸³ This too is an incorrect statement, for Velikovsky (as even Bauer himself admits) carefully explains to the reader which elements of "established knowledge" he is putting into question, and even where the differences are not as great as his critics had made them out to be. (*e.g.*, Bauer, 19.) Bauer, however, is either unaware of his own counter-evidence, or he disregards it so that he may continue to construct his exaggerated portrait of the Affair. For example, Velikovsky may have been original, but "In science, what is acclaimed is originality . . . that has connections with what was previously known."⁷⁸⁴ If there are none (and, to defend this, Bauer must dismiss *every* single thing Velikovsky said as "unconnected"), then anyone publishing such idle speculations should not be "surprised at the chorus of hoots and

⁷⁷⁷Bauer, *op. cit.*, p. 309.

⁷⁷⁸Bauer, *op. cit.*, p. 311.

⁷⁷⁹Bauer, *op. cit.*, p. 315.

⁷⁸⁰Bauer, *op. cit.*, p. 279.

⁷⁸¹Bauer, *op. cit.*, p. 261.

⁷⁸²Bauer, *op. cit.*, p. 262.

⁷⁸³Bauer, *op. cit.*, p. 264.

⁷⁸⁴Bauer, *op. cit.*, p. 263.

jeers" it will provoke.⁷⁸⁵ To Bauer, that is no more than a "foreseeable reaction."⁷⁸⁶ The Velikovsky Affair, hoots, jeers and all, was normal and understandable.

Such is Bauer's picture, and by means of it he is free to declare that "the "injustice" done to Velikovsky's ideas was only that scientists judged them to be not science."⁷⁸⁷ That in his opinion constitutes the *entirety* of the Velikovsky Affair. He is aware that some scientists did *occasionally* behave unethically towards Velikovsky, and concedes that, while the response by science to Velikovsky's theories was correct, "The treatment accorded Velikovsky, of course, is a different matter,"⁷⁸⁸ but he tends to gloss over all of this as secondary, as mere excess, understandable and forgivable. Why? Because science was driven beyond its patience, that's why.

"Finding that their collective judgement was not being accepted at large, the critics responded not by taking extra pains to clarify their argument but by becoming increasingly impatient and strident."⁷⁸⁹

According to Bauer, the scientists who responded to Velikovsky started out "patient" and "non-strident," but were pushed by the adverse "public" response to their patience into the excesses of impatience and stridence which some of them (regrettably) committed. They then ridiculed Velikovsky rather than debating him, and Bauer righteously criticizes them for it.

"Ridicule is certainly unnecessary, and certainly some of Velikovsky's critics could have comported themselves in a more gentlemanly manner."⁷⁹⁰

That, however, is as far as Bauer will go in condemning science, and, even then, he adds that what the scientists did is entirely the fault of "the public."

"In the Velikovsky Affair scientists chafed when they found their authoritative judgements not being accepted without question by the public (or by intellectuals and scholars who were not scientists)."⁷⁹¹

I will argue extensively in my last chapter that this concoction of Bauer's is not at all what the Affair was, that the "chafing" of the scientists derived from a reason very different from the one Bauer offers (a reason far more emotional and unconscious), and that most of what was said by certain scientists about Velikovsky, especially in the explosive period around the publication of *Worlds in Collision* in 1950, never even remotely approached the class of "authoritative judgements." They were outraged squawks and attempts by certain scientists to suppress, blackmail, ridicule and defame, as even a glance at the first chapter indicates. Bauer's analysis of the Velikovsky Affair is therefore wrong *ab initio*, wrong in every part, for he sees it totally incorrectly, but it will prove interesting if we look at it Bauer's way first, to see how incorrect he is even on his own manufactured terms.

⁷⁸⁵*Ibid.*

⁷⁸⁶*Ibid.*

⁷⁸⁷Bauer, *op. cit.*, p. 287.

⁷⁸⁸*Ibid.*

⁷⁸⁹Bauer, *op. cit.*, p. 210.

⁷⁹⁰Bauer, *op. cit.*, p. 280.

⁷⁹¹Bauer, *op. cit.*, p. 310.

Let us take his version point by point. His first argument, after envisioning the Velikovsky Affair as a deadlock between "science" and "the public," is to argue that what underlay the Affair was a question of whose *judgement* would prevail, "the public's" or "science's." In his opinion, the entire blame for the Affair must be shifted from science to "the public" because it did not respond to science's judgement as scientists do, but sought to impose its judgement on science.

"Scientists were bound to be criticized for unfairness, injustice, and the rest unless they accepted the judgement of others on matters that are rightly the scientists' business, matters of scientific judgement."⁷⁹²

Matters which are, in plain words, none of the public's business. How should the public have responded? In what way do scientists (as opposed to "the public"), accept such judgements? Here is Bauer's fairy-tale version of the response the public should have followed.

"Within the disciplines it is an everyday event that theories are rejected . . . on the basis of nothing but the judgement of qualified practitioners."⁷⁹³

These rejections by science, however, can sometimes be delivered in a most un-republican manner.

"Nor is it uncommon for rejection to be given in *ex cathedra* fashion, sometimes accompanied by sarcasm and ridicule, even *ad hominem* remarks."⁷⁹⁴

(Is that how things happen in science? If so, it is hardly the idyllic Merton-world, is it, Henry?) Nevertheless, even if the rejection of a theory or proposal by science for scientific reasons may contain some very unscientific elements, says Bauer, how does the rejected scientist react in Bauer's fairy-tale? Is he angry? Is he resentful? Not at all, Bauer tells us, not even if he totally disagrees with the referees. The typical scientist, according to Bauer, simply accepts that that's the way it is.

"Those who are on the receiving end of such rejections do not always agree that the judgements are correct."⁷⁹⁵

But, no matter how "poor, false, wrong" (Bauer's own words) the rejected individuals may feel the judgement to have been,

". . . they do not usually complain that the judgements were unfair or that an injustice has been committed."⁷⁹⁶

⁷⁹²Bauer, *op. cit.*, p. 280.

⁷⁹³*Ibid.*

⁷⁹⁴*Ibid.*

⁷⁹⁵*Ibid.*

⁷⁹⁶*Ibid.*

What do these model human beings do, according to Bauer? "It is simply accepted that these are matters of *judgement*, inevitably fallible."⁷⁹⁷ The rejected scientist simply says "Oh, well" and takes it all with perfect equanimity.

That, believe it or not, is how Bauer sees the Affair. Had Velikovsky and all of his supporters and the gullible public shrugged their shoulders in the face of science's judgement and said "Oh, well," like all good scientists do, even if they felt that the "judgement" was very fallible, there would have been no Affair. To Bauer this mitigates entirely the accusation that science behaved (in his own words) with "sarcasm and ridicule" towards Velikovsky. In his eyes it does not matter, for it is all Velikovsky's fault. Had he reacted differently (as *all* scientists allegedly do), the Affair would not have occurred. (We will only spend a moment to note here that Bauer contradicts himself once more when he says that all of science's "judgements" are "authoritative," but in the next breath tells us that they are "inevitably fallible." Can't he make up his mind?)

The second accusation against science which Bauer attempts to dismiss under this rubric is that science deliberately failed to publish test data which would support Velikovsky.

"It is often said that scientists have an ethical obligation to publish *all* their data, not to select only what supports their hypotheses and to discard the rest."⁷⁹⁸

Reference is being made here to the most notorious example of this practice in the Velikovsky Affair, when dates obtained from radiocarbon analysis of objects from the tomb of Tutankhamen, performed by the British Museum in London, dates which Bauer himself understands "were in conflict with conventional chronology . . . but would have given some support to Velikovsky's revised chronology,"⁷⁹⁹ were not published. This is the sort of thing that led to these complaints.

Bauer, however, sees no problem in any of this: "the discarding of some data is normal, permissible, and expected."⁸⁰⁰ It would be wrong, he says, to discard data which support a competing hypothesis "to one's own," but, if the discordant data point to no known or accepted alternative hypothesis (Velikovsky's revised chronology being an "unknown" hypothesis), why then, as in the results from the Egyptian tomb, "the data would naturally have been discarded" on the very proper "presumption that something must have been grossly wrong, presumably contamination of the samples."⁸⁰¹ In his opinion, therefore, "the discarding was not an unethical act."⁸⁰²

There are several major faults with Bauer's explanation. First, Bauer seems unaware how deeply his explanation contradicts his own standards, for he himself has insisted that science must always go from data to law to theory,⁸⁰³ yet here he upholds the reverse, allowing data to be discarded the moment it disagrees with theory (or agrees with Velikovsky's theory). It does not occur to him that this procedure which he terms "normal" violates the scientific process that he himself upholds, nor does he ever consider that perhaps a theory ought to be discarded or questioned if it does not agree with the data. Which comes first, Henry, data or theory? Please make up your mind.

Second, to go beyond Bauer's explanation and look at what occurs in science itself, we have to ask: Is science really that rigid? One wonders how science ever makes any progress if it always behaves in the inflexible theory-driven manner Bauer proposes, for change, as we have already discussed, is often initiated by the unassimilable anomaly. Bauer in contrast contemptuously dismisses the immense amount of radical evidence which Velikovsky had assembled to buttress his historical revision as merely a few "lonely facts" and then says that, as lonely facts, they were rightly ignored by orthodox historians because they were "unconnected with the mainstream

⁷⁹⁷*Ibid.*

⁷⁹⁸Bauer, *op. cit.*, p. 283.

⁷⁹⁹*Ibid.*

⁸⁰⁰*Ibid.*

⁸⁰¹*Ibid.*

⁸⁰²*Ibid.*

⁸⁰³Bauer, *op. cit.*, p. 258.

of the discipline."⁸⁰⁴ Two pages later, however, this argument is demolished when he contradicts himself by conceding that

"It may, of course, happen that the dates are later found to have been correct after all, the lonely facts right and the accepted theories wrong."⁸⁰⁵

Bauer is guilty here of the same glaring weakness in argumentation which we had noted in the previous sections: he presents a one-sided and very faulty argument and then, at the last second, casually tosses in powerful counter-evidence, but does nothing with it. He does not face the implications of the counter-evidence (perhaps because he cannot deal with it), but runs away from it, leaving it unhandled, and his discussion is therefore unbalanced, ignorant and inadequate. Notice for instance how sneakily he tries to slide this issue past us, glossing over a matter which is at the very core of today's debates in the history and philosophy of science—the process by which lonely facts (perhaps like Velikovsky's) "are later found to have been correct after all . . . and the accepted theories wrong." How are they "found"? Bauer does not say. Once "found," where are they put? What happens to the old theories? Bauer does not say. How do new theories arise? Bauer does not say. These are huge issues, but Bauer never discusses any of this (perhaps because he does not know how it occurs). He merely states it in total ignorance of its ramifications:

". . . it is common practice to discard or ignore the discordant facts until such time as the 'discordant' ones come to predominate over the concordant: then, only then, are the laws or theories questioned anew."⁸⁰⁶

We, however, cannot let it go by that easily: we have to wonder how the "finding" that the old theories are incorrect can ever happen if science were to proceed only as Bauer dictates, *i.e.*, in what way would "discordant facts . . . come to predominate"; in what way would "lonely facts" lead to new theories, if they were routinely gotten rid of? Bauer does not tell us. He merely passes it off without explanation as occurring at "such time as" the discordant facts "predominate." This is wholly inadequate, for it merely re-states the unexplained in different words. In the discovery of penicillin, for example, it is only because the two scientists involved decided to look for the cause of an anomaly, rather than discarding it because it did not support *any* orthodox theory, that the first wonder drug was found. (Had Bauer been there, presumably, the discovery would not have occurred.)

Bauer's argument seems to be that the ideas of mainstream science are so solid, so long-proved, so "well-established," that science's only conceivable, rational and honorable course was to reject Velikovsky "outright."

"Had Velikovsky's interpretations been in some way consonant with accepted laws or theories, they would not have been rejected outright; but, being contrary to a whole set of ideas that had been satisfactory in the past and continue to be satisfactory . . . Velikovsky's 'facts' could not be taken seriously by scientists."⁸⁰⁷

We will find it interesting, therefore, to look a bit more closely at Bauer's description of this invulnerable knowledge which gave science a complete right to ignore Velikovsky. Here is one picture of it:

⁸⁰⁴Bauer, *op. cit.*, p. 282.

⁸⁰⁵Bauer, *op. cit.*, p. 284.

⁸⁰⁶Bauer, *op. cit.*, p. 259.

⁸⁰⁷*Ibid.*

"In any discipline the mainstream of methodology and theory exists because, on the whole, it seems to work adequately for the bulk of the available material."⁸⁰⁸

That is why, says Bauer, "mainstream . . . methodology and theory" must be believed. We remember, however, (as I pointed out in the second section of this chapter), that Bauer severely attacked imprecise statements where, because of the presence of devious "escape clauses," what is said "is qualified to virtual meaninglessness." It indicated, says Bauer, that the writer was "not so sure of the point he was asserting." When we apply Bauer's warning to his own words once again, we perceive once again that his own attitude toward what he is saying is far less certain than it appears at first sight. Note the prevalence in his own text of what Bauer scornfully called "unquantifiable adjectival judgements": "mainstream," "on the whole," "it seems," "adequately," "bulk" and "available." Look at what remains when they are removed: In any discipline the methodology and theory exist because they work for the material.

That is what Bauer should have said, had he been sure that scientific knowledge was correct. When he tells us in contrast that certainty applies only to "the mainstream" of scientific thought, and only because "it seems to work," not with certainty but only "adequately," and only for "the bulk" of the material, and only for that material which is presently "available," we get a very different picture, do we not? We see that Bauer himself was "not so sure of the point he was asserting." Bauer was therefore right to warn us, wasn't he, that, with evasive statements such as his own, "it behooves us to ponder such words . . . lest we unknowingly imbibe . . . unsupported opinions."⁸⁰⁹

Despite his subcutaneous doubts, however, Bauer is quite certain on the surface that science is wholly right to routinely ignore discordant facts. "Facts alone have little *scientific* value."⁸¹⁰ They are only useful if they correlate with established knowledge.

". . . when we talk about the importance of scientific facts, what we actually regard as important are the correlations . . . "⁸¹¹

There are many discordant facts in science—"Every experimenter has had experience of discordant facts"⁸¹²—but scientists are "reluctant to propose modifications of established theories merely on the basis of some discordant and apparently unexplainable facts."⁸¹³ To Bauer, theory must always come first.

"To make science, it is not enough to connect new "facts" with new "theories."
Somewhere a connection has to be made with what already exists in science."⁸¹⁴

"In science the speculative ideas will be ignored or rejected unless they connect in some acceptable manner with existing knowledge . . . "⁸¹⁵

⁸⁰⁸Bauer, *op. cit.*, p. 92.

⁸⁰⁹Bauer, *op. cit.*, p. 229.

⁸¹⁰Bauer, *op. cit.*, p. 259.

⁸¹¹Bauer, *op. cit.*, p. 258.

⁸¹²*Ibid.*

⁸¹³Bauer, *op. cit.*, pp. 258-259.

⁸¹⁴Bauer, *op. cit.*, p. 260.

⁸¹⁵Bauer, *op. cit.*, p. 261.

If we wonder why he always puts theory above data, especially since it contravenes his own statements (*e.g.*, p. 61), the answer is simple—he *knows* that this is the correct procedure, he tells us, because he *knows* that present scientific theory is correct and will in time explain everything—"eventually"—discordant facts will become concordant as knowledge grows"816 All new data, says Bauer, will be inevitably absorbed into what is already believed. (Isn't such confidence delightful? It is too bad not every scientist shares it, not even Bauer, as his escape clauses have shown.) Velikovsky's ideas, in contrast, "are just 'lonely' facts; they have not been correlated or fitted in, so they have little scientific value,"817 and consequently, says Bauer, all they deserve is to be ignored, for to do differently (to place the fact above the theory), would be unscientific.

"Velikovsky sought to turn the scale of values upside down, from a scientists' viewpoint; he placed greater importance on the outcast facts than on the set of concordant ones."818

No proper scientist, implies Bauer, would ever do that.

To provide some idea of how outdated (and actually antiscientific) Bauer's attitude to discordant data is, here is an extremely different (and in my opinion far more productive) scientific attitude than Bauer's to the anomaly or lonely fact or seemingly-"unconnected" datum. It is found in Stephen Jay Gould's (improper? upside-down?) *Dinosaur in a Haystack* (1995), where Gould discusses the wastefulness of "the underreporting of negative results."819 A study in 1986 of publication bias in the field of psychotherapy, for instance, showed that 82% "of studies with positive outcomes" were submitted to journals and 80% of these were published, while no more than 43% of the negative results were submitted, and only half of these were published, giving a final ratio of close to 3 to 1 in favor of publishing positive outcomes. That seems to be what scientific journals (and scientists) prefer—data which upholds the dogma.

"Publication bias is serious enough," says Gould, "in its promotion of a false impression based on a small and skewed subset,"820 but there is a more serious problem as well.

"What if our conceptual world excludes the possibility of acknowledging a negative result as a phenomenon at all?"821

(Especially negative results like Velikovsky's "lonely facts"). Orthodox referees may be biased, says Gould, but they nevertheless consider all instances before, like "a guard at the party door, giving passage to those with the right stamp on their hands."822 But where *conceptual* exclusion pre-exists, he observes, the lonely or non-correlatable results are never considered at all (as we have found with Velikovsky). The result, says Gould, is that the lonely facts

⁸¹⁶Bauer, *op. cit.*, p. 259.

⁸¹⁷Bauer, *op. cit.*, p. 260.

⁸¹⁸*Ibid.*

⁸¹⁹Stephen Jay Gould, *Dinosaur in a Haystack*, (New York, 1996), p. 124.

⁸²⁰*Ibid.*, p. 126.

⁸²¹*Ibid.*

⁸²²*Ibid.*, p. 127.

". . . are 'unpersonned' in the most Orwellian sense. They are residents in the last gulag in inaccessible Siberia, the farthest outpost of Ultima Thule. They are not conceptualized and therefore do not exist as available explanations."⁸²³

That is just what mainstream science did to Velikovsky's lonely facts, rendering both him and them "unpersonned," and it is a procedure which, as we see, Bauer heartily applauds. Gould, in contrast, proposed the direct opposite, that the theory not be allowed to "unperson" the fact but be itself modified to be able to accept the fact. In order to make the lonely fact useful, says Gould, to promote the alleged "nothing to a meaningful something,"⁸²⁴ what must occur is not (as Bauer wants) the blind discarding of anomalous results or discoveries if they do not correspond with established theory, but the reverse, a "conceptual overhaul" of the theory-pool (*i.e.*, a re-orientation of orthodox "consensus" or dogma), which will permit a change in how science *sees* the lonely fact.

This change, however, can seldom arise within the domain ruled by the established dogma (says Gould, contrary to Bauer), for the reigning orthodoxy in any scientific field (Bauer's "consensus"), has always made the lonely fact a "non-phenomenon."⁸²⁵ To Gould this is a very major fault (not a virtue), and here is what must happen to correct it.

"A different theory must be imported from another context to change conceptual categories Correction of error cannot always arise from new discovery within an accepted conceptual system. Sometimes the theory has to crumble first, and a new framework be adopted, before the crucial facts can be seen at all."⁸²⁶

It is only by means of this sort of upending process, says Gould, founded not upon the suppression of data which does not correlate with old theories (as with Bauer), but upon "the role of new theories in promoting previously ignored phenomena to conceivability and interest,"⁸²⁷ that the lonely facts, the "ignored phenomena," can be turned from "outcasts" into "persons" and at last be enabled to speak. The importation of "a new framework" from outside the orthodoxy will allow them to "be seen" and to become useful. Science must "change conceptual categories," *i.e.*, break free of the stranglehold of the "accepted conceptual system," if it is to be saved from stagnation.

This, then, is Gould's prescription, as opposed to Bauer's—a positive response to negative data must become not merely more desirable among scientists, but routine, if science is to move forward. It is the very heart of scientific progress. Bauer's model, in contrast (like any concentration-camp mentality), is obscene in that it would exile radical new ideas to a gulag policed by orthodox guards (as occurred to Velikovsky), where, as long as the orthodoxy is able to maintain its suppressive power, the new ideas would be silenced in "the farthest outpost" of scientific Siberia, remaining "nothing" rather than ever becoming a potential "something." Is this a valid or useful or honorable principle? How little Bauer understands science.

Third, Bauer omits to add (although he must have known), that the British Museum went much further than merely not publishing discordant data which might support Velikovsky: it denied that the tests were performed, saying therefore that *such data did not exist*. This, I'm afraid, smacks of suppression. The point Bauer seeks to make, therefore, about the alleged intrinsic right of science to ignore Velikovsky categorically, is nonsensical. The behavior of science in general (and the British Museum in particular) re Velikovsky's discordant data cannot be as easily excused as he thinks. This whitewash will not wash.

⁸²³ *Ibid.*

⁸²⁴ *Ibid.*

⁸²⁵ *Ibid.*

⁸²⁶ *Ibid.*

⁸²⁷ *Ibid.*

A third accusation against science which Bauer attempts to dismiss, with equally feeble results, is that science was not wrong when it failed to give Velikovsky due credit for his correct predictions.⁸²⁸ The reader may remember that in Chapter One I described how two eminent scientists, Valentin Bargmann (physics) and Lloyd Motz (astronomy), wrote a letter to the journal *Science* in 1963, asking that proper acknowledgement be made of the priority of several of Velikovsky's successful and revolutionary predictions. (The reader should note that this followed upon written support Velikovsky had received at different times in the years before on the same topic from scientists like Hess and Kallen (see the first chapter), and with reference to an even greater number of successful predictions, but with the same churlish response by science.) Bauer indignantly dismisses the Bargmann-Motz letter as "an extraordinary anomaly in the scientific literature,"⁸²⁹ (as if these two scientists did not know what they were doing), and insists that Velikovsky deserves no recognition for any of his successful predictions because "it is no part of the scientific ethic to give credit for predictions made by nonscientific means or in nonscientific work."⁸³⁰ (One must presume that by "nonscientific" he means Velikovsky). Bargmann and Motz, poor fellows, were therefore entirely wrong. Recognition is to be restricted to scientists, and Velikovsky is not a scientist, and therefore science committed no injustice: it is therefore false to say that science ignored and gave no credit to Velikovsky merely because it hated his ideas. Bauer's argument, however, is demolished by the example of the 1994 Shoemaker-Levy comet, for David Levy, after whom mainstream science *named* the comet, is only a lowly English professor! Once again, therefore, Bauer does not know what he's talking about. His attempt to whitewash science on this point (and to instruct Bargmann and Motz on how they should have behaved), leaves him with egg-white all over his face.

What follows is more of the same whitewash. Was Velikovsky refused publication in "accepted scientific journals"? No problem, for the blame lies solely with Velikovsky. "That was Velikovsky's own doing—his work did not meet the disciplinary norms."⁸³¹ Once again, however, Bauer is destroyed by self-contradiction (or perhaps he has just forgotten his own evidence), for he knows that many of Velikovsky's formal submissions, *often supported directly by letters from established scientists*, were either returned unopened and unread, as he himself cites with indignation in his own book (*e.g.*, 58), or, when they were accepted by some of the orthodox scientific referees, were then met with resistance by the rest. Neither of these cases implies in any way that Velikovsky's submissions were rejected (or not read) because they "did not meet the disciplinary norms." Much more was involved, and it apparently had nothing to do with norms, for in none of the instances where Velikovsky was rejected is there any record of the referees attributing this simply to violation of the norms, as Bauer would have us believe. (How, Henry, could a referee reject an article by Velikovsky for violation of norms if he never read it?) Even in the specific case to which Bauer refers (a single instance from which he has extruded his entire argument), no mention is made of norm violation, but of a dispute between Velikovsky and the referees of *Science* concerning the nature of Venus' atmosphere.⁸³² Bauer's own judgement on this instance equally has nothing to do with norms, for all he can say is that "there is something to be said on both sides,"⁸³³ which in no way implies that norms were violated, and he confirms this with a further quotation, in which he calls Velikovsky's article a submission "where the initial judgement of the referees was that the material might be acceptable,"⁸³⁴ (*i.e.*, that it did *not* violate the norms). As usual, therefore, he has contradicted himself once again, and his own words destroy him. The point is wrong. (By the way, it should be pointed out that nowhere in Bauer's expert argumentation about the "norms" does he ever explicitly define what they are).

As a counter-example, Velikovsky was the subject of several critical talks at a 1952 meeting of the American Philosophical Association, and was allowed to make an extensive defense at the meeting. When the Proceedings were published, however, and all the attacks on Velikovsky were included but his response was not (an incident which Bauer himself notes with outrage), no one ever wrote or said or even implied that Velikovsky's

⁸²⁸Ginenthal, *op. cit.*, pp. 92-93.

⁸²⁹Bauer, *op. cit.*, p. 287.

⁸³⁰Bauer, *op. cit.*, p. 285.

⁸³¹Bauer, *op. cit.*, p. 281.

⁸³²Bauer, *op. cit.*, pp. 234-235.

⁸³³Bauer, *op. cit.*, p. 235.

⁸³⁴Bauer, *op. cit.*, p. 281.

article was rejected simply because of a failure to meet the norms. It was rejected because Velikovsky was a pariah, as every commentator including Bauer deduces, for here is what he himself says: "The American Philosophical Society refused to publish a paper by Velikovsky even though it was recommended by a prominent member of the society, H.H. Hess, who was also president of the American Geological Society."⁸³⁵ Would Hess have supported a paper which failed to meet the norms? Once again, Bauer is demolished by his own evidence. This argument is merely an invention by Bauer to help with his whitewash.

One of the highlights of Bauer's ineptitude (or cleverness) in trying to whitewash science of its taints of extreme prejudice in the Velikovsky Affair is his attempt to argue that, even if some scientists here and there did behave a bit badly now and then, it is only because they were driven to it by adverse publicity, which means *it is entirely not their fault*. True, says Bauer, science did go a bit too far in how it rejected Velikovsky, for normally the "sarcastic and degrading comments" of referees are restricted to the referees, the editor, and the individual involved. "He is not held up to public ridicule and condemnation as Velikovsky was."⁸³⁶ Does this mean that Bauer is going to admit even this once that science was wrong? "Are Velikovsky's critics, then, not to be judged for publicly defaming him?"⁸³⁷ No, sir, they are not. Why? Because "the public nature of the Affair was Velikovsky's own doing."⁸³⁸

We must take time here to grasp the enormity of the ridiculousness (and therefore sly cleverness) of this ingenious reversal in favor of science. Here is how Bauer sets it up: the Affair is not the scientists' fault because they were not the ones responsible for bringing "the matter into the glare of wide publicity."⁸³⁹ Once it had been done by Velikovsky, however, the scientists could not fail to respond or it would look like they had been defeated.

"But, when the specialists do comment publicly, they are told that they should talk in tones of respect about what their judgement leads them to regard as ridiculous. This certainly amounts to being damned if you do and damned if you don't."⁸⁴⁰

No, Henry, I'm afraid it doesn't. This is a ridiculous argument, for it seems never to have occurred to you that one can talk respectfully even of ideas one considers wrong. That is the whole point of the Affair (which you seem to have missed), that, had science responded respectfully *and scientifically*, it would not have been "damned." Because Bauer fails to see this, he applies his own nonsensical principle to the Velikovsky Affair as if it were a natural law and gives us this perception of it: the scientists did respond (following Bauer's macho rules), but quite naturally it was not "in tones of respect" because they felt Velikovsky was ridiculous, and it is for that understandable lapse alone, says Bauer, that they were condemned, a lapse for which they are not really responsible. Bauer's conclusion is therefore nothing less than this: the Velikovsky Affair *is not the fault of science*. The devil (oops, the publicity), made them do it.

This argument is so ridiculous, evasive and self-serving that one hardly knows where to begin to reply. We must start, however, by setting the record straight. When science was guilty of such despicable things as blackmail of a publisher and destruction of careers and a campaign of *ad hominem* vilification and blockage of Velikovsky's access to response, none of which is scientific and each of which Bauer explicitly admits (and condemns) in his book, and when these acts came to public attention, this was not in any way "Velikovsky's doing." Velikovsky behaved like a gentleman throughout, as Bauer acknowledges,⁸⁴¹ and never descended to the depths of acts like these. Who then brought "the matter into the glare of wide publicity"? Bauer knows the answer full well, although he acts as if he did not. The scandalous excesses of science were brought into the light by scientists like Hess and

⁸³⁵Bauer, *op. cit.*, p. 58.

⁸³⁶Bauer, *op. cit.*, p. 280.

⁸³⁷*Ibid.*

⁸³⁸*Ibid.*

⁸³⁹*Ibid.*

⁸⁴⁰Bauer, *op. cit.*, pp. 280-281.

⁸⁴¹Bauer, *op. cit.*, p. 35.

Kallen, by science reporters like O'Neill and Larrabee, by journalists like Fadiman, Winchell and Sokolsky and by publications like *The New York Daily News* or *The New York Herald Tribune* or *The Compass* or *Newsweek* or *The New York Times* or the *Harvard Crimson* (see the first chapter), each of which was similarly and independently outraged at the wholesale violation of norms of respect and civil discourse by large numbers of scientists. They learned of the violations not at all because of any "doing" by Velikovsky, but because the news could not be suppressed. Bauer knows all of this, for it is on record. Why then does he pretend that it is "Velikovsky's doing"? We shall perceive his purpose in a moment.

Second, Bauer is extremely wrongheaded in his depiction of the *sequence* in which the Affair came about. As he tries to present it, "the matter" was brought to public attention by non-scientists, *after which* the scientists faced a dilemma.

"They had a choice: to ignore; to answer privately . . . which would have had the same effect as ignoring; or to answer publicly."⁸⁴²

As a result, says Bauer, they had to answer publicly, forced to it by "the public." We shall find, however, that this version of the Affair is a self-serving myth, for Bauer has it all *backward*. The "matter" which was brought to public attention was the inexcusable behavior of science to Velikovsky, and this occurred *before, not after* it was brought to public attention. These scurrilous and degrading acts by science were not carried out *after* the public refused to listen to science, as Bauer would have us believe, but before the public ever became involved, *i.e.*, before *Worlds in Collision* was published and in the immediate aftermath. To be precise, the scorn, defamation and misrepresentation which I have catalogued in the first chapter constituted almost the entirety of science's "public answer," as Bauer ought to know.

As it turns out, therefore, it was only this barrage of vilification and sarcasm by science (not any "authoritative judgements"), that was available to the public in order for it to decide if it would agree with science or not. Bauer himself proves this for us when he states "that Velikovsky's critics . . . failed to make a case that would convince disengaged . . . observers who were not scientists."⁸⁴³ That is being too polite. He gets closer to the truth when he adds "In fact, the critics' efforts might well have caused people to side with Velikovsky: 'Chesterton was converted to Christianity by reading the denunciations of the Nineteenth century atheists The writings of some of Velikovsky's opponents must have had a similar effect on many fair-minded people.'⁸⁴⁴ What Bauer is referring to is the outrageous (and totally unscientific) misbehavior of science, which he calls not merely a "gross ineptness on the part of Velikovsky's critics,"⁸⁴⁵ but a violation of standards of evaluation (*i.e.*, Bauer himself *knows* that science ought to have reacted with more discipline, but that "Scientists . . . sought to demolish Velikovsky's case and, by blundering in the attempt, in the end accomplished far less."⁸⁴⁶

That is how science responded, unfairly and unscientifically, as Bauer himself testifies. When, quite legitimately, the public reacted adversely to the news of science's blackmail and defamation (their "answer"), which was brought to public attention by scientists and science reporters and journalists and journals and newspapers (not by Velikovsky), this occurred because the scientists had brought shame upon themselves beforehand, of their own accord, without in any way being forced into it by anyone else's "doing," especially Velikovsky's. They had acted first, and there was nothing "authoritative" about it, and the public response came afterwards because, in Bauer's own words, the acts of science angered "fair-minded people." That is how it happened, and Velikovsky himself was not involved.

Bauer is therefore completely wrong here. He either hasn't done his research (and is therefore ignorant of what happened), or he does not understand what he has read or *what he has himself written*, or he misrepresents the

⁸⁴²Bauer, *op. cit.*, p. 280.

⁸⁴³Bauer, *op. cit.*, p. 210.

⁸⁴⁴*Ibid.*

⁸⁴⁵Bauer, *op. cit.*, p. 175.

⁸⁴⁶Bauer, *op. cit.*, p. 310.

truth. One would have thought that at some point Bauer would emerge from his self-manufactured cloud and deal with the reality that science did not respond politely and professionally to Velikovsky, and that the public never had the opportunity to disagree with sober calculations and evidence because none was offered, but he does not. He has it all disarranged, false and out-of-order, and it is astonishing that he could believe it himself, or expect anyone else to believe it who had read the truth about the Affair.

Third, and most inane, is Bauer's underlying argument that it is the publicity about Velikovsky, and not the acts of science, which caused the Velikovsky Affair. It is as if, in Bauer's eyes, the misbehavior of science is hardly wrong in itself (merely venial), and all the blame for how they acted and the public reacted is to be laid upon the publicity. This astonishing inversion of responsibility is exemplified in Bauer's explanation of why so much vitriol was directed at *Worlds in Collision* when it appeared: "Well, perhaps simply because it is wrong but was launched with so much laudatory publicity; that might well serve to infuriate a number of people."⁸⁴⁷

This approach is so ludicrously reversed, so utterly preposterous, that one is hard put to find words to adequately describe it. Perhaps an analogy will serve here: it is as if Bauer were saying that the Watergate Affair would never have occurred if only the night watchman at the building had not discovered the break-in and "brought it to public attention." Looked at in this Bauerian way, the entire Watergate Affair would therefore be *the fault of the night watchman*. It would be his "doing," and no blame (or only a little) would be attached to the Nixon administration for such trivial (merely "disrespectful") excesses like spying on its rivals or hiding under the illegal protection of the White House or abusing the trust of the nation or attempting a miscarriage of justice. It merely acted as it did because it was "infuriated." In the Bauerian myth, there would have been no cover-up, and tapes would not have been erased, and a president would not have been forced to resign, if only the night watchman had not brought the illegal entry to the public's attention. Whatever happened after that is entirely the night watchman's fault. That is how Bauer sees the Affair. Astonishing, is it not?

On the next point, Bauer's folly becomes not merely execrable, but laughable. In his urge to establish that the "authoritative judgements" of science (which is what he calls the jeers and catcalls), should never have been questioned by "the public," he quotes this as an example of the sort of thing done by "the public" which exasperated the poor scientists:

"Many experts will have sympathized with the astronomer who thought that the 'rare near-unanimity among scientists should have completely disposed of the question.'"⁸⁴⁸

Sounds convincing, does it not, until we ask who might the unnamed "astronomer" be with whom most of the "experts" should sympathize. When we check the references, why, it turns out to be Donald Menzel. The same Menzel, we ask, whom Bauer himself had scorned for saying that there were "thousands of other erroneous suppositions and conclusions" in Velikovsky beyond the ones science had cited? I'm afraid so. (Even Bauer himself had sarcastically commented "What an effort must have gone into counting all of them."⁸⁴⁹) The same Menzel, we ask, whom Bauer had ridiculed for comparing Velikovsky to "Dr. Brinkley of the goat-gland era"? Whom Bauer had criticized for an extreme "lack of care and rigor" in his attacks on Velikovsky? Whom Bauer had castigated for failing "to correct some erroneous calculations that he wished to use against Velikovsky"?⁸⁵⁰ I'm afraid so. The same Menzel who Bauer tells us actually asked "a fellow scientist not to publish something simply because it provided support for an idea that stood in opposition to Menzel's own"?⁸⁵¹ I'm afraid so. (Bauer conveniently neglects to mention that the "opposing idea" was Velikovsky's, which gives us an indication of how objective and honest and worthwhile Menzel's opinion about Velikovsky is.) In this instance, Bauer plainly accuses Menzel of trying to suppress counter-evidence (a most unscientific act), yet Bauer is nevertheless quite prepared to overlook all of this when it suits his purpose to invoke Menzel *as a reliable scientific witness*. Worst of all, he seems to be

⁸⁴⁷Bauer, *op. cit.*, p. 170.

⁸⁴⁸Bauer, *op. cit.*, p. 310.

⁸⁴⁹Bauer, *op. cit.*, p. 242.

⁸⁵⁰Bauer, *op. cit.*, p. 225.

⁸⁵¹Bauer, *op. cit.*, p. 246.

unaware that (as I have shown in the first chapter and will illustrate more extensively in my last), Menzel is part of a *group* reaction against Velikovsky led by Menzel's *boss* Harlow Shapley, and therefore that everything he might say about Velikovsky is suspect. Are these bilious outbursts by Menzel "authoritative"? Is this sort of biased, hysterical, vituperative person qualified to speak of "near-unanimity" in any sense apart from herd behavior? What a witness Bauer has chosen!

Perhaps the pinnacle of Bauer's pompous self-destruction in his analysis of the Velikovsky Affair comes in the passage where he describes how science ought to behave when it becomes involved in a controversy with "the public," (as if this is what actually happened).

"In such public controversies, 'the most science can do is inject some intellectual discipline . . . into the debate.'"⁸⁵²

This alone, he says, would make the public, as well as uninvolved scientists, pay attention. "Scientists inevitably listen with greatest respect to those who have a record of past accomplishment."⁸⁵³ Which people does Bauer single out as those to whom everyone should have listened "with respect" in 1950? Not Velikovsky, for he "had no qualifications at all in astronomy or physics,"⁸⁵⁴ and scientists therefore had a right to pay no attention to him.

"It would therefore have been irresponsible to lend more credence to his statements on these matters than to the statements of Shapley, Payne-Gaposchkin, Struve, Herget, and the other astronomers."⁸⁵⁵

These, we guess, must be the people who injected "discipline" into the Velikovsky debate. Bauer tells us that he spent several years reading and thinking about the Velikovsky Affair, so he must have pondered deeply and consulted his notes carefully before he chose these specific critics of Velikovsky as his exemplars of scientific method, substance and objectivity. Let us therefore take a close look at Bauer's choices, and particularly at what these accomplished scientists "injected." (Note: in the following extracts, in order to enable the reader to verify the evidence easily, I will quote exclusively from material presented in the first chapter of this book, without page reference).

1. Shapley

Harlow Shapley, Director of the Harvard Observatory, called the publication of *Worlds in Collision* by Macmillan a "venture into the Black Arts," for the book was "arrant nonsense" and "intellectually fraudulent," a "successful fraud" and "pure rubbish" like "astrological hocus-pocus." That is as far as his precise scientific refutation of Velikovsky went. When he met Velikovsky, Shapley wrote later that he "looked around to see if he had a keeper," and even as late as 1963 he called *Newsweek* to block publication of an article favorable to Velikovsky (a perfect example of scientific objectivity). All the while, he disclaimed any role in the Velikovsky Affair: "The idea is absolutely false." "The claim that Dr. Velikovsky's book is being suppressed is nothing but a publicity promotion stunt." Is this tissue of insults, blackmail and lies the "injection" of "intellectual discipline"? Sociologist Michael Mulkey plainly accused Shapley of "abrogating the rules of universalism in science," while the editor of the magazine *The Compass* called Shapley's behavior "totally unscientific and viciously emotional" and was "alarmed at the intensity and character of the attack" on Velikovsky, which he considered "morally and criminally slanderous

⁸⁵²Bauer, *op. cit.*, p. 310.

⁸⁵³Bauer, *op. cit.*, p. 298.

⁸⁵⁴Bauer, *op. cit.*, p. 299.

⁸⁵⁵*Ibid.*

and libelous." Are these the sort of authoritative "statements on these matters" which Bauer prefers? Quite a choice you made, Henry. (Note that there is surprisingly very little on Shapley in a book allegedly written about the Velikovsky Affair. For a much more thorough treatment of Shapley's role, consult the first chapter).

2. Payne-Gaposchkin

Shapley's disciple, Cecilia Payne-Gaposchkin, called *Worlds in Collision* mind-boggling, a book of "rubbish" which deserved only "derision" for its "sloppy parade of jargon." At a 1952 conference, this exemplary scientist "misrepresented" and "misquoted" Velikovsky (as Bauer himself records), and ended with this memorable scientific observation:

"His supporters imagine that we are shaking in our shoes. This is partly true: we *are* shaking, but with laughter."

Is this the sort of highly-qualified "authoritative judgement" to which Bauer insists we "lend . . . credence" above the unqualified babblings of Velikovsky? The editor of *The Compass* knew better. He openly charged in a letter to Shapley that *all* the scientists who suddenly chose to attack Velikovsky in almost the same terms in 1950 "had not reached their conclusions completely independently of discussions with you" and that "you and Mrs. Gaposchkin made extensive and successful efforts to suppress the book." Bauer himself states "that Payne-Gaposchkin had deliberately misquoted Velikovsky in order to discredit him,"⁸⁵⁶ yet he feels that to listen to Velikovsky over someone as admirable and open-minded as Payne-Gaposchkin would be "irresponsible." (A second good choice, Henry).

3. Struve

Otto Struve, mainstream astronomer, associate of Shapley (and, as we shall see, hit-man for Shapley), President of the American Astronomical Society, wrote to Gordon Atwater, Curator of the Hayden Planetarium, and, in a memorable display of scientific fairness and open-mindedness, wanted to know before *Worlds in Collision* was published where Atwater stood re Velikovsky. Atwater then published a favorable review (or preview) in the magazine *This Week*, and within days "a colleague of Atwater's walked into his office and spat in his face," (a further example of science's "authoritative judgement" on Velikovsky). "Atwater was fired on the spot and told to clear out his office in minutes . . . and the projected planetarium show on Velikovsky was immediately cancelled." (How's that for "intellectual discipline"?)

As if this were not enough to establish his intellectual honesty, Struve performed an encore. Just before *Worlds in Collision* appeared, he wrote to John J. O'Neill, science editor of the New York *Herald Tribune*, asking him in plain terms, in advance, to write a negative review. (A peerless example of scientific objectivity). When O'Neill refused, he was not assigned to review the book, even though it would normally be his task. Who was? Why, that objective scientist Struve, who, in a laudatory exploit of scientific responsibility, called the book "mystical" and "ignorant" and compared it to "astrology and flying saucers." This is the third of Bauer's exemplary critics of Velikovsky to whom the public should have listened.

4. Herget

Paul Herget, colleague of Shapley, injected "intellectual discipline" by calling Velikovsky "a fraud" for pretending that his book was "scientific," apparently not having noticed that the book was intended for a general

⁸⁵⁶Bauer, *op. cit.*, p. 58.

audience. He then evaluated Velikovsky's work in this calm, objective, scientific way: if Velikovsky insists on presenting his theory of comets, said Herget, then he "might equally well insist that the State of Washington somehow rose up and threw a silver dollar across the Rappahanock River." (The acuity of this criticism is simply dazzling). To further illustrate how good a scientist he was, Herget openly admitted playing a role in the blackmail of Macmillan—"I am one of those who participated in this campaign"—and he even contested Shapley's importance in it. "I do not believe he was in any sense the leader . . . I was a very vigorous participant myself." (Good choice again, Henry. You did your research well. Such a person obviously deserves our most rapt attention).

5. The "Other Astronomers"

As an example of these, let us take Donald Menzel, who qualifies because he worked under Shapley and was a friend of Struve, Payne-Gaposchkin and Herget and is quoted as an authority by Bauer. When science reporter Eric Larrabee wrote a favorable article about Velikovsky in 1963, Menzel retorted (in a perfectly scientific manner) that "Velikovsky has been as completely discredited as was Dr. Brinkley of the goat-gland era." He called Velikovsky's methods "a return to the dark ages" and said "They no more represent science than the practice of voodoo represents medicine." Any scientist could of course easily verify these precise, quantified, "authoritative" statements.

The same Menzel at that time published a calculation purporting to prove that Velikovsky's planetary theory was impossible. When Larrabee pointed out that Menzel's "proof" had been invalidated by an Australian physicist, what did Menzel the model scientist, idol of Bauer and inheritor of Shapley's post at the Harvard Observatory, do? Did he recant in favor of the truth, as all good scientists do according to Bauer? I'm afraid not. He did completely the opposite: he wrote to the Australian ("intemperately," as Bauer himself puts it), asking him to withdraw his refutations for the very good scientific reason that, as Bauer himself perceives, they were "giving aid and comfort to Velikovsky's supporters."⁸⁵⁷ What a shining example of scientific fair-mindedness!

Remember, these are Bauer's own choices. No one forced him to pick these laughable individuals, nor was he constrained to list them as "authoritative." When they blow up in his face as they do, it is an indication of the quality of Bauer's research (not to mention his thinking). Moreover, Bauer fails to give adequate weight to (or even to notice) the coincidence that all of his choices are not independent but are connected, in that they are part of a closely-linked professional group (see my last chapter), and that this, and not any individual cause, could be why most of "the other astronomers" responded to Velikovsky in "near unanimity," (*i.e.*, in the same way that Shapley and Struve did). Bauer misses this entirely, yet it is to "authoritative" statements such as these that Bauer insists we "lend . . . credence" or we are being "irresponsible." More than that, it is because "the public" did not lend its credence to "judgements" such as these, says Bauer, that the Velikovsky Affair occurred. (Very well done, Henry! You have grasped the essence of the Velikovsky Affair to a tee.)

Having set up this approach, Bauer then tries to excuse not only science's emotional (*i.e.*, unscientific) responses to Velikovsky, but even its alleged intellectual misbehavior. For instance, was science wrong to categorically reject all of Velikovsky's theories after hardly looking at them?

". . . there was nothing unfair, no injustice, in the complete dismissal of Velikovsky's *ideas* by scientists, even after only quite cursory acquaintance with them."⁸⁵⁸

Why? Simply because, says Bauer, they were so self-evidently wrong that *any* average scientist could easily perceive this, even after "only quite cursory acquaintance." If that is true, why did Bauer feel it necessary to publish a book on Velikovsky *34 years after* the first phase of the Affair, a book in which he tries to outline for the first time (so he tells us) why Velikovsky was wrong, if all of this was evident in 1950? This hardly seems to accord with his own complaint that he wrote his book

⁸⁵⁷Bauer, *op. cit.*, p. 59.

⁸⁵⁸Bauer, *op. cit.*, p. 279.

". . . because I could not find, 25 years after the publication of *Worlds in Collision*, a satisfactory discussion of the merits of Velikovsky's work."⁸⁵⁹

Which is it, Henry? You can't have it both ways, you know. The answer, I think, is this: Bauer must pretend that Velikovsky's errors were obvious even at the beginning, in order that the response of science to Velikovsky at that time (which almost everyone else has admitted was brutal and personal), could be deemed by Bauer to be fair and just. Later, when it comes to his own book, he can pretend the reverse, that no one successfully debated Velikovsky before him. It is all quite convenient. Only if all three of these interrelated sneaky over-generalizations were true together could Bauer hope to pass off the shameful acts of Shapley and Friends as just and fair. When we perceive, however, that none of them is true, we must deduce that, on this point too, Bauer is wrong.

In general, therefore, when we look back over what Bauer has presented as his evaluation of the Velikovsky Affair, we are forced to the induction that his attempt to whitewash science of every accusation of unfairness that was raised against it has been a complete failure, as we have seen. On his own terms, he is wrong every time. The reader must be reminded, however, that these glaring weaknesses in Bauer's analysis of the Velikovsky Affair derive only from his way of looking at it. As I promised, it is now time to look at the Affair as it actually occurred, not as Bauer mistakenly thinks (or says) it did, within which framework his ineptitude will appear even more palpable.

We will start with a specimen passage that presents quite usefully Bauer's seriously-flawed understanding of what the Velikovsky Affair was.

"One finds consistently that the accusations of unfairness and injustice were made as a result of disagreements with the *judgement* reached by scientists on the substantive merits of the case."⁸⁶⁰

This is what Bauer thinks the Velikovsky Affair was, and here precisely is the core of his total failure to understand it. I shall argue that this is not what the Affair was at all, that the charges of unfairness raised against science with regard to its response to Velikovsky have nothing to do with scientific *judgement* but scientific *misbehavior*, that they do not involve "substantive merits" but subjective and uncontrollable passions (as anyone who has read the account in the first chapter can easily see), and therefore that Bauer's analysis of the Velikovsky Affair ultimately *has nothing to do with the Affair*. This is his greatest error on this topic (among the many glaring faults of his we have listed and will continue to catalogue), that he has totally missed the mark, and it renders his analysis useless, for what he says talks of something which never happened, while what really happened totally eludes him. It is a misunderstanding of enormous magnitude which can be stated plainly: Bauer *does not understand what an "Affair" is*. As a result, he writes an entire book on the topic of the Velikovsky Affair with no idea of what the word means. It is hard to believe that he could be guilty of such appalling ignorance, especially when he presents himself with an air of great learnedness, but unfortunately that seems to be the case.

Remember, Bauer insists that the Velikovsky Affair is a disagreement on substance between "science" and "the public." We have already seen that this opposition ("science" versus "the public") is an over-exaggeration invented by Bauer, and that, while it may be usefully misleading for Bauer's purposes, it is historically inaccurate. It is not how the Velikovsky Affair occurred. Bauer's misrepresentation, however, is irrelevant at the moment, for what we must notice now is that Bauer's description of the Affair, whether it is right or wrong, *does not describe an "Affair."* As a glance at any good dictionary will show, an "Affair" can have many meanings, but none of them have anything to do with a simple disagreement about ideas. There are affairs of state and affairs of honor, love affairs and business affairs and private affairs, and then there are events like the Watergate Affair or the Dreyfus Affair, incidents besmirched by scandal and misbehavior. *That*, Henry, is what an "Affair" is, something disreputable, shameful and tainted, something reproachful, objectionable and degrading. It is "a matter occasioning public . . .

⁸⁵⁹Bauer, *op. cit.*, p. 180.

⁸⁶⁰Bauer, *op. cit.*, p. 281.

scandal," (*Webster's New Collegiate Dictionary*), "a notorious, or scandalous event," (*Random House College Dictionary of the English Language*), or "A matter causing public scandal," (*American Heritage College Dictionary*). One can have a *debate* about fluoride, or a *debate* about Star Wars, or a *debate* about the death sentence, and a debate can even escalate into a raging controversy, but, unless one of the sides in the debate engages in outright illegalities, in skulduggery or malfeasance, as science did concerning Velikovsky, it is not an "Affair." Poor Bauer, however, seems to have completely failed to understand this, and has consequently written a book which (aside from being often wrong on specific points), is pointless as a whole. It is simply not about the Velikovsky Affair.

To describe the Velikovsky Affair more accurately than Bauer does, I have assembled a group of descriptive terms, culled from writings on the Affair, which together will be seen to paint a picture having little to do with Bauer's fantasy of a disagreement about ideas. There are so many of these phrases that, for the sake of economy, they will be presented in parallel columns:

"virulence"	"futile attempts"	"intemperate tone"
"violent"	"made fools of themselves"	"outbursts of anger and frustration"
"emotional"	"exhibiting jealousy"	"monolithic opposition"
"unprincipled conduct"	"sloppy mistakes"	"dogmatic grounds"
"derision and sarcasm"	"errors of omission"	"slippery and sophistical"
"detractors . . . acted unethically"	"merely speculated"	"misrepresented"
"dogmatically and in authoritarian fashion"	"improprieties"	"an insidious misleading fashion"
"not to be excused"	"sweeping, simplistic generalizations"	"sweeping generalizations"
"inexcusable"	"penchant for simplicism"	"gross insult"
"unsupported"	"conspiracy and skulduggery"	"hoots and jeers"
"quite unfair"	"merely asserting an analogy"	"personal attacks"
"unwarranted dogmatism"	"blunder upon blunder"	"jumped . . . to conclusion"
"outrage"	"impatient and strident"	"the numbers racket"
"intellectually dishonest arguing"	"ludicrous hyperbole"	"outright lies"
"denounce . . . before its publication"	"strong emotion"	"misreported"
"passionate denunciations"	"harsh denunciation"	"discrediting"
		"precipitate actions"

Each of these terms has been used to describe the behavior of science in the Velikovsky Affair. Can the reader guess where *every single one of them* comes from? Why, from Bauer himself, and every one of them is applied by him to the actions of Velikovsky's critics—(*i.e.*, once again, in order to demolish Bauer, we have only to

turn to Bauer himself, and he, as usual, has provided the rope with which he will be hanged. Can these words in any way be construed to evoke a mere "disagreement . . . on . . . substantive merits"? What on earth can Bauer be talking about? Certainly not the Velikovsky Affair which *he has just described*, to which we will now add more of Bauer's own comments on the behavior of science towards Velikovsky, to show how enormously he has failed to understand even his own evidence (not to mention all the material in *Pensée*, *KRONOS* and *SISR*, plus two full-length books on the topic, *The Velikovsky Affair* and Velikovsky's own *Stargazers* and *Gravediggers*, which were available to him when he wrote). What Bauer is surprised at is how "scientists could have reacted so violently."⁸⁶¹ He is appalled by "the violence of the astronomers' reaction."⁸⁶²

"His detractors—even if only a small group of them—had acted unethically College professors and scientists were castigated for themselves doing what they traditionally fought against: suppressing ideas, assaulting academic freedom, acting dogmatically and in authoritarian fashion."⁸⁶³

"They 'argued' dogmatically, *ex cathedra*, by analogy, *ad hominem*, with ridicule, in most every other way than by calm, comprehensive discussion of the relevant points."⁸⁶⁴

". . . a wide variety of verbal weaponry was used: innuendo, implication, misleading statement, misquotation or misinterpretation or misrepresentation of the opponent; argument by analogy, *non sequitur*, *ex cathedra*, and *ad hominem*; wishful thinking; sarcasm and ridicule . . . shades of meaning, statements taken out of context"⁸⁶⁵

It is a very damning list, is it not, *and it all comes from Bauer*. To sum up, Bauer decides that

"Velikovsky's critics were not only ineffective; many of them also behaved offensively. It cannot be gainsaid that literally inexcusable steps were taken"⁸⁶⁶

"Many of Velikovsky's critics argued tendentiously and untruthfully . . . acts of intellectual dishonesty."⁸⁶⁷

". . . the manner in which many scientists attacked Velikovsky is not to be excused."⁸⁶⁸

These quotes, taken directly from Bauer himself, paint a very definite, not-easy-to-misinterpret picture of vast and insidious misbehavior on the part of science toward Velikovsky. It is "a resounding condemnation of Velikovsky's

⁸⁶¹Bauer, *op. cit.*, p. xi.

⁸⁶²Bauer, *op. cit.*, p. 57.

⁸⁶³Bauer, *op. cit.*, p. 26.

⁸⁶⁴Bauer, *op. cit.*, p. 201.

⁸⁶⁵Bauer, *op. cit.*, p. 227.

⁸⁶⁶Bauer, *op. cit.*, p. 180.

⁸⁶⁷Bauer, *op. cit.*, p. 181.

⁸⁶⁸Bauer, *op. cit.*, p. 205.

critics."⁸⁶⁹ How then could Bauer have been so inept as to confuse *his own evidence* for a mere disagreement about the substance of certain ideas? Does he not understand what he himself has written?

When I first read Bauer's explanation of the Velikovsky Affair, and noticed his weak, unsupported and finally nonsensical attempt to shift almost all of the blame for it to Velikovsky himself and to that facile invention "the public," questions as to Bauer's motive crossed my mind. We have seen that he has very badly misunderstood and misrepresented the response by "science" to Velikovsky's ideas. The question is whether this unbalanced view was an honest mistake, or was it part of an attempt to exculpate and to whitewash science?

To decide, we need additional data, and it is supplied for us as soon as we realize that Bauer is guilty of two more enormous misconceptions concerning the Affair, first that science was completely right in its condemnation of Velikovsky, and second, that it was the positive public response to Velikovsky despite science's "authoritative judgement" which made scientists occasionally misbehave. When we see that he is crediting science with everything positive about the Affair and blaming everything discreditable on "the public," (*i.e.*, on those who are not scientists), our suspicion that Bauer is out to whitewash science increases.

I would guess that Bauer may have an additional motive here, which would be a desire on his part, on behalf of rigid, orthodox, mainstream science, to stake out its territory and to defend the line over which many supposedly pseudo-scientists like Velikovsky (and non-scientists like "the public") had dared to cross. Science is our business, says Bauer, not yours, and judgements within science are our prerogative, no one else's. This motive too, I'm afraid, would play a role in a whitewash, for it implies that, had "the public" not interfered where it did not belong, the Velikovsky Affair would not have taken place. As the first chapter has demonstrated, however, this is not what happened. The public became aroused *after* the misdeeds of science were called to its attention, but Bauer presents the opposite picture because it is only as a result of a distortion like this in favor of science that he is able to argue that the misdemeanors of science were minor (if deplorable) and were caused mostly by the impertinence of "the public." This certainly smells more and more like whitewash.

The third interrelated error to be found in Bauer's portrait of the Affair is his startling undervaluation of the excessive, subjective, emotional, biased and often almost-hysterical behavior of science towards Velikovsky. (I have merely skimmed the highlights of this sordid phenomenon in the first chapter, and look at how substantial it is, yet, surprisingly, these types of acts are given curiously little emphasis in Bauer's analysis). If we put these three misconceptions together—that science was entirely right, that its overreactions were minor and that the public response made scientists angry—it becomes more difficult to avoid the thought that a whitewash is taking place. We begin to see that, by underemphasizing what everyone else has emphasized (the scandalous acts of science), and by over-emphasizing what everyone else treated in a less-emphatic fashion (the alleged role of "the public" in causing science to misbehave), Bauer is trying to put his own "spin" on the Affair, a spin which, contrary to what most commentators on the Affair (scientist and non-scientist) have said, lets science off the hook very gently, with only a minor slap on the wrist, while "the public," along with Velikovsky and his supporters, are left to shoulder most of the blame. One must admit it is a rather dexterous (and crafty) performance. Does it imply a whitewash?

That is the question we shall now try to answer. We must remember that, by the early 1980's when Bauer's book was published, most analysts of the Velikovsky Affair had conceded that many censurable and unbecoming acts had been committed by science. Even staunch anti-Velikovskians like Asimov, Sagan, Storer and Mulholland (see the chapter on the AAAS Symposium), not to mention more objective observers like the sociologists of science Barnes, Dolby, Mulkay and Hagstrom (see my final chapter), had acknowledged the arrant misbehavior of science. It had by then become a sad given, to be avoided in the future. We may therefore describe the history of the story of the Velikovsky Affair as undergoing several transformations. First came the Original Version of 1950, in which the accusations of Larrabee, O'Neill and Hess were denied by Shapley and Co., and then the Revised Standard Version, c. 1965 and onwards, in which many of the transgressions of science were admitted, even by members of the mainstream.

Now comes the Bauer Version, in which many of these concessions are retracted. He turns the tables around and implies that science only misbehaved a little concerning Velikovsky, and that that little was not even the fault of science, which means that science is more or less *not to blame at all*. According to the Bauer Version, the poor little innocent scientists of 1950, interested only in pure truth, were goaded into unscientific misbehavior by the big bad arrogant public which would not listen to science when it dismissed Velikovsky. Everything bad which the

⁸⁶⁹Bauer, *op. cit.*, p. 180.

scientists did is therefore attributable to "the public," and everything else they did (refusal to publish, suppression of data, even derision and misrepresentation), is good or at least excusable. In the end, therefore, according to the Bauer Inversion, science actually has very little to be sorry for.

To achieve this inversion requires two enormous displacements by Bauer, and that is where the question of motive intrudes. First, Bauer has to grossly over-value the role of the unkind "public" in provoking the kindly scientists into committing their regrettable faults, by acting as the prod which drove mainstream science berserk. This is a facile and ignorant oversimplification, as I have shown, but it could be an honest error on Bauer's part. (Perhaps he simply did not do his homework, or he badly misinterpreted the data). It may not be a deliberate error. The second of Bauer's huge displacements, however, cannot be as easily absolved of deceit, for, at the same time that he overvalues the effect of "the public" on the patience of science, he extremely undervalues all the disgraceful and un-scientific things which the scientists did to Velikovsky—it wasn't that bad, only a few scientists participated, they didn't do so much, it was overblown by the public press and it was right, anyway.

To give the reader some idea of how completely Bauer has inverted what the Affair was (and denied the evidence from his own book), here is how he tries to fob off alternative descriptions of the Affair which disagree with his:

"In 1976 a review of *Velikovsky Reconsidered* in *New Society* stated that 'for almost thirty years now, Velikovsky has been persecuted with a McCarthy-like intensity' But could a reader of that periodical realize that the 'persecution' consisted in the main of not accepting the views of someone who failed to present them in the accepted scholarly manner"⁸⁷⁰

What on earth is Bauer trying to get us to believe here? That the behavior of science toward Velikovsky, which he himself has described as consisting mostly of "innuendo . . . misleading statements . . . misrepresentation . . . sarcasm and ridicule," did not occur? That his own picture of Velikovsky's persecution is false or exaggerated and, "in the main," the Affair was merely science calmly and respectfully "not accepting" Velikovsky's views, which his own book denies? Or that Velikovsky's views were not accepted simply because he did not "present them in the accepted scholarly manner" (*i.e.*, that had he altered the *format*, the substance of his ideas *would* have been accepted)? Or that this bland "not accepting" continued in the same polite manner "for almost thirty years," during which time scientists from Shapley to Sagan would have been happy to accept Velikovsky's views as soon as he complied with the "scholarly manner"? This is nonsense. We have to wonder why the people at *New Society* missed all this and actually thought that Velikovsky had been persecuted. The editors of *New Society* had lots of evidence at their disposal by 1976, but Bauer accuses them of incompetence and attempts to re-define their noun "persecution" as if they had misunderstood the matter and he must correct them. (How good it is of Bauer to set them straight.)

The same desire by Bauer to invert the history of the Affair is to be found in his attempt to rebut a second accusation of scientific blameworthiness.

"According to the *Philadelphia Magazine* in 1968, 'Velikovsky was . . . relegated to a silent limbo in which he became a non-person.'⁸⁷¹

Bauer tries to get round this by arguing that the charge is not true because it could not happen.

"[It] sounds like an academic sent into the wilderness, perhaps through losing an academic post. But Velikovsky had never held an academic post and had never been anything but a nonperson in the disciplines of the sciences and history."⁸⁷²

⁸⁷⁰Bauer, *op. cit.*, p. 234.

⁸⁷¹*Ibid.*

No, Henry, I'm afraid it doesn't sound like that at all. The evasion simply won't do. I do not see how the notion of losing a post can be inferred from the quoted words, for the *Philadelphia Magazine* wrote the article in 1968, two years after de Grazia's *The Velikovsky Affair* had brought the story of scientific degradation to the public eye. It is obviously to that body of scandalous evidence, and not in any way to the fiction of anyone losing a post, that the accusation refers, as Bauer should himself have known. (If he did not understand to what the magazine was referring, he should have consulted *his own bibliography*, where the title of the article from which he himself has chosen to quote is "The Persecution and Character Assassination of Immanuel Velikovsky as Performed by the Inmates of the Scientific Establishment.") Does this title, my dear Henry, imply that Velikovsky lost an academic post? I think not, for it is your own contention that everything science did was aimed *at the public*. In that framework, science could certainly try to make Velikovsky into a nonperson, the aim being to silence and destroy Velikovsky in the public's eye so that he would not be read or even talked about. (It is my theory that, if Velikovsky became a nonperson, consequently his ideas would not exist: it would be as if they had not been uttered, and therefore they would not be true. For a substantial explanation of this phenomenon, which derives from collective unconscious terror, see my last chapter.)

Whatever the cause, the effect was evident: science undeniably attempted to persecute and to smother Velikovsky. That is why the magazine quite correctly characterized it as "a silent limbo," with the operative word being "silent." These magazines deduced these things eighteen and twenty-six years respectively after the Affair, in the light of all the evidence which had appeared in the interim. The same evidence was available to Bauer, and more, yet he tries to pass the attempted silencing off as *not having happened at all*. Why? Because only academics can be put into a limbo, but Velikovsky was not an academic and therefore it did not occur! That, believe it or not, is his argument, which denies not only his own evidence (especially the very title of the article from which he quotes), but common sense, for why would a respectable magazine speak of Velikovsky having been put into a limbo by science *for years* if it were not possible? Is everyone at the magazine a fool? Is only Bauer intelligent? We therefore have to choose—either this magazine is wrong, as is the one quoted previously, or Bauer's argument makes no sense and he is trying to invert the whole thing. In my opinion, the choice is not difficult for us to make.

If we seek proof that science most certainly did try to make Velikovsky into a non-person, and that it continued for many years, the evidence is not hard to find. In 1969, almost two decades after the original furor, Katherine Lindeman wrote to Harlow Shapley and Harold Urey, asking for their opinions on her projected paper on Velikovsky. Here is what Shapley wrote, three years after *The Velikovsky Affair* had exposed his machinations to the world.

"All professional astronomers consider Velikovsky a fraud. Can't you find a reputable subject for your research paper?"⁸⁷³

What does that sound like, Henry, if not an attempt to persecute Velikovsky by making him into "a non-person"? Second, here is the "authoritative" statement which Nobel prize-winner Urey "injected" into the discussion:

"Velikovsky is a tragedy. He has misguided people like you in great numbers, and my advice is to shut the book and never look at it again in your lifetime."⁸⁷⁴

What does that sound like, Henry, if not an attempt to relegate Velikovsky to a "silent limbo"?

Together, Bauer's two inversions form a very nimble and shifty device, do they not? It is simple: turn the faults around of which you stand accused and find a way to blame them on the accuser. Did the first Russian 5-year-plan fail? It was the capitalists' fault. Did the public say science behaved degradingly? It was the public's fault. A neat trick which, if deliberate, would cleverly support an intent to whitewash. Early on in his book, Bauer had asked

⁸⁷²*Ibid.*

⁸⁷³*Pensée* I, p. 36.

⁸⁷⁴*Ibid.*, p. 40.

"A major theme in the whole controversy was: to what extent might the treatment of Velikovsky be symptomatic of something generally 'rotten in the state' of science?"⁸⁷⁵

We have seen ample evidence of rottenness. Considering therefore what Bauer has done (an attempt to almost completely exonerate science), we must ask: did Bauer deliberately undervalue and therefore try to trivialize the rotten acts of science which by the 1980's almost everyone else had admitted, in order to deny that there was anything rotten in science, or has this displacement occurred naively, out of ignorance, simply because Bauer did not know what an "Affair" is? (Or had not understood what he had read?) (Or had forgotten all of it?) If it is the first, he is false, whereas, if it is the second, he is merely lacking in ability and knowledge. The choice is left to the reader, who no doubt understands that in either instance Bauer is shamed, for, if he does not remember the facts he has presented in his own book (not to mention all the external material available to him when he wrote), he is incompetent, and if he does and fails to give them due weight in his considerations, he is dishonest. We are led to consider that he might be either a liar or a fool, for there seems little room for any alternative deduction.

I prefer to think that Bauer is more a fool than a liar, which leads me to the following observation: in the first two sections of this chapter, the ignorance which Bauer displayed had forced us to conclude that, on the topic of each section (why people believe Velikovsky and how science functions), Bauer did not know what he was talking about. Here in the third section, we have to deliver a double verdict of incompetence against Bauer: on the topic of the Velikovsky Affair, not only does he not know what he is talking about, but, in this instance, he doesn't even seem to know what he's *supposed* to be talking about. He has tried to discuss why people believe Velikovsky, and failed, he has tried to tell us "what science really is" and failed, and now he has tried to discuss the Velikovsky Affair in terms of these two previous misunderstandings, but has failed to understand what on "Affair" is! It is ignorance piled on ignorance, and Bauer therefore is truly, in his own ironic words, "an ignoramus masquerading as a sage." We are given a description of science as it never is, and then of the Velikovsky Affair as it never was, and out of this never-never land of his own concoction is derived Bauer's fantasy that the Velikovsky Affair is entirely the fault of "the public." It is a ludicrous performance.

4. Bauer as Moral Guide

The object of Bauer's book, however, as he proudly avers in the later sections, is not primarily to assess the theories of Velikovsky or the rejoinders by science, but to go "beyond" Velikovsky and talk about the role of science in public life. That is his stated agenda, which we shall now look at. What he will say appears to derive from the earlier sections and is presented as if it had been reached in a reasonable and almost inevitable manner: if A and B and C, then D follows. We will discover, however, that this fourth shaky step in his shaky argument is false in two ways, both extremely damaging to Bauer. First, if A and B and C are ignorant (why "the public" believes silly ideas and what science is and what the Velikovsky Affair was), as we have just seen, then D, the moral instruction of science leading from these previous nonsenses, will be equally non-sensical. That, unfortunately for Bauer, is what happens. Rather than being founded on rock, his Sermon simply crumbles away, being built on quicksand. Second, Bauer's true purpose in writing the book, as opposed to his propagandistic one, will be shown to be far more personal and self-serving than the disinterested desire to help science which he proclaims. He has a hidden agenda, and that is what we shall unearth here, as we go "behind" Bauer.

Let us take the stated purpose (the propaganda), first, whose moral purity and utility seem unassailable: he is going to teach science what it should have done when faced with a "public issue".

"Scientists and other expert professionals are increasingly drawn into public debate on issues related to their expertise."⁸⁷⁶

⁸⁷⁵*Ibid.*, p. 59

⁸⁷⁶Bauer, *op. cit.*, p. 181.

When that occurs, they must understand what their role should be.

"As with all professions, that of science must recognize that it is accountable to the wider society, and scientists who act as spokesmen for the profession must learn to behave responsibly."⁸⁷⁷

Some scientists sometimes didn't behave "responsibly" because they did not recognize the situation in which they were enmeshed, but Bauer will try to help them to be more perceptive in the future.

"I try to show that the Velikovsky Affair can usefully be seen as a type-specimen of public controversies about technical issues."⁸⁷⁸

If they can discern that, then scientists can avoid the pitfalls of emotion into which some of them lamentably sunk concerning Velikovsky, and will be free to act only intellectually.

"It is my hope that the following chapters will stimulate thought about the challenges facing scientists when they seek to sway public opinion to their side."⁸⁷⁹

What must be understood, says Bauer, is that in such instances science is engaged in a competition:

". . . scientists had better learn that they must *compete* for acceptance of their viewpoints. If they wish to be believed, they must find more persuasive arguments than their opponents."⁸⁸⁰

Why does this concern Bauer? It is because "Velikovsky's critics, on the whole, did not compete effectively."⁸⁸¹ His object therefore is to educate his colleagues in the art of skillful debate: "to make plain what scientists must do, and what they must not do, if they are to be effective in public controversies."⁸⁸²

The flaw in this presentation, of course, is that it has nothing to do with the Velikovsky Affair as it occurred, *i.e.*, because the Velikovsky Affair was not in any way a public debate "about technical issues," as we have just seen, but an attempt by science to suppress, defame and destroy Velikovsky, Bauer's aim is a *non-sequitur*. It is beside the point. Rather than trying to teach science how to "find more persuasive arguments," he should be trying to teach it how to acquire more gentlemanly (and scientific) behavior. Having said this, however, let us be charitable and assess Bauer's argument on his own terms. Why, we wonder, does Bauer appoint himself to become the supreme guide for science? The answer is simple, as he himself states: it is because he is an expert, not only in the substance of issues like Velikovsky, but also in how they should be handled. He is, in his own words, a double expert, perhaps the only one of his type in the world, from which he derives great pleasure.

⁸⁷⁷*Ibid.*

⁸⁷⁸Bauer, *op. cit.*, p. 176.

⁸⁷⁹Bauer, *op. cit.*, p. 181.

⁸⁸⁰Bauer, *op. cit.*, p. 312.

⁸⁸¹*Ibid.*

⁸⁸²Bauer, *op. cit.*, p. 223.

"I have the satisfaction of having become an expert; when I now read some new piece of writing about the Velikovsky affair, I am usually able to spot errors and misinterpretations and red herrings very efficiently, and that is a source of personal satisfaction."⁸⁸³

This astonishing and satisfying expertise, we are told, which Bauer proclaims he worked long and hard to achieve, extends far "beyond" Velikovsky.

"Going a bit further afield, I find that my reading on other subjects . . . is more critical and analytical: neither advocates pro nor advocates con have as easy a time with me as they used to."⁸⁸⁴

He is, therefore, in his own informed estimation, the complete expert, not merely on Velikovsky but on any topic "on the fringes of science,"⁸⁸⁵ for only he among all the scientists has studied such matters thoroughly and effectively. How did he do that? By checking all the evidence for himself and not glossing over anything: "much can become clear when one pays attention to things that were ignored by others."⁸⁸⁶ Because he did not overlook things that even his fellow scientists missed, he is now able to

". . . enjoy the human satisfaction of having been right, for the right reasons, when everyone else was wrong."⁸⁸⁷

He and he alone, therefore, among all the scientists in the world, having cleansed himself of error, is pure enough in knowledge and method to deal with the Velikovsky Affair, and that is why he wrote his book, as a free gift from his own proper intellect to science. That is how Bauer presents himself.

How good and admirable it all is, we think, but, in the midst of our dizzied gratitude to Bauer, disquiet appears, for we are told that many such issues like Velikovsky are abuzz in the world. Bauer has been our expert concerning this one, but, unless he were to write a book about each of the rest (because we are not experts of the magnitude and perspicacity of Bauer), how are we to be saved from tumbling into inevitable error on those, as we did on Velikovsky before Bauer rescued us? O horror! O despair! Who can help us avoid the damnation of error to which, without external help, we seem to have been born?

No problem, says Bauer, *he* can save us, for, not only has he demonstrated his state of invulnerability from Velikovsky by his own perfect acts, but he holds out the same salvation to all those previously-accursed people who will believe in him and follow his path, as presented in his testament.

"I would be very pleased if the reading of this book were to be of like help to others. I should like my readers not just to accept my analysis and interpretations but to be stimulated to do as I did—to go to the original material and work it out for themselves."⁸⁸⁸

⁸⁸³Bauer, *op. cit.*, p. 319.

⁸⁸⁴*Ibid.*

⁸⁸⁵*Ibid.*

⁸⁸⁶Bauer, *op. cit.*, p. 321.

⁸⁸⁷*Ibid.*

⁸⁸⁸Bauer, *op. cit.*, p. 320.

It is not easy, he warns. Strait is the gate and narrow is the path, but the benefit is worth the effort.

". . . the only rewarding way to seek an answer is to proceed as I tried to do with the Velikovsky business."⁸⁸⁹

"This manner of proceeding involves effort," he tells us (for it is the struggle to kill the old way of doing things so that a new and better researcher may be born), "But it is worth the effort,"⁸⁹⁰ for one ascends out of error into a better understanding of science.

"One becomes one's own expert, and it is more agreeable to have one's own informed opinion than to cling to the opinions of others."⁸⁹¹

The old way, the old dispensation, is abolished, to be replaced by a new insight in which we can no longer be seduced by error or misperception, and the reward is truth, shining truth, which will live eternally in the new understanding to come. Anyone can do it, he says, and we are led to it by the ascent of Bauer himself from ignorance to a revelation of certitude, which he earned by his own effort and earnest desire. "It is worth thinking for oneself."⁸⁹²

The problem with this dazzling vision is that Bauer does not quite tell us how it is to be achieved by everyone. To be more precise, the evidence he supplies in his book concerning how various types of people pursue knowledge bodes very ill for the efforts of most people to put ignorance behind them and emerge from the wilderness of error to the light of wisdom. The problem is this: just who is it who is to do the "thinking for oneself"? What type of person can become "one's own expert"? It can hardly be the average member of the public, whom Bauer had characterized as being easily "seduced by such fallacious stuff" as Velikovsky because of an unreasonable "wish to believe."⁸⁹³ To Bauer, "we dearly wish to believe . . . And, having once embraced a belief, we are prepared to ignore all sorts of evidence that points to the inadequacy of that belief . . . gullibility is much more common than skepticism."⁸⁹⁴ Not much hope for understanding there. Perhaps, then, Bauer means that only scientists are able to think for themselves and arrive at truth? Yet Bauer insists that each scientist is only competent within his own discipline because "There do exist . . . barriers between disciplines," leading to "extreme specialization."⁸⁹⁵ Not much chance there, either. Perhaps, then, it is the very restricted specialists who can span all the different areas of science upon which Velikovsky's ideas touch "and work it out for themselves"? Bauer denies this too.

"The different branches of modern physics now speak different languages: each uses its own jargon, unintelligible to those working in other areas . . . It is instructive to recall just how narrow a given specialist's actual competence may be, and how uncomfortable he may be in trying to make his own individual assessment even in closely related areas."⁸⁹⁶

⁸⁸⁹*Ibid.*

⁸⁹⁰*Ibid.*

⁸⁹¹Bauer, *op. cit.*, pp. 320-321.

⁸⁹²Bauer, *op. cit.*, p. 321.

⁸⁹³Bauer, *op. cit.*, p. 185.

⁸⁹⁴Bauer, *op. cit.*, p. 186.

⁸⁹⁵Bauer, *op. cit.*, p. 293.

⁸⁹⁶*Ibid.*

This appears to present a major difficulty for Bauer's evangelical mission, which I will summarize in rabbinic fashion:

If, Henry, the public is not qualified to think for itself because it believes things unreasonably,

and if the average scientist is not qualified to think for himself because there are "barriers between disciplines,"

and if even specialists in different branches of a discipline are not qualified in alternate areas of their own discipline,

who, then, dear Henry, is going to think for himself and become his own expert?

Bauer seems to have demolished his argument once again (by his tried-and-true method of self-contradiction).

Not quite, for I said at the beginning of this section that Bauer's professed intent of helping science (and the world) to achieve truth over error was a screen for a much more personal and self-serving agenda which underlies his seemingly-disinterested goal of impersonal benevolence. We can begin to perceive that agenda when we try to answer the question posed by Bauer's contradictory portrait of those who are not able to think for themselves. If it is not "the public," nor the average scientist, nor even the advanced specialist who can become his own expert, who then can it be? Why, it is Bauer himself, of course, whom he himself has presented as the only man in the world sufficiently purged of the frailties of mankind to be able to ignore "the opinions of others merely because they are supposedly in a better position to form one,"⁸⁹⁷ and to think for himself. We have to accept this because Bauer tells us he is an expert, and then, in his expert opinion, tells us that we must regard him as an expert. Bauer therefore is the model, the book implies, Bauer is the ideal, and Bauer must therefore be listened to about Velikovsky because he alone knows the path and has the ability to discern the truth. All the rest, including his fellow scientists, cannot "work it out for themselves"⁸⁹⁸ because they "cling to the opinions of others,"⁸⁹⁹ but he can climb above that and reach his "own informed opinion,"⁹⁰⁰ which he finds much "more agreeable."⁹⁰¹ That is the bastion of agreeability which Bauer holds out to us, that we too, if we follow his lead, can rise out of error. It simply takes one's breath away.

Here we can see why I had said at the beginning of this chapter that Bauer's book is ultimately an advertisement for himself and the superior skills he feels he has acquired. That is what his book implies, that he alone is qualified to guide us out of ignorance, and we can trace the steps by which he implies it.

The public cannot hope to attain truth because it is easily misled by a cultist desire to believe.

⁸⁹⁷Bauer, *op. cit.*, p. 321.

⁸⁹⁸*Ibid.*

⁸⁹⁹*Ibid.*

⁹⁰⁰*Ibid.*

⁹⁰¹Bauer, *op. cit.*, p. 320.

Scientists are far better people than the public, not only because they are always professionally honest and sharing and reserved, but because they resist belief, test evidence and reach careful deductions.

Nevertheless, "misconceptions about science are held not only by nonscientists but also by scientists."⁹⁰²

Bauer is a scientist but, as he tells us, he is totally free of misconceptions.

Bauer is therefore the best of scientists.

Bauer is therefore the wisest, most insightful, person in the world.

We had found, at the end of each of the three previous sections of this chapter, that Bauer himself was guiltier than anyone else of the charge he had laid against Velikovsky, *i.e.*, that he himself was in each instance "an ignoramus masquerading as a sage." It is at this point here that Bauer's masquerade reaches its apotheosis, for now he presents himself not merely as *a* sage, but as *the* sage. How glorious it all is!

5. The Verdict on Bauer

We have seen enough already to allow us to draw up a very damning assessment of Bauer. There still remain several additional characteristics, however, which should be looked at before we draw the full portrait.

5A. Errors in Substance

These are not very important, either to Bauer's argumentation or to our overall opinion of him, but they should be mentioned nevertheless. For instance, Bauer is emphatic that

". . . the idea that particularly close conjunctions of Mars and earth every fifteen years could be a vestige of contacts between these planets at similar intervals in the past is just too simplistic to have any plausibility It simply carries no conviction at all for a scientist."⁹⁰³

He seems entirely unaware that Robert Jastrow, director of the Institute for Space Studies at NASA and professor of astronomy (Columbia), had written five years before, on the occasion of Velikovsky's death, that

". . . if two bodies orbiting the sun under the influence of gravity collide once, that encounter enhances the chance of another, a fact well known in celestial mechanics."⁹⁰⁴

⁹⁰²Bauer, *op. cit.*, p. 251.

⁹⁰³Bauer, *op. cit.*, pp. 270-272.

⁹⁰⁴Robert Jastrow, 'Velikovsky, a Star-Crossed Theoretician of the Cosmos.' *The New York Times*, December 2, 1979, New York, p. 22E.

Not well known to Bauer, however, despite his very extensive research on Velikovsky. Second, Bauer says

". . . Jueneman remarked that Velikovsky had been accused of publishing simply in order to make money, yet I have been unable to find that accusation in the published material. It is perhaps the only unfair criticism *not* made of Velikovsky."⁹⁰⁵

In the first chapter, I quote at least two instances where this accusation was made.

A third error by Bauer occurs in his description of a controversy about Velikovsky's use of a source which he himself had translated.

". . . when Velikovsky gave an incorrect translation from a German source, thereby actually and drastically changing the meaning, he gave a misstated fact."⁹⁰⁶

The reference is to a charge laid against Velikovsky by science historian Otto Neugebauer in the important journal *Isis* that Velikovsky had deliberately altered a number in his source to strengthen his argument. I discuss the subject fully in the first chapter, where Velikovsky demonstrates not only that it is Neugebauer himself who was wrong, who was guilty of distorting the source, but that, when this was pointed out to Neugebauer, he dismissed it as a "misprint of no concern" and never corrected his mistake or withdrew his charge, but continued to circulate the article until it had become accepted as the truth about this incident. This information was available in Velikovsky's *Stargazers and Gravediggers*, published two years before Bauer's book appeared.

To those who take the gnat's-eye view (seeing the dots but not the picture), mistakes like these would be magnified into hanging crimes. I regard them, however, as trivial and peripheral, for, when a person tries to make as many points as Bauer does in his book, he is bound to get some of them wrong. If they are not crucial, however, this is not too important, and I mention them more as clarifications than criticisms.

5B. Condemnation by Juxtaposition

Less excusable is a tendency Bauer displays to smear Velikovsky by innuendo. For example, here is Bauer discussing Velikovsky's contention that, because there is methane on Jupiter, then "Jupiter probably has oil."⁹⁰⁷

". . . these are statements that inevitably seem fairly logical to a layman but appear to the scientist as simply *non sequitur* This type of oversimplification, making connections on the basis of superficialities, is the same as that which led our forefathers in prescientific days to belief in fortune telling and magic."⁹⁰⁸

Bauer does not say openly that he is establishing a similarity (or setting up an implicit condemnation), but, by putting Velikovsky's theory and fortune-telling side by side, he is leading the reader to make the connection that they are equal. It is a sneaky device.

⁹⁰⁵Bauer, *op. cit.*, p. 236.

⁹⁰⁶Bauer, *op. cit.*, p. 198.

⁹⁰⁷Bauer, *op. cit.*, p. 271.

⁹⁰⁸Bauer, *op. cit.*, p. 271.

This sort of thing, unfortunately, crops up in Bauer too often. Here are some examples.

". . . one cannot by science absolutely prove Velikovsky wrong, just as one cannot by science disprove that the earth and all else were created in 4004 B.C."⁹⁰⁹

"Attempts to substitute the judgement of outsiders [Velikovsky and "the public"] for that of the specialists [the scientists] are unwarranted, impertinent even. What is more, if the attempt is successful, the results thereof are undesirable and error rather than truth is served—as in Nazi "science," or in Soviet "science," or where laws are enacted to forbid the teaching of evolution . . ."⁹¹⁰

"Had Velikovsky appeared ludicrously wrong, obviously wrong, to the humanists and social scientists as well as to the astronomers, would there still have been an outcry about how his concepts were treated? With how much respect shall we treat the idea that the earth is flat?"⁹¹¹

"No scientist, no academic, no individual anywhere has a right to be published on his own terms alone. The scientific journals seek a necessary compromise between rejecting anything that is unorthodox and thereby stultifying progress, and, on the other hand, publishing everything sent to them and thereby broadcasting much rubbish and nonsense."⁹¹²

". . . a discovery is premature when it cannot be used in the mainstream of science. That certainly is an apt way to classify Velikovsky's ideas—they cannot now be connected to the body of existing canonical knowledge. Therefore, quite irrespective of the 'truth' of the ideas, they could not be taken up by scientists. This phenomenon of prematurity can often explain why subjects remain on the fringes of, or outside, science: for instance, 'in the absence of a hypothesis of how ESP could work it is not possible to decide whether any set of relevant observations can be accounted for only by ESP to the exclusion of alternative explanations.'⁹¹³

Notice the tactic—sneaky connections are made without being said outright. Velikovsky and his ideas are being implicitly compared by Bauer to magic, fortune-telling, flat-earthers, the Nazis and the Soviets and fundamentalist anti-evolutionists, as well as to creationists and believers in ESP, which makes his theories, of course, "rubbish and nonsense." This sort of thing isn't very nice, Henry.

5C. Bauer's Methods

Up to now, we have been looking at the low quality of Bauer's research and the negligible value of the results therefrom. It is a very weak performance, as we have seen, but we will find that its ineptitude of substance is matched by the nonsensicality of its prior principles, *i.e.*, the methods of approach Bauer has used, to which we shall now turn to complete the picture.

⁹⁰⁹Bauer, *op. cit.*, p. 256.

⁹¹⁰Bauer, *op. cit.*, p. 276.

⁹¹¹Bauer, *op. cit.*, p. 279.

⁹¹²Bauer, *op. cit.*, p. 281.

⁹¹³Bauer, *op. cit.*, p. 300.

When I first heard of the title of Bauer's book, *Beyond Velikovsky*, I unfortunately misunderstood what he meant by "beyond." In my innocence, I thought that at last there would be an independent and open-minded review of the theory of catastrophism as it had developed and been modified and had branched off "beyond" the original formulation by Velikovsky. When I read it, of course, it was a great disappointment, for Bauer's book, alas, provides nothing of the sort. Rather than going "beyond" Velikovsky, it deals only with Velikovsky, and, as we have seen, in a manner which is not at all open-minded or fair, but prejudiced, naïve and inadequate.

There are many reasons for the failure of the book, and we have already examined several of the most important. It is time now to look at one more of the major causes of Bauer's failure (beyond his ignorance of the topics he tries to discuss), which is the methods Bauer used to conduct his investigation, *i.e.*, the procedure by which he approached Velikovsky. We shall find that this is not merely inadequate but ridiculous, to the point where he not only fails to get beyond Velikovsky, but never even manages to get up to Velikovsky.

Consider what Bauer has done. His book is 321 pages long, but, in that total, only a mere 36 pages, or 10% of the whole, is devoted to what Bauer calls "Velikovsky's Physical Science." All the rest (or over 280 pages), does not deal directly with the ideas of Velikovsky. Seems preposterous, does it not, but matters get worse, for, out of that 10%, more than half is devoted to a little pamphlet of Velikovsky's, *Cosmos Without Gravitation*, which was published in 1946, four years before *Worlds in Collision*. The substance of this pamphlet is unknown to most of Velikovsky's supporters, the majority of whom have never even read it, and it plays no role in the major part of Velikovsky's work, being almost never mentioned or even referred to. It is, that is to say, a minor, secondary, early piece of writing. The bulk of Bauer's analysis of Velikovsky's science in this section, however, concerns this pamphlet, with the result that only a minuscule portion of his book, not more than 4% in all, is given over to a formal evaluation of all of the rest of Velikovsky's theories put together. That, hard as it is to believe, is Bauer's method of analysis. The vast amount of evidence and argumentation which Velikovsky presents in *eight* published books beginning with *Worlds in Collision*, plus what he wrote in a number of articles that appeared in the journals *Pensée* and *KRONOS*, is treated in only a dozen pages or so, yet Bauer actually thinks that this is a proper and adequate method of procedure.

That is not the extent of Bauer's methodological ineptitude, however, for, not only does he leap over Velikovsky in a mere part of a chapter, but he fails completely to go beyond Velikovsky to all of the work on catastrophism that has been produced by those who follow his ideas. This work amounted, even by 1984 when Bauer's book had appeared, to literally hundreds of scholarly articles by people who take his ideas seriously, articles in many disciplines, articles which, as with any great innovator, not only carry his ideas further, but also expand, revise, contest and even reject some of them, which is always the characteristic of a science in the first heady energetic days of its infancy and rapid proliferation. This is a vast field in itself and is growing steadily, and much of what it contains is as important and often as revolutionary as the original. It constitutes the science of catastrophism, a mine of valuable ideas and data, yet Bauer in his allegedly thorough analysis of Velikovsky pays almost no attention to this entire field. Not only therefore does he seem to misunderstand which of Velikovsky's writings are the most important (even though he says he spent much time on the matter), but, beyond that, he has not taken into adequate account the work of what he calls "the Velikovskians." Why? Because, unfortunately, he has failed to grasp the difference between Velikovsky's books, which we may call Velikovskianism, and catastrophism, the larger field that has developed out of them, yet he feels that he has correctly and usefully approached Velikovsky. The incompetence of this pattern of procedure is astonishing. As a method, it is devoid of sense.

To provide the reader with some framework of comparison by which to measure how foolish Bauer's method is, let us imagine that he had proceeded in the same manner regarding Freud. Suppose he had spent most of his time evaluating an early article by Freud entitled "On the Psychical Mechanism of Hysterical Phenomena," written with Josef Breuer and published in 1893. He would have chosen it, let us say, because he has some knowledge of hypnotism and this is the only piece of Freud's writing that he can grapple with (as Bauer did with colloids in *Cosmos Without Gravitation*). What he would find, writing his evaluation let us say in 1950, is that in that article Freud insists on getting at the trauma through hypnosis rather than free association, and that he does not emphasize the sexual nature of the child's desires, both of which are the foundations of later work. Bauer would then conclude about Freud (as he did with Velikovsky on science), that Freud was incompetent in psychology, and therefore that it would be safe to deduce that whatever Freud might write after that would be equally incompetent (as Bauer did with Velikovsky). That would be the extent of his evaluation, and Freud would be dismissed as "an ignoramus masquerading as a sage."

He would then spend a mere dozen pages on all the rest of Freud's writings put together, that is, everything he wrote from 1900 to 1939, which constitute what we might call Freudianism, and dismiss them with the observation "that similarly compelling arguments are available to contradict" each of them,⁹¹⁴ as he dismisses Velikovsky. This wholesale rejection would be derived mostly from his assessment of the early article, which plays almost no role in Freud's later writings and is virtually never mentioned by the man himself or by any of his followers.

It gets worse, however, for, not only would he concentrate mostly on the early (and non-indicative) article, a tactic which is in itself grossly unbalanced and wrong, but he would pay no attention at all to the vast amount of work produced by Freud's colleagues and followers (the post-Freudians). These would include people like Bleuler, Jung, Adler, Stekel and Abraham, as well as later theorists like Rank, Ferenczi, Reich, Sullivan and Fromm, not to mention still-later psychologists like Horney, May, Klein, Jones, Sachs and Anna Freud. The work of these people, as anyone knowledgeable in psychology would be aware, constitutes an extremely important body of ideas, but Bauer would totally ignore it because he would have failed to grasp the difference between Freudianism and the larger field of psychoanalysis. Despite all of these faults, Bauer would nevertheless feel (in his ignorance, incompetence and naivety), that he had usefully evaluated Freud, unaware that what he had done was not merely a travesty but a decapitation.

A second example might be Darwin. Imagine that Bauer sets out to get "beyond" Darwin and begins with his early article *Journal of Researches into the Natural History and Geology of the Countries Visited During the Voyage of H.M.S. Beagle Round the World*, published in 1834. He would find in that work conjectures about "repeated exterminations" apparently caused by "some great catastrophe" that apparently shook "the entire framework of the globe," (as quoted in Velikovsky's *Mankind in Amnesia*, 72) and he would deduce that, since these ideas contradict the attitudes of science in 1950, and since the article displays no knowledge of genetics or mutation or neo-Darwinism or punctuated equilibrium (not to mention even natural selection), the author is without doubt "an ignoramus masquerading as a sage." He would then, in a 300-page book, spend no more than a dozen pages on all the rest of Darwin's writings put together, including the *Origin of Species* and *The Descent of Man*, saying simply that "similarly compelling arguments are available to contradict" each of them, and derive the bulk of his assessment from the early article, which is unknown to most biologists and plays no role in their thinking. Despite the appalling inadequacy of such a method, Bauer would nevertheless feel (as he does about Velikovsky), that he had "worked through the record" and had "thought about the general issues involved" and that he therefore understood everything—"All that happened now seems explicable to me."

Worse than that, he would totally ignore all of the discoveries made by those who came after Darwin, including the work of Mendel and de Vries on genetics and mutation, not to mention the host of biologists in the twentieth century (like Kellog, Bateson, Chain, Grasse, Waddington, Dobzhansky, Gould, Eldredge), who have developed, extended, refined, revised, debated and even rejected some of Darwin's ideas as they passed through the various stages of favor or disfavor to the point they have reached now, which is itself only a stage on their path. All of this would simply not be treated. The reason for this monumental weakness in his method would be that Bauer would have failed to appreciate the difference between Darwinism and evolution. Does anyone think that such an evaluation of Darwin is adequate or makes sense? Or of Freud?

Yet that, I'm afraid, is what Bauer has done regarding Velikovsky. He concentrates mostly on an early minor work, zips through the major works in a dozen pages, pays no attention to the voluminous work of people who have come after and fails to understand the difference between Velikovsky and catastrophism. As a method, it is absurd. Perhaps this method of evaluating Velikovsky is so ludicrous because Bauer approaches ideas like a chemist approaches matter. That is to say, if a chemist wishes to identify a substance before him (say, a pile of sulphur), then, if he evaluates only a small part of it, he can usually safely infer that all the rest must be the same. Even if he were to go into a coma, the substance before him would remain what it was. What Bauer does not understand, however, is that a person's ideas are not homogeneous and inert like an amount of sulphur. Thinking is not comatose, but active and ongoing, because ideas interact (as even substances might do), and therefore one's ideas grow, they evolve, they are modified, they are in motion, for that is what it is to think. Ideas are never simply added to earlier ones inertly, like more sulphur onto sulphur. Bauer the chemist, however, seems unable to perceive this interactive nature of speculation and treats Velikovsky as if thoughts were like sulphur, that is, as if the part is like the whole and will always be, and to catch any of it at any point is indicative of it forever. It never occurs to him

⁹¹⁴Bauer, *op. cit.*, p. 112.

that Velikovsky's ideas (like Freud's or Darwin's), may have evolved during these men's long and highly interactive lifetimes (as every biographer says they did). To Bauer they are fixed eternally from the first moment, like a pile of sulphur on a laboratory table.

It is evident, therefore, that Bauer the scientist has set up a method of approach which is not merely scientifically incorrect and ineffective, but is actually ignorant of the field it intends to evaluate. It is no wonder, therefore, that the results it produces are as inadequate and foolish as we have found the rest of Bauer's book to be. Ironically, therefore, we are amused when Bauer proclaims

"I have thought a great deal about the Velikovsky Affair, and now I have written quite a lot about it."⁹¹⁵

Well, Henry, you *have* written a lot, to be sure, but most of it is worthless. Perhaps you would have been better advised to think more and write less.

5D. Bauer's Motive

Having seen the extent of the ineptitude of Bauer's work, we now have to wonder why he would be so eager to exhibit his foolishness in public in a book, for everyone to see. The most evident answer is that he is not aware that he is ignorant and incompetent, and therefore does not know that he is making a fool of himself. That is probably correct, but we must now ask an anterior question. We must try to find out why Bauer felt compelled to write this particular book, on this particular topic and with this particular approach, at the particular time in his life when he began.

To answer this, we will look as usual for unconscious evidence in Bauer's own book, and our first set of indications is to be found in Bauer's attempts to explain why in his opinion Velikovskian research is attractive.

1. It is easier: "the research effect per publication is likely to be greater in conventional research than in Velikovskian research; the latter can usually be done more quickly."⁹¹⁶
2. "In Velikovskian research no particular expertise in special techniques is needed. On the other hand, in conventional research one needs to apply refined methods acquired in years of study and practice."⁹¹⁷

Why is that?

". . . conventional research is the province of the specialist; it requires refined techniques, a deep understanding and knowledge of facts and theories accumulated over a long period of time Velikovskian research, however, is open to any intelligent and enthusiastic layman—no training or professional qualifications are needed."⁹¹⁸

⁹¹⁵Bauer, *op. cit.*, p. 319.

⁹¹⁶Bauer, *op. cit.*, p. 190.

⁹¹⁷*Ibid.*

⁹¹⁸Bauer, *op. cit.*, pp. 190-191.

3. "Velikovskian studies are new, and we need to learn little before participating actively."⁹¹⁹
4. "Velikovskian research is comparatively easy: a result is quickly obtainable, and Anyone who has ideas and insights . . . can derive satisfaction from writing about them."⁹²⁰
5. "In Velikovskian research . . . the satisfaction of having made a new connection is unlikely to be washed away by the cold water of established facts and theories."⁹²¹

That is Bauer's assessment of the research produced by Velikovsky's followers (which, it is implied, is of course totally unlike his own). The verdict is rather harsh, and what we then expect from a man like Bauer (who tells us that he is an expert on science and should therefore proceed scientifically), is that he would provide evidence for these blanket condemnations. Alas, as with so many of his facile pontifications, no evidence at all is forthcoming, not even a shred. The accusations are merely tossed off at us by Bauer, without support, as if we must take them for granted at face value, without question, seeing that Bauer is an expert. He never even mentions one instance. Some of us, however, (despite our admiration for Bauer the expert), might nevertheless feel that it would have been more effective had Bauer furnished even a bit of support, say, by comparing the experiences of three anti-Velikovskian chemists against those of three pro-Velikovskian chemists, to see if the results bear out his statements, *i.e.*, how much time each one spent on preparing articles and doing research and achieving results. Nothing of this kind, however, is forthcoming from Bauer.

There *is* a bit of evidence that we the readers might adduce, however, is there not, for we know of one anti-Velikovskian chemist, don't we? His name is Henry Bauer, and we have his data in his book. Let us therefore take a look at Bauer's research, to see to what extent it does or does not present *him* (he being the expert of experts, as he tells us), as the model "conventional" researcher, one whose work never falls into the errors he casually attributes to all Velikovskians absolutely, but demonstrates "particular expertise" and "deep understanding" and "professional qualifications" far above the Velikovskians.

The evidence is Bauer's own work and, when we examine it, it turns out that he fails to satisfy any of the criteria which he established for "conventional" research (and he is, once again, hung by his own words). There is a lot of proof for this, but, to save time, we might sum it up by addressing a series of questions to Bauer.

When you try to discuss why people seem so ready to believe in unusual ideas like Velikovsky's, and you merely toss off random ideas without any support, is this the application of "particular expertise in special techniques"? What techniques would they be, Henry?

When you try to describe science and betray an almost total ignorance of the up-to-date discussions of science by philosophers, historians, sociologists and psychologists of science, that's pretty "easy," isn't it, Henry? Doing it that way, "a result is quickly obtainable," isn't it, Henry? What "refined techniques" were you applying there, Henry?

When you try to evaluate the Velikovsky Affair and you completely miss the boat because you don't even know what the word means, what "deep understanding" were you displaying, Henry, what "knowledge of facts and theories accumulated over a long period of time"?

⁹¹⁹Bauer, *op. cit.*, p. 191.

⁹²⁰*Ibid.*

⁹²¹Bauer, *op. cit.*, p. 192.

When you present yourself as the world's greatest expert on such matters as Velikovsky, that's something from which one can "derive satisfaction," is it not, Henry, a satisfaction which "is unlikely to be washed away by the cold water of established facts" at the hands of your fellow scientists?

When you employ a method of evaluation which indicates that you do not know which the important writings in the field are and that you cannot distinguish between the man and the movement he initiated, what "refined methods acquired in years of study and practice" did you use, Henry? Are they easily available? Can your fellow scientists learn them too?

Do you not see, Henry, that you yourself are ironically the victim of your own accusations? That *your* work more than Velikovsky's indicates that "the research effort" was "done more quickly"? That in your case you certainly acted as if "no particular expertise . . . is needed"? That "no training or professional qualifications are needed"? That "one needs to learn little before participating actively"? That "anyone who has ideas and insights," (no matter how foolish, ignorant and incorrect they are)," can derive satisfaction from writing about them"?

Don't you see any of this, Henry?

The answer is, of course, that Bauer does not, that he seems compelled to write his book in oblivion of his ineptitude, and it is this clue which we must pursue. That will therefore be our task now, to search for Bauer's true motive, and we will proceed on the principle that, if it is permissible for Bauer the chemist to psychoanalyze Velikovsky sight unseen (as he does), then it will be equally permissible for us to psychoanalyze Bauer on these terms, and to utter our opinions about him as definitively as he does about Velikovsky. What is sauce for Velikovsky is sauce for him.

When we ask why Bauer exposes his incompetence so willingly, why he seemingly cannot help himself, the unconscious indications for it are, of course, ironically supplied by Bauer himself in his remarks about Velikovsky. Efforts like Velikovsky's, he says, which are "easy," "quick" and require "no training," might

". . . be attractive to those who, though successful, have experienced and perhaps tired of the difficulties of working within one of the specialized disciplines. After laboring within the confines of the judgment of one's specialized peers . . . how relaxing to be able to write speculatively, with a broad brush, about the most major questions."⁹²²

We suddenly realize that this description, aimed at Velikovsky, sums up precisely what we had found in Bauer—most of his book *is* broad-brushed, unsupported, facile speculation. Does this constitute, we wonder, an unconscious self-revelation? If it does, however, we must then ask why someone who is set up in a known and established specialty would risk entering an unknown field? Here is Bauer's shallow answer:

"We are all prone to the desire to excel at other things than our professional specialty . . . and the opportunity to make lauded contributions without too much difficulty is offered by Velikovskian [or, as we see, anti-Velikovskian] research . . ."⁹²³

⁹²²*Ibid.*

⁹²³Bauer, *op. cit.*, pp. 192-193.

It is a personal satisfaction, says Bauer, a source of much pleasure: "One has a creative pastime."⁹²⁴

I find that explanation rather feeble. It seems like a smoke-screen to obscure the truth. Surely, there must be more to it than that? Something deeper, perhaps, something that Bauer himself cannot consciously perceive? Using as our working hypothesis the concept that what Bauer intimates about Velikovsky might be actually true of himself, we look about for more unintended revelations, to see if there could have been pressures in Bauer's own life that might have propelled *him* to embark on an ill-fated expedition into a field and a task for which he was so singularly badly-equipped (as he accuses Velikovsky). As usual, Bauer does not disappoint us, for his book abounds with intimations about what might have been a crisis in his life, intimations of which he is not aware. These are directed at Velikovsky, but, when we turn them around, we see that they might apply to Bauer himself. For instance, Bauer asks

"Was it by chance that Velikovsky set out in an entirely new and ambitious direction when he was in his forties? . . . The phenomenon of a man in early middle age who seeks to start afresh is a well-recognized one."⁹²⁵

We note with surprise that that was precisely Bauer's age when he began to think about the ideas of Velikovsky, and suddenly we begin to suspect that perhaps Bauer's attack, when it started, did not begin "by chance." Next, why would Velikovsky have launched himself into a totally *new* project? Here is Bauer's answer.

"Most of us have, by then, gone about as far in our professions as we are ever likely to . . . there is, in the ordinary way of things, not much to anticipate in the form of new challenges and new excitements."⁹²⁶

It is left to the reader to decide how true this was of Bauer himself, but it must be remembered that this thought was first raised by Bauer. We did not invent it to use against him, it was put into our minds by Bauer. Next, what happens to people in such a crisis? Some switch careers or divorce and remarry, says Bauer, but

"Perhaps Velikovsky, like so many of us, was ready to take up something quite new, to throw himself with full energy and conviction into a venture that offered new insights and the possibility of new achievements."⁹²⁷

Here, I think, Bauer has unknowingly revealed the nature of his own motive: it is ego, pure and simple. He has an intense need to present himself as the purest scientist in the world (as I described a moment ago in Subsection "5D. Bauer as Moral Guide"), which requires him to pretend that his own research, unlike that of the Velikovskians, and even unlike that of his predecessors among Velikovsky's critics, is faultless, and now we can guess why. Following his own lead, we might surmise that it is he himself, not Velikovsky, who had reached middle age and had gone as far as he could in his profession, with "not much to anticipate," and that the Velikovsky debate, which he came across out of the blue, was "something quite new," something which "offered . . . the possibility of new achievements." What could those "achievements" be? Well, Shapley had tried, Payne-Gaposchkin had tried, Menzel had tried, Asimov had tried, Sagan had tried, even the mighty AAAS had tried, and they had all failed to destroy Velikovsky. Now Bauer suddenly felt that he could do it, he alone, and debunk all the rest in the process. What a way to score points in the game of status (*i.e.*, to keep death and oblivion at bay)! He would become better

⁹²⁴Bauer, *op. cit.*, p. 193.

⁹²⁵Bauer, *op. cit.*, p. 177.

⁹²⁶*Ibid.*

⁹²⁷Bauer, *op. cit.*, pp. 177-178.

than them! Not much was available to him in his own field, and, even if it were, hardly anyone would ever notice, but, by demolishing the notorious and hitherto-invulnerable Velikovsky, the bugbear of science, he, Bauer the ordinary chemist, would elevate himself in one fell swoop above all of these more famous luminaries and become not only The Man Who Killed Immanuel Velikovsky (which none of them could do), but the savior of the reputation of science! Bauer the Ugly Duckling, rather than Sagan or Shapley, would turn out to be the Swan of Salvation. How much better to do something as sensational as that than to merely plod along in chemistry.

We may then guess that he threw "himself with full energy and conviction into a venture that offered" such large rewards for so little effort, and he partly admits this himself because he is so very pleased with the result. "Most of the time," he says smugly, "I thoroughly enjoyed what I was doing, aware that I was learning much in the process."⁹²⁸ What was he learning which gave him so much "enjoyment"? Why, it was the truth about Velikovsky, that's what! He began, he tells us, "in something of a state of surprise" because "the issue apparently had not been decisively settled,"⁹²⁹ but now he is joyous and happy. He is exalted.

"I am no longer in a state of surprise. All that happened now seems explicable to me—I flatter myself that I understand it."⁹³⁰

Herein derives his tone of evident self-satisfaction. Sagan didn't understand "the issue," Shapley didn't understand it, but *he* has both understood and "settled" it! That is his "new achievement," his "lauded contribution," to be the first one in the world to understand it *all*, and it explains why he presents his book to the world in the tones of hot, flushed messianism that I pointed out a moment ago. This is his summit, his Everest. He has boldly gone where none have been able to go before him, and nearer to perfection than this no man may aspire.

5E. The Verdict

We have found, without question, that Bauer's book on Velikovsky is an ignominious failure. It remains for us now only to organize our evaluation of him in succinct form. As usual, if we seek an angle of attack, we merely turn to Bauer himself and he faithfully provides it for us. The opening, as always, is an assault on Velikovsky which turns out to be much more descriptive of Bauer himself.

"Velikovsky had not followed the approach that is the accepted one in scholarly investigation. He did not present for discussion his methodology, so that it could be judged apart from his conclusions and as a prerequisite to them; he used as a test of validity only concordance with his own premises; and then he asked that his conclusions be accepted despite their conflict with existing ideas in a variety of well-established intellectual disciplines."⁹³¹

That is Bauer's assessment of Velikovsky. Let us see if it is true, and, if not, whether it applies more truly to Bauer than to Velikovsky. What we shall find, of course (as the reader by now has no doubt anticipated), is that Bauer aims at Velikovsky but, with unerring accuracy, always shoots directly into his own foot. In the quotation, Bauer lays three charges of inadmissible procedure against Velikovsky. First, "He did not present for discussion his methodology, so that it could be judged apart from his conclusions." I think Velikovsky's methodology is evident, so much so that most of his critics have had no problem in perceiving it, for one of the chief accusations against Velikovsky, echoed by critic after critic from the very first moment (as Bauer himself states in his book), is that it is

⁹²⁸Bauer, *op. cit.*, p. 319.

⁹²⁹Bauer, *op. cit.*, p. xi.

⁹³⁰*Ibid.*

⁹³¹Bauer, *op. cit.*, p. 92.

incorrect for him to use ancient history, myth, legend and religion as reliable data. All one has to do is to look in Bauer's book and one will find a number of these criticisms quoted or cited. Were all these people writing of a method they could not perceive? On the contrary, Velikovsky not only states quite plainly in *Worlds in Collision* the principles upon which he will proceed, but follows them to the letter.

Alas, it is not the same for Bauer. He must be approached with more caution, for there is a great difference between what he says and what he does. (It is the distinction between "noise" or propaganda, which is irrelevant, and what I choose to call "action," which is the essence.) The reader may remember how, in the second part of this chapter, I began my analysis of Bauer's projected discussion of science by quoting his proclamation of how he would proceed. It sounded noble and correct, but I then showed that what he did (action), as opposed to what he said (noise), was the opposite. He did not follow his plan, but inverted it. The same thing, I contend, happens here too, concerning Bauer's method—he does not follow his dicta, but inverts them. If we listen to what he *says*, he talks a good game, but if we pay more attention to what he *does*, we see that his game becomes weak or even false. For instance, he declares, as early as the Preface, that Part I will recount the Velikovsky Affair, Part II will discuss Velikovsky's ideas and crackpots, while Part III will comment on the general issue of public controversy in the light of the earlier sections. Sounds perfectly straightforward and proper, does it not? (Just like Bauer's description of how he would approach science). That is what Bauer *says*, but what he *does*, as I outlined a moment ago in the subsection "5C. Bauer's Method," is completely different. He spends only 12 pages out of 321 discussing Velikovsky's major ideas, offers no data to support his facile over-generalizations about why people believe sensational ideas like Velikovsky's, uses as his description of science merely the shibboleths and truisms he was taught in school, fails to understand what the Velikovsky Affair was and presents himself as the Savior of Science. That, I'm afraid, is Bauer's real method (as opposed to his propagandistic one), but nowhere does he present it "so that it could be judged." It is precisely for this reason that I have had to pick it out from behind his writing and present it myself to the reader to "be judged." On this point that he himself raised, therefore, Bauer fares far worse than Velikovsky. He did not, alas, follow "the approach that is the accepted one in scholarly investigations."

Second, Velikovsky is said to have asked "that his conclusions be accepted despite their conflict with existing ideas." Well, Henry, what about your conviction that the Loch Ness Monsters exist? You accuse Velikovsky of always speaking as if he were absolutely right, but there is no trace of doubt in your *pronunciamentos* on the Loch Ness Monsters, despite your own admission in your own book that your belief cannot be proved, *i.e.*, that it is in "direct conflict with existing ideas in a variety of well-established intellectual disciplines." Doesn't this sort of make *you* a crank, Henry? Haven't you, *by your own standards*, "not followed the approach that is the accepted one in scholarly investigations"?

Third, Velikovsky is accused of using "as a test of validity only concordance with his own premises." I think that that is a serious misrepresentation of Velikovsky, for he did the opposite. His method was to gather as much concordant data as he could to support a point, hoping thereby to convince the reader that the data was relevant. That is to say, he offered the reader the opportunity to reach an independent decision. With Bauer, however, the situation with regard to his beliefs is completely the reverse, for we shall find not only that it is he, rather than Velikovsky, who has a very strict standard of belief with which every datum must agree beforehand, but that this standard is to a large extent his own private preference, to the point where he himself alone becomes the lodestone by which all truth is to be evaluated.

As I will offer proof for this point, one last great Bauerian myth will be demolished. We have already destroyed a great many of them, but now it is time to deal with the final one, which is the fiction of Bauer's pure and serene objectivity. To do this, we will once again have to be careful to separate what Bauer says about his approach (the noise), from what he actually does (the truth). When we do that, we shall find that his book is not merely biased and slanted beforehand, but that its tilt is dictated by Henry Bauer himself. It is not objective in any way.

In order to see this, what must be overcome is a barrage of propaganda. It is not much unlike the situation of a government ministry setting out to cut, shall we say, some social benefits, while all the time proclaiming that it is performing a great service to the country. If you listen to what is said, you might believe it is an act of great humanitarianism, but, if you look at what is being done, it becomes something very different. So it is with Bauer and his propaganda.

Consider the way the book comes to us as a package. On the jacket, it is called "fair-minded and lucid," in contrast, it is said, to the earlier derisive attacks. The book, we are told, is "a great improvement over previous critical studies," (which are said to have "undercut their case by resorting to innuendo, ridicule, misrepresentation"), it is "a very valuable contribution to the literature on a major controversy," it is "the best comprehensive analysis of"

the Velikovsky debate, it is "a model of how such public disputes . . . can be understood and even resolved." One would think that the Millennium had arrived.

Bauer himself is no slouch in this area, for his own proclamations about his efforts are presented in the same propagandistic vein of stainless objectivity. He has no axe to grind, he tells us.

"I began this work simply because I found the Velikovsky Affair a fascinating controversy about which I wanted to become clear."⁹³²

(The previous sub-section of this chapter, "5D. Bauer's Motive," offers a very different opinion about why Bauer began). After that, says Bauer the model scientist, he proceeded only on facts.

"My aim has been to document my own attempt, begun initially without conscious prejudice, to reach a conclusion on the basis of the facts"⁹³³

That and that alone is his alleged method, as a result of which he is certain that he is right.

"I . . . feel satisfied that I had reached a reasonably objective view of Velikovsky and his work—'objective' in the sense that I began with no preconceptions (other than those inherent in being a scientist) and can point to facts to support my opinions."⁹³⁴

Emotion was therefore avoided by Bauer at all times, he insists, because it hampers objectivity. He has seen instances of people

". . . who caused harm to varying degrees as a result of naivete, incompetence, personal feelings of insecurity"⁹³⁵

all of which we have found in Bauer himself. Being certain, however, that he is free of these faults, he proclaims that he has totally avoided subjective partiality.

"Strong emotion makes it more likely that one will behave . . . in such a manner . . . ,"⁹³⁶

but Bauer escapes all this "by attempting to come to terms with reality."⁹³⁷ For those lesser mortals, however, who cannot achieve such a lofty peak of disinterestedness, Bauer casually offers several sources of self-help.

⁹³²Bauer, *op. cit.*, p. xii.

⁹³³Bauer, *op. cit.*, p. 173.

⁹³⁴Bauer, *op. cit.*, p. 175.

⁹³⁵Bauer, *op. cit.*, p. 246.

⁹³⁶*Ibid.*

⁹³⁷*Ibid.*

"Techniques for recognizing and avoiding the irrational 'should' and the ensuing irrational anger have been proposed by Ellis and Maultsby."⁹³⁸

Bauer himself, of course, does not need any of this improvement because he is far above that. The impression we get, consequently, is that Bauer is purged of such emotions, even though he lets slip that Velikovsky displays "an intellectual arrogance that I find somewhat distasteful,"⁹³⁹ and that, for these and related characteristics (*e.g.* "an overwhelming sense of self-importance,"⁹⁴⁰ or "the extremeness, the one-sidedness, of Velikovsky's assertions,"⁹⁴¹) Velikovsky is "someone whom I would always be reluctant to believe."⁹⁴² None of these objectionable traits, however, sway Bauer from his pure, single-minded, scientific purpose, we are told, so that, when he finished his great task, he can in all honesty present to the reader, purged of all subjective bias and distracting emotion,

". . . the train of thought that led me at last to feel satisfied that I had reached a reasonably objective view of Velikovsky and his work."⁹⁴³

How can one dare to argue with such noble principles?

That, however, is the noise. The truth is somewhat different, which brings us back to the third of Bauer's charges against Velikovsky, that "he used as a test of validity only concordance with his own premises." Let us approach this contention step by step. First, Bauer accuses Velikovsky's supporters of believing something to be true only if it agrees with Velikovsky's theories: "Velikovskian science is based on the assumption that Velikovsky's cosmic scenario is largely correct."⁹⁴⁴ He then accuses Velikovsky himself of the same bias: "he judged the validity of his interpretations by how satisfying they seemed to him, not to anybody else."⁹⁴⁵ Bauer calls this a "purely subjective" method,⁹⁴⁶ and compares it very unfavorably to the method of science at large, and even less favorably to his own expert rules of procedure. Velikovsky, in contrast to Bauer, is quite simply non-scientific.

That is the noise. Now let us look at the truth. Over and over again, Bauer tells us that science will only consider something to be true if it is "consonant with accepted laws or theories."⁹⁴⁷ It has to agree with "a whole set of ideas that had been satisfactory in the past and continue to be satisfactory."⁹⁴⁸ There is therefore in science "a definite preference for data that can be correlated,"⁹⁴⁹ for "facts" which can be connected "to what already exists in science."⁹⁵⁰ The criterion in science above all is that one's ideas have to fit with "established knowledge,"⁹⁵¹ "with what was previously known."⁹⁵² Everything that is believed has to fit "within the framework of accepted

⁹³⁸*Ibid.*, footnote 5.

⁹³⁹Bauer, *op. cit.*, p. 154.

⁹⁴⁰Bauer, *op. cit.*, p. 154.

⁹⁴¹*Ibid.*

⁹⁴²Bauer, *op. cit.*, p. 173.

⁹⁴³Bauer, *op. cit.*, p. 175.

⁹⁴⁴Bauer, *op. cit.*, p. 67.

⁹⁴⁵Bauer, *op. cit.*, p. 178.

⁹⁴⁶*Ibid.*

⁹⁴⁷Bauer, *op. cit.*, p. 259.

⁹⁴⁸*Ibid.*

⁹⁴⁹Bauer, *op. cit.*, p. 260.

⁹⁵⁰*Ibid.*

⁹⁵¹Bauer, *op. cit.*, p. 262.

theories,"⁹⁵³ *i.e.*, within orthodox science alone—"The test of what is likely is whether there is a concordance with reliable experience, which is embodied in the existing disciplines."⁹⁵⁴

I see no difference in form between the alleged attitudes of Velikovskians and scientists as herein described. They are (if true) identical, for each group believes wholly in its own "premises" or "knowledge," to which everything must be in "concordance." Bauer would, no doubt, retort that there is a difference in content, *i.e.*, that science is correct to measure everything by its "established knowledge" because that knowledge is correct, whereas the Velikovskians are wrong to use Velikovsky's theories as a measuring rod because Velikovsky is incorrect, but that is irrelevant. The charge of which Bauer accuses the Velikovskians, of measuring data by theory, is precisely what science does too, as he himself unintentionally substantiates. He is very wrong there.

Now to the second part of the accusation, that Velikovsky's own method was equally subjective, that "he judged the validity of his interpretations by how satisfying they seemed to him." This, in Bauer's eyes, is perhaps Velikovsky's most terrible sin, being totally contrary to how things are done in science, and particularly contrary to how Bauer himself conducted his investigation. Again, that is the noise, but now here is the truth, in Bauer's own words, which destroys him utterly. He describes his struggle "to form an opinion about two opposing views,"⁹⁵⁵ that is, whether Velikovsky is correct or not. It was difficult, he says, "But it is worth the effort."⁹⁵⁶ Why? Because,

"When or if one arrives at a conclusion, he has established a belief that is in accord with his own set of other beliefs, and he can feel comfortable with it."⁹⁵⁷

Notice what Bauer has just said: he does not evaluate his conclusion according to the "established knowledge" of science (as his "noise" had postulated), but only in terms of how well it "is in accord with his own set of other beliefs." That is his *action*: it is "a conclusion with which I could be comfortable, since I had arrived at it myself."⁹⁵⁸ "I had answered that question to my own satisfaction."⁹⁵⁹ We suddenly realize that this is the sole criterion that makes him "comfortable" with his conclusion: "it is more agreeable to have one's own informed opinion than to cling to the opinions of others."⁹⁶⁰ What this means, quite simply, is that Bauer himself, and he alone, is the standard of truth. That is the limit of his alleged "objectivity."

To perceive this very incongruous and embarrassingly unscientific attitude in Bauer, this colossal self-centeredness, allows us to make some sense of the bizarre statement in his Preface where he describes how, having at last (in his expert opinion) finally understood the Velikovsky Affair, he is "no longer in a state of surprise."

"I think that sort of change, from a state of surprise to one of calm acceptance, is in fact a demonstration that understanding has been gained."⁹⁶¹

⁹⁵²Bauer, *op. cit.*, p. 263.

⁹⁵³Bauer, *op. cit.*, p. 264.

⁹⁵⁴Bauer, *op. cit.*, p. 202.

⁹⁵⁵Bauer, *op. cit.*, p. 320.

⁹⁵⁶*Ibid.*

⁹⁵⁷*Ibid.*

⁹⁵⁸Bauer, *op. cit.*, p. 113.

⁹⁵⁹Bauer, *op. cit.*, p. 175.

⁹⁶⁰Bauer, *op. cit.*, pp. 320-321.

⁹⁶¹Bauer, *op. cit.*, p. xi.

What does this mean? That, if one accepts something *calmly*, it therefore must be correct? It has been "understood"? Is this a universal law? No, I'm afraid not: it applies only to Bauer. This passage makes no sense unless one sees it only in terms of Bauer himself as the sole standard of scientific value: when *he*, Bauer, is in "a state of . . . calm acceptance," this "is in fact a demonstration," it means absolutely that "understanding has been gained." He alone is the measure of "understanding."

Is this science? Are these personal and private criteria scientific? Notice the important determining words—"comfortable," "agreeable," "satisfied," "calm acceptance." What on earth is going on? Are these self-referential *descriptions of feeling* in any way a legitimate standard of scientific evaluation? Or has Bauer illegitimately elevated himself here into the most perfect arbiter of truth (according to how "comfortable" he is), as he had tried to present himself earlier as the most perfect scientist and the most perfect moral guide and the most perfect "student of science,"⁹⁶² not to mention the most perfect student of Velikovsky? Are we dealing with proper rational scientific procedure here, or with the most outright and blatant hubris? In my opinion, what we have here is megalomania—something is right not because it agrees with common sense, not because it agrees with tradition or science, but because it agrees with *Bauer*, and, more precisely, with his feelings. That's how objective Bauer really is, and he himself therefore stands accused, out of his own mouth, of the very sin which he attributed to Velikovsky, "of judging the validity of his interpretations by how satisfying they seemed to him." This charge has turned about, as have the two earlier ones, and he himself (as usual) is seen to be far guiltier of all three of them than Velikovsky. (To demolish Bauer is so easy that it is almost embarrassing, for he is so inept that he more-or-less invites his own destruction. It is like shooting fish in a barrel).

We must also remember that the Henry Bauer who dismisses all arguments for Velikovsky as incorrect is the same Henry Bauer who accepts arguments in favor of the Loch Ness Monster as correct. As a result, even though Velikovsky presents books full of data to support his theories, Bauer "knows" they are wrong: of that he is absolutely certain. On the contrary, although no valid proof whatever for the existence of the Monsters has been offered—Bauer himself admits "it is staggeringly implausible that these animals should exist"⁹⁶³—nevertheless Bauer declares "I do believe that Loch Ness Monsters are real animals."⁹⁶⁴ Of that he is also absolutely certain. "In Loch Ness there is"—[not "there might be," or "it is possible that there might be," but "there is"]—"a breeding population of large aquatic animals with powerful flippers, long thin necks, and bulky humped bodies."⁹⁶⁵ That is Bauer's science. So convinced is Bauer that the Monsters exist (and that Velikovsky is altogether wrong), that in his mind anyone who "lumps together Velikovsky and the Loch Ness Monster . . . loses credibility in my eyes."⁹⁶⁶

In the face of so self-blinded an approach to his pet hypothesis, we have to wonder whose credibility is ultimately lowered the most by this argument of Bauer's, for he blandly accuses all Velikovskians of believing anything that agrees with Velikovsky, even though (so he says), the scientific evidence for it may be lacking, yet he adamantly believes in Loch Ness Monsters, even though (as he himself blunderingly admits), these animals are "not as yet known to science."⁹⁶⁷ (They are apparently only known to Bauer). Faced with such a grossly unscientific attitude on the part of Bauer the scientist, we cannot avoid the decision that he himself is far more guilty than Velikovsky of seeing things only "through the lenses of his own conviction,"⁹⁶⁸ and therefore that his book, much more than anything written by Velikovsky, is "not science, not even a suggestion of science, but unfettered, undisciplined speculation,"⁹⁶⁹ yet this is the person who presents himself as the model scientist, the wholly objective observer, the only one ideally suited to assess Velikovsky. It is a joke.

In the end, therefore, we perceive that many of Bauer's accusations against Velikovsky (like all roads leading to Rome), rebound against himself. For example, when Bauer declares "It is surely reasonable to expect that

⁹⁶²Bauer, *op. cit.*, p. x.

⁹⁶³Bauer, *op. cit.*, p. 312.

⁹⁶⁴Bauer, *op. cit.*, p. 142.

⁹⁶⁵Bauer, *op. cit.*, p. 139.

⁹⁶⁶Bauer, *op. cit.*, pp. 138-139.

⁹⁶⁷Bauer, *op. cit.*, p. 139.

⁹⁶⁸Bauer, *op. cit.*, p. 198.

⁹⁶⁹Bauer, *op. cit.*, p. 202.

one who purports to discuss [Velikovsky] . . . should be familiar with these concepts," his book ironically indicates that he himself "does not have such familiarity."⁹⁷⁰ Or, he tries to prove that Velikovsky is wrong, and that the popular perception of science's guilt in the Velikovsky Affair is wrong, and that he alone has discovered the truth about the whole thing, but his book turns out to be merely a self-serving tract "replete with wishful thinking, uncritical commentary, preconceived notions."⁹⁷¹ What he does succeed in proving (contrary to his intention), is only that "he is ignorant of physical science but does not recognize that he is, and seeks to speak authoritatively in that field,"⁹⁷² and therefore that "he is not competent to carry on such discussions"⁹⁷³ because his "ignorance is evident."⁹⁷⁴ As a result, his book's "content is misleading or wrong or hand waving"⁹⁷⁵ and his ideas "are not worth taking seriously."⁹⁷⁶ (They are pinkie-waving.) We see therefore that Bauer is constantly his own best victim, and only on rare occasions does he (unknowingly) hit the truth about himself:

". . . we are all a little paranoid, certainly sure that we are right, unaware of the true extent of our ignorance" ⁹⁷⁷

As far as Velikovsky's ideas themselves are concerned, we will not discuss their validity here, of course, because our subject is the Velikovsky Affair. But there is something significant that must be said about the way Bauer has approached them. It is that Bauer is guilty here of a fundamental weakness in thinking—he cannot separate the specific hypothesis from the general theory, and fails to see that they are individual entities which fall or stand on their own merits. As a result, his entire effort is flawed from the start. For instance, Freud is generally to be seen as having proposed the existence and importance of the unconscious. Even if, therefore, his specific hypothesis about the role of repressed infant sexuality were to be rejected or modified (as many post-Freudians have done), this would have no effect upon the general theory, which might still turn out to be valid and very important on its own, a milestone in its field. Similarly with Marx, even if his specific predictions about how the class struggle would evolve and in what sort of nation it would be most likely to occur have been rejected or modified by many post-Marx Marxists, his general theory about the role of economic forces as a propellant and determinant of the course of history might still turn out to be valid on its own, a milestone in its field. It is the same, I feel, with the ideas of Velikovsky. That is to say, even if his specific concepts about Venerian and Martian episodes, and about the periods around -1500 and -700, are rejected or modified (as even Bauer perceives that several post-Velikovskians have done), his general theory about global catastrophes might still turn out to be valid on its own, and it would certainly be a milestone in geology, astronomy, psychology, and ancient history. It would be epochal. Bauer the scientist, however, the man who set out to thoroughly assess Velikovsky, has completely failed to understand this dimension of the matter, and it vitiates his entire book. As Ev Cochrane, editor of the journal *AEON*, wrote in his 1992 review of Bauer's book:

"More important, in the final analysis, is whether Velikovsky was right with regard to the central claims of *Worlds in Collision*, a question scarcely addressed by Bauer."⁹⁷⁸

⁹⁷⁰Bauer, *op. cit.*, p. 108.

⁹⁷¹Bauer, *op. cit.*, p. 318.

⁹⁷²Bauer, *op. cit.*, p. 152.

⁹⁷³Bauer, *op. cit.*, p. 121.

⁹⁷⁴Bauer, *op. cit.*, p. 198.

⁹⁷⁵Bauer, *op. cit.*, p. 285.

⁹⁷⁶Bauer, *op. cit.*, p. 133.

⁹⁷⁷Bauer, *op. cit.*, p. 173.

⁹⁷⁸*AEON*, II, 6, p. 118.

Not addressed because not perceived, and it is an extremely serious lacuna. Bauer writes as if he cannot see past his nose.

What Bauer should have done (had he been more interested in a fair appraisal than in elevating his status), may be gleaned as usual from Bauer's own book, where he admits

"Velikovsky rightly acknowledged that 'what I offered is primarily a reconstruction of events in the historical past. Thus, I did not set out to confront the existing views with a theory or hypothesis and develop it into a competing system. My work is first a reconstruction, not a theory.'⁹⁷⁹

Bauer recognizes this tentative dialogic nature of Velikovsky's book, and realizes that it was the reaction by science and the public and the commentators which "conspired to obfuscate that plain and important fact"⁹⁸⁰ and transform the matter into a bitter, shameful controversy. He does almost nothing with this insight, however, referring only marginally to Velikovsky's very important historical theories (his "reconstruction"), and almost not at all to the way in which the Velikovsky controversy (against Velikovsky's wishes) took on a sharp adversarial life of its own, which in my opinion is perhaps the most important characteristic of the Affair. The book is therefore unbalanced because of this tremendous gap. Had Bauer wanted to find the correct way to exploit his own insight, Cochrane shows him how in his review, where he pointedly quotes a passage from *Worlds in Collision*.

". . . it is also probable that in some traditions various elements from different ages are fused together. In the final analysis, however, it is not so essential to segregate definitively the records of single world catastrophes. More important, it seems, is to establish (1) that there were physical upheavals of a global character in historical times; (2) that these catastrophes were caused by extraterrestrial agents; and (3) that these agents can be identified."⁹⁸¹

The reader must be told that this passage appears in the Preface to *Worlds in Collision*, and was therefore written perhaps two years *before* any of the Affair had begun. It is therefore pristine, untainted by what happened after, and here we may find (as Bauer ought to have known), Velikovsky's true general theory, as opposed to his specific hypotheses. A number of his followers have clearly expressed their stance that, if the idea of vast, sudden and global catastrophe as here expressed were to turn out to be true (and the evidence for it is accumulating, as my last chapter indicates), this would make Velikovsky important for that reason, as the unconscious would Freud or economic determinism would Marx, despite any disagreements about specifics. The power of the whole, and the evidence offered for it, should overshadow the parts. That general dimension is what Bauer should have dealt with above all (had he done his research intelligently), for it is in my opinion the only honorable way to approach Velikovsky, but he did not, concentrating only on the specifics, and Bauer's book is therefore a dishonorable failure. He has looked at the trees, but failed to consider the forest. Far from going "beyond" Velikovsky, he has never fully gotten *to* Velikovsky.

Before we finish, a warning must be issued to the reader concerning the purpose of this chapter and of the whole book. We have found many inconsistencies and inaccuracies and misperceptions and misrepresentations and misinterpretations in Bauer, as well as much naivety and ignorance and foolishness and ineptitude and incompetence and self-contradiction. The point I wish to make, however, is this: even if we put all of these deficiencies of Bauer's together (and there are a great many of them, are there not?), they do not mean that Velikovsky is unanswerable (merely because Bauer could not do it), nor that Velikovsky must be right. That would be an incorrect assumption, and it is not the topic of this analysis. What they do mean, however, quite incontrovertibly, is that Bauer is completely, ludicrously and unanswerably wrong. To put it ironically in his own words, he offers "clear proof that .

⁹⁷⁹Ev Cochrane, "Beyond Bauer," *AEON*, *op. cit.*, p. 235.

⁹⁸⁰*Ibid.*

⁹⁸¹Ev Cochrane, *op. cit.*, p. 117.

. . [he] does not know what he is talking about," (even though his "ignorance is coupled with a readiness to discuss these subjects with an air of expertise"), and that he has made a considerable fool of himself throughout the book "by discussing, at ponderous length, a subject of which he is ignorant."⁹⁸² That alone has been Bauer's achievement. We may sum it up by saying that his research is incompetent, his deductions are incompetent and his methods are incompetent. He is, in all senses of the words, "an ignoramus masquerading as a sage," a person who pretends to expertise but exhibits none. Bauer tells us that he derived this great expertise from meetings of the Association for Rational Thinking. Perhaps he should have attended a few more sessions.

Here ends our trip through the convoluted tangle of Bauer's labyrinthine concoction. We have watched him continually try to pierce Velikovsky and shoot only his own foot. His book should not be called *Beyond Velikovsky* but *Beside Velikovsky*, or, better yet, *Falling Short of Velikovsky*. It is an example of monumental vainglory and astonishing ineptitude, displaying error, bungling and self-destruction. I am told, nevertheless, that a former follower of Velikovsky abandoned the man after reading Bauer, which he is said to have described as "a near-death experience." In my opinion, however, the reading of Bauer would be a near-death experience only if one's brain were near death to begin with. This is a pathetic book, and in it Bauer destroys mainly himself.

* * * * *

Skeptical Inquirer: Journalists and Scientists as Misrepresenters, By Charles Ginenthal

Galileo said,

"I shall say a sort of official policy on the part of those who want to cover up their original error of having wronged an innocent man by continuing their offenses and wrongs, so that people will conceive that other grave demerits, not made public, may exist to aggravate the guilt of the culprit . . .

"Hence when a man is wrongly condemned to punishment, it becomes necessary for his judges to use greater severity in order to cover up their own misapplication of law . . . "

"Galileo went on to say that if [the] frauds and stratagems that had been used at Rome in 1616 to impose upon the supreme authority could be revealed, the uprightness of his intentions would be clear. Since theologians *were* the supreme authority, the frauds and stratagems by which they had been imposed must have come from other men . . .
"⁹⁸³

Paul Kurtz, the publisher and editor of *The Skeptical Inquirer*, prior to founding that journal was the editor of *The Humanist*. It is important to understand how Kurtz, while at *The Humanist*, dealt with Velikovsky in order to fathom the stratagems, as publisher, he would perpetrate later in *The Skeptical Inquirer*. In November 1977, he offered

⁹⁸²*Ibid.*

⁹⁸³Stillman Drake, *Galileo*, (New York, 1980), pp. 92-93.

Velikovsky the opportunity to publish two papers, one of which had been presented in 1974 at an American Association for the Advancement of Science (AAAS), and another titled, "Preface" in *The Humanist*. Kurtz had led Velikovsky to believe that a few remarks critical of his work by Carl Sagan would also appear to give some balance to the presentation. What Kurtz did instead was publish Velikovsky's address to the AAAS as the first article, then 55 percent of Sagan's criticism second, and then without getting permission from Velikovsky, changed the title of "Preface" to "Afterword—1977" and printed it third. This arrangement made the three articles appear as an actual debate between Sagan and Velikovsky. In fact, Kurtz allowed Goldsmith to refer to the "Preface" as *Velikovsky's rejoinder to Sagan* to create the impression of an actual debate. Since many of Velikovsky's full responses to Sagan presented at the AAAS symposium were omitted from these articles in *The Humanist* by Kurtz, it was made to appear that Velikovsky had not and could not refute Sagan's attacks which, in fact, he had in the journal *KRONOS*, Vol. III, No. 2.

To make matters worse, Kurtz then inserted additional remarks by Donald Goldsmith at the end, which also criticized Velikovsky's remarks about the AAAS symposium, and presented further criticisms of Velikovsky's material which Velikovsky was not informed about nor to which he could respond.

This manipulative behavior by Kurtz is in reality a kind of "Star Chamber" proceeding. Velikovsky's responses to Sagan's criticism at the AAAS were not published. That is, Sagan's attacks were presented but Velikovsky was not given the opportunity to refute them in the very journal where his work was impugned. He was not shown what parts of Sagan's criticism Kurtz would publish so that he could answer them, and the entire presentation was a *fait accompli* before Velikovsky knew what had happened. Needless to say, this is a far cry from balanced or ethical journalism. Entrapment of this kind is hardly worthy of a journal which purports to be *The Humanist*.

Shortly thereafter Kurtz was fired as editor of that journal and started his own—*The Skeptical Inquirer*—to deal with what he perceives to be all forms of pseudoscience. At *The Skeptical Inquirer*, where Kurtz is the real boss, one will discover that Velikovsky's work will receive much worse. As the reader will become aware, Kurtz's approach to Velikovsky, in a journal where he runs the show, will be neither humanistic nor skeptical, but instead, a blatant attempt to destroy Velikovsky and his ideas with the most belligerent forms of journalistic misrepresentation and vilification.

According to Robert Anton Wilson, the late J. B. Priestly, novelist, playwright and essayist of Yorkshire, England, wrote about what he called "the Citadel," the scientific academic elite who are more commonly known as "the Establishment." Wilson, in his deft and engaging book, *The New Inquisition*, which is as apt a label as anyone may term "the Establishment," describes the Citadel, as personified by *The Skeptical Inquirer*, and the Committee for Scientific Investigation of Claims of the Paranormal, its parent organization, as follows:

"The Citadel has always been arrogant and intensely territorial. After all, it grew out of the science and philosophy of the 18th—19th centuries, and has inherited many qualities of that epoch, including anti-religious bias (the Citadel had to fight the Church to find its own place in the world) and also including tacit allegiance to the political powers that support and *feed* it . . . I am concerned with the *libertarian* objections to the Citadel—with the evidence of its increasing intolerance and inquisitorial attitude toward all old or new paradigms which conflict with its own favorite reality—tunnel.

"In casting a Swiftian eye on the modern Laputa, I am not advocating any specific old or new paradigm. I am merely advocating agnosticism and tolerance of dissent, for the usual reasons that political libertarians advocate those attitudes. As Lord Acton said, 'all power corrupts,' and I think the Citadel has acquired enough power and corruption to become, at times, as dangerous to open enquiry and free speculation as the Church ever was."⁹⁸⁴

⁹⁸⁴Robert Anton Wilson, *The New Inquisition, Irrational Rationalism and the Citadel of Science*, (Scottsdale, Ariz., 1987), p. 21.

Wilson goes on to describe the nature of individuals who contribute to *The Skeptical Inquirer* on page 45 thus:

"The Citadel has a vigorous and versatile propaganda department in the United States called the Committee for Scientific Investigation of Claims of the Paranormal or CSICOP for short. You will not be surprised to learn that Martin Gardner and Professor [Mario] Bunge are among its spokesmen." [On page 39 Wilson describes Gardner as follows:]

"Mr. Gardner has an infallible method of recognizing real science and of recognizing pseudo-science. Real science is what agrees with his idol and pseudo-science is what challenges that idol. Colin Wilson has written, 'I wish I could be as sure of anything as Martin Gardner is of everything.' Not all the Popes of the 20th century collectively have dared to issue as many absolute dogmas as Mr. Gardner; no man has had such superb faith in his own utter correctness since Oliver Cromwell." [On page 136, Wilson in his footnote states:]

"Martin Gardner['s] . . . mathematical games have delighted me almost as much as his dogmatism frightens me."

But how solidly based are Martin Gardner's insights into science as opposed to pseudoscience? Gardner's knowledge of pseudoscience is well expressed in his book, *Fads and Fallacies in the Name of Science*. In it on page 137, Gardner, arguing with Dr. Mortimer J. Adler regarding Adler's concept of the validity of evolution, discusses Neanderthal man whom Gardner describes by asking, "Where is one to place the dozens of well-preserved skeletons which have been found of Neanderthal man—a creature with a low forehead like an ape, a head hung forward, no chin, and non-opposable thumbs?" For all his understanding of science, Gardner's statement on each and every one of these characteristics of Neanderthal man turned out to be misguided and wrong!

1. Neanderthals turned out to have an opposable thumb. "[Jonathan] Musgrave was able to show that Neanderthals had neither short thumbs . . . —nor any particular limitations on manual dexterity."⁹⁸⁵

2. Neanderthals turned out to have chins, albeit weak ones.⁹⁸⁶ And it is interesting to note that,

"Samuel Laing, in his hugely popular *Human Origins*, published in 1895, conveys the casual racism of the time based on an effortless sense of European superiority: [He stated] 'The form of a chin seems to be wonderfully correlated with the general character and energy of the race. It is hard to say why, but as a matter of fact a weak chin generally denotes a weak, and a strong chin a strong race or individual.'

[The authors add] "What hope then for Neanderthals, the chinless wonders on the scale of values."⁹⁸⁷

3. Neanderthals had a low forehead but a larger brain size, on average, than modern man.

4. Where to place Neanderthals is answered by William Straus, Jr. and

⁹⁸⁵Erik Trinkhaus, Pat Shipman, *The Neandertals*, (New York, 1993), p. 356.

⁹⁸⁶Christopher Stringer, Clive Gamble, *In Search of the Neanderthals*, (New York, 1993), p. 26.

⁹⁸⁷*Ibid.*

A. J. E. Cave. "Notwithstanding, if he could be reincarnated and placed in a New York subway—provided that he were bathed, shaved, and dressed in modern clothing—it is doubtful whether he would attract any more attention than some of its other denizens."⁹⁸⁸ Martin Gardner, knowing the difference between science and pseudoscience, examined the evidence and made absolute pronouncements about the nature of Neanderthal man and got everything wrong, as did the rest of the experts he relied upon. As paleontologist, Leonard Krishtalka, points out, "When living man studies fossil man, ego and fancy can perfume the facts . . . [because] hominid fever blessed the marriage of shoddy evidence to wilful expectation."⁹⁸⁹ Krishtalka further shows that being fooled by hominid fever, as Gardner apparently is, is an old, old story.

"Edwin Ray Lankester, who, until 1907, was director of the British Museum of Natural History. Lankester [like Gardner] a staunch advocate of science over pseudoscience, often published vitriolic attacks on Britain's spiritualist movement."⁹⁹⁰

But Lankester, like Gardner, was fooled by his own belief that he could tell what the hominid bones showed about ancient man's precursors. Lankester also advocated that Piltdown Man, a scientific fraud, was the genuine article, just as Gardner proposes, and that he knows that his description of Neanderthal Man is the genuine article. Gardner, like Lankester, didn't know the fossil truth from the fossil pseudoscience. What Gardner did was present "ego and fancy to perfume the facts" and "married hominid fever to shoddy evidence and wilful expectation." His ability to tell what truths in science will stand the test of a few years of research proved to be baseless. Yet he is one of the leading contributors at *The Skeptical Inquirer*. What Gardner and his compatriots of CSICOP suffer from is an "audacious faith" in their infallibility of knowing true science from pseudoscience. As Gardner states,

". . . we can distinguish between the scientific value of Einstein's work and the contributions of a Velikovsky. We can grant that Einstein may be wrong, and there is a faint (*very* faint) possibility that Velikovsky may be right, but the extremes of the continuum are so great that we are justified in labeling one a scientist and the other a pseudo-scientist."⁹⁹¹

Notice how casually Gardner uses the authoritative "we" as in "we know." This is the same arrogance he expressed when he discussed Neanderthal man. Gardner-like views are also presented by Professor Bunge whose work Wilson claims he found in the Fall 1984 issue of *The Skeptical Inquirer* which

". . . implies that he already knows all the laws of the universe, or all the *important* ones; and that is . . . a huge and audacious faith.

"To you and me and the man and woman in the street it is now obvious that nobody in the past ever knew all the laws, or all the important laws; the scientists of 1904 were astounded by the discoveries of 1910; those of 1914 by the discoveries of 1920; etc. From this we have learned a certain mild agnosticism or open-mindedness; we are prepared to be startled by new discoveries. Professor Bunge is not so prepared; he knows in advance what is possible and what is not possible. Few theologians these

⁹⁸⁸Trinkhaus, Shipman, *op. cit.*, p. 303.

⁹⁸⁹Leonard Krishtalka, "Aromatic Man," *Dinosaur Plots*, (New York, 1989), p. 71.

⁹⁹⁰*Ibid.*

⁹⁹¹Martin Gardner, *Fads and Fallacies in the Name of Science*, (New York: Dover Pub., 1957), p. 50.

days dare to speak with that kind of dogmatic Authority. Professor Bunge's 'skepticism' has become a blind faith that he *knows* in 1984 what may and may not be proved in 1990."⁹⁹²

Yet interestingly, each of these paladins of *The Skeptical Inquirer*—Gardner and Bunge—are members of CSICOP which is opposed to fortune telling, astrology and all other forms of foretelling the future. Gardner and especially Bunge, however, know intuitively what will occur in the future of scientific discovery; Einstein will be proven correct, Velikovsky will be proven incorrect. They can foretell these kinds of things by examining their opinions and understanding of the evidence as Gardner did with the Neanderthals. Robert Anton Wilson calls this modern form of dogmatic self assurance "fundamental materialism." And it seems that instead of preaching as a "religious fundamentalist" by standing up in houses of worship and regaling the congregations that the future will be filled with fire and brimstone; these new "material fundamentalists" stand up before the public in print and tell the entire world that the future of scientific discovery will be filled with their own particularly marvelous forecasts. Of course, this is only scientific fortune telling. As Wilson points out, based on their superior understanding of science:

"CSICOP's method of "scientific investigation" generally is to wage a campaign of vilification, in the media, against any researcher whose ideas they don't like.

"What did I just say? That was polemical and unfair. I apologize. They are all honorable men."⁹⁹³

What was the cause of Wilson's condemnation of CSICOP?

In October of 1981, *Fate* magazine published an article by Dennis Rawlins titled "Starbaby." Rawlins is a Harvard University physics graduate specializing in celestial mechanics and an insider at CSICOP having been one of its co-founders in 1976 and a member of its Executive Council until 1979, until he got the boot. He also acted as an associate editor for *The Skeptical Inquirer*.

In 1977 Rawlins learned that one of the earliest analyses of astrology carried out by that journal was fraudulent. When he approached Professor Elizabeth Scott, a statistician at the University of California, about the techniques employed by CSICOP, which he called "bungling," she examined the evidence and came to the same conclusion. But this had no effect on those involved in debunking astrology at CSICOP. Why?

The Skeptical Inquirer had decided to investigate the predictions of neo-astrologist and French statistician Michael Gauguelin, whose astrological work dealt with athletes and their performance based on their astrological birth dates. Rawlins makes it quite clear that the aim of the researchers was to debunk Gauguelin's claims because CSICOP already knew such claims must be false. Gauguelin maintained that there was a phenomenon called "The Mars Effect" that he claimed influenced athletic ability and performance. Mars, in relation to the Earth, passes through twelve particular positions of the zodiac, of which Gauguelin believes two points are astrologically favorable for athletes born at these times. Since two of twelve positions are about 17 percent, if, astrologically, sport champions were shown to be born well above the 17 percent level in the two favorable positions, it could confirm the predictions of astrology. Gauguelin statistically found that, of the European sport champions, 22 percent of the sample were born in the favorable positions. The five percent difference of such a statistical analysis is quite a pronounced figure. It was highly unlikely that by using over 2,000 athletes, he would find this five percent variation from the expected 17 percent. It is in the millions to one chance of occurring by accident.

The investigators at CSICOP maintained that they could explain the difference by a concept called "the Mars dawn effect." If not only 22 percent of sport champions were born around dawn, then so too, 22 percent of non-

⁹⁹²Wilson, *op. cit.*, pp. 36-37.

⁹⁹³Wilson, *op. cit.*, p. 45.

champions were expected to be born at these same times. Therefore, Gauguelin's 22 percent was merely an expected outcome of the statistics. Evidently for some reason, women in labor are prone to give birth to children well after midnight in the early dawn—hence the Mars dawn effect would have proven astrology wrong. Case closed!

Unfortunately, the CSICOP investigators, to their consternation, did not find that the numbers they obtained exhibited this at all. Thus, what were they to do to debunk astrology? What they did, in the parlance of science, was "cull the data." They used 303 sport champions instead of 2,088 used by Gauguelin. But then they claimed that the smaller figure of 303 athletes which *they* employed was really statistically too small to prove the astrological concept. But even a moment's consideration illustrates they had simply finagled with their own numbers. If 303 sport champions is too small to prove the astrological claim of Gauguelin, then the 2,088 is a large enough sample to confirm the claim. In essence, the CSICOP investigators had actually given the astrological claim support!

Rawlins called this "bungling" and Professor Scott called it "misleading." Honest and honorable researchers would have presented their findings to the world through *The Skeptical Inquirer* and allowed the chips to fall where they may. But *The Skeptical Inquirer* would have none of this. Since they are "all honorable men," they decided to present their evidence in such a way to suggest just the opposite of what they had discovered. That is, they misrepresented the analysis with a straight face. Rawlins, in all innocence, attempted to have the matter corrected, believing he was dealing with reputable scientists and journalists, but found that these upholders of rationalism were not interested in what he had to say. He naively attempted to publish a letter on the matter but ran into a stone wall. The game now was not open to scientific discussion but became a political cover-up. Truth was dumped for face-saving propaganda.

This was clearly shown by the fact that when Rawlins presented a second analysis of the Mars effect that did, in fact, negate the astrological claims of Gauguelin, *The Skeptical Inquirer* grabbed it up and published it. However, they categorically shot down Rawlins' material in this second report which exposed and described what had existed in the first one. Rawlins demanded, at the very least, that they publish his statement that this part of his article had been censored, to which the editors acquiesced, but then censored that statement as well, without informing him of doing so. Thus they covered up their cover-up.

Rawlins finally demanded that a group of referees be brought into the dispute to judge the actions being taken by *The Skeptical Inquirer*. The editors then chose referees, but not an impartial team. Instead, they assembled one favorable to the editors. In spite of this, the evidence was so clear-cut that the referees had to affirm that the original report confirmed Gauguelin, it did not refute him, and it supported Rawlins' and Professor Scott's claim of being "misleading" or bungled. *The Skeptical Inquirer* then suppressed this finding and failed to publish the referees' statements of the facts.

Rawlins, still unwilling to damage the journal he had helped to found, tried to have his finding presented by CSICOP by speaking out at a press conference about the evidence; but again the executive council thwarted this move by halting the press gathering just prior to the time Rawlins was to speak. Rawlins had become the enemy and something now had to be done to thwart him. A secret closed-door meeting of the executive council was held which kicked Rawlins off the council but kept him on as associate editor. Rawlins tried vainly to have his findings published to save the journal from doing itself irreparable damage, but he finally resigned in 1980 disgusted and angry.

In essence *The Skeptical Inquirer* presented a patently dishonest report of the work of Gauguelin. They did everything they could to suppress Rawlins' exposure of their error and behavior; when their referees contradicted them about the evidence they suppressed the referees' work. Administrative means were used again and again to bury, cover-up, misrepresent and hide the facts from their readers. All this was done in the name of rationalism and good science!

Although I do not believe in astrology, I think Rawlins has made a cogent point about the behavior of the individuals at *The Skeptical Inquirer* respecting this affair.

"CSICOP's idea of internal scandal-preventing is not to eject the culprits but eject those who expose them. A Watergate analogy would be to throw Sam Ervin out of Congress and keep Nixon as President on his promise not-to-do-it-again.

"Everyone of the Councilors who say they know something about the "*sTARBABY*" [article] knows that it was a disaster. Yet *The Skeptical Inquirer* readers are given to believe that nothing went wrong."⁹⁹⁴

Later, when the evidence did come out, face-saving statements were presented, but fraud was denied. With this outrageous behavior as the model of its *modus operandi*, one wonders how Velikovsky will fare at the hands of these new guardians of science. Here too, Wilson has information for us about the way in which *The Skeptical Inquirer*, and particularly Martin Gardner, have decided to handle the work of Velikovsky.

"*Fate* September and October 1979, "The Crusade against the Paranormal" by Jerome Clark and J. Gordon Melton.

"Another founding member of CSICOP [like Dennis Rawlins] resigned or was ejected—accounts differ—but—

"Professor Marcello Truzzi, a sociologist from Eastern Michigan University, was editor of the CSICOP journal when it was called the *Zetetic* [which was changed to *The Skeptical Inquirer*]. He had a difference of opinion with the Executive Council about whether dissenting views should be published. He says CSICOP isn't skeptical at all in the true meaning of that word but is 'an advocacy body upholding orthodox establishment views.' In other words, their alleged skepticism has become, as my paradox suggests, just another dogmatic blind faith.

"Professor Truzzi has started his own journal, now called the *Zetetic Scholar*, in competition with CSICOP's journal, now called *The Skeptical Inquirer*. He follows the normal procedures of what is usually considered debate among sane people: he prints articles on both sides of every question and allows open debate, unlike *The Skeptical Inquirer*, which only prints articles on one side, since they already know the truth. Their fury against him is what any student of priesthood would expect."⁹⁹⁵

What is most unfortunate, respecting this revelation, is that suppression of Velikovsky's side in these kinds of debates is not isolated and restricted to *The Skeptical Inquirer* alone. Certain journals and magazines of the scientific establishment also pursue this same policy.

Typical of this are the following examples:

The journal *Mercury*, an astronomy journal, is published on the west coast of the United States. After running endorsements for several publications critical of Velikovsky's theories and actually describing Velikovsky's theory as "wacky" in the September/October 1990 issue, page 164, the editors refused to publish an ad for a book titled *Carl Sagan and Immanuel Velikovsky*, which answered many of these criticisms. That is, the editors at *Mercury* apparently felt it was journalistically ethical to promote critical materials published after *The Velikovsky Affair* of 1963 and characterized Velikovsky's theories as "wacky," but felt that they should not even allow an ad to appear in its pages for a book presenting the opposite viewpoint.

⁹⁹⁴Dennis Rawlins, "Starbaby," *The Velikovskian*, Vol. II, No. 1, (1994) p. 47; see also, *Fate*, (October 1981), p. 49.

⁹⁹⁵Wilson, *op. cit.*, pp. 47-48.

It is fascinating to observe the absurd contortions to which its editors resorted to explain their actions. Their editors claimed that they could not publish an ad for a book that had not been peer reviewed.⁹⁹⁶ However, when the author sent the peer reviews as proof the book had been properly refereed, the peer review process was changed to "anonymous scientific [peer] reviewers chosen by the publisher and editors, not by the author."⁹⁹⁷ When the author, Charles Ginenthal, sent *Mercury* a copy of a letter by Professor William Stiebing, Jr. stating that his book, critical of Velikovsky's work, and promoted in their journal *had never been peer reviewed*, but which they described as a "responsible book by a historian examining some of the 'fiction science' of mankind's past including the works of Velikovsky,"⁹⁹⁸ they replied, "the editorial board has decided not to accept your ad for publication . . . This decision is final. [But they still claimed] Peer reviewed books . . . [is] an important issue," and went on to say, "We consider whether the book is about astronomy and science or not. We tend not to publish ads that promote books or anything else that is not about astronomy and science in general."⁹⁹⁹ This was written in spite of Stiebing's letter which that journal possessed which admitted his "book was a work of history and archaeology, not science."¹⁰⁰⁰

This is the general way in which many of the scientific journals and publications have responded to Velikovsky. For example, in 1978 two pro-Velikovsky books were to be advertised in *Science News*. According to Lewis M. Greenberg,

"Early this year, *KRONOS* Press and *LAR* Research reserved space for six one-inch ads in *Science News*; the purpose was to advertise *Velikovsky and Establishment Science* and *The Age of Velikovsky* [pro-Velikovsky books]. The ads were accepted and the two books were advertised alternately for each week of the month of February. Then quite abruptly, *Science News* unilaterally terminated the ads after they had run for only a third of their allotted time. Written notice of the cancellation was sent to the Angelyn Advertising Agency which had placed the ads. That letter is here reproduced:

Science News

The Weekly Summary of Science

February 28, 1978

"Dear Mr. Helton [Angelyn Advertising Agency],

"As you know I received your insertion order scheduling space for *LAR* Research and *Kronos* Press in various issues of *Science News*.

"A few of these ads have run. However, at a recent advertising review committee meeting, the Editor and Publisher reversed their original decision to accept the "Velikovsky" ads. Therefore, I was informed to notify you that ads on that subject will no longer be accepted by *Science News*.

Cordially,

(Signed)

Fred W. Dieffenbach

Sales Director

⁹⁹⁶"Scientists, Journalists And Editors As Suppressors (Part II)," *The Velikovskian*, Vol. II, No. 2, (1994), p. 90.

⁹⁹⁷*Ibid.*, p. 98.

⁹⁹⁸*Mercury*, (May/June 1985), p. 92.

⁹⁹⁹"Scientists, Journalists And Editors . . .," *The Velikovskian*, *op. cit.*, p. 101.

¹⁰⁰⁰*Ibid.*, p. 99.

"To date, all effort to persuade *Science News* to recant its censorship have failed. We can only wonder, therefore, at the extreme insecurity of the scientific community. If Sagan, *et al.*, are so right and Velikovsky so wrong, why were two book ads deemed so threatening that they had to be exorcised from the pages of *Science News*."¹⁰⁰¹

But more typical of *The Skeptical Inquirer's* policy of attacking a man but not allowing rebuttal articles to appear is that of the *Biblical Archeology Review*. In an article critical of Velikovsky's historical restructure, William H. Stiebing, Jr. presented several points.¹⁰⁰²

In late 1978, Dr. John J. Bimson and Peter James sent a scholarly joint rebuttal to Stiebing's critique. But the *Biblical Archeology Review* would have nothing to do with this response and rejected it since they said it was "out of date." After allowing its pages to be used to attack Velikovsky's historical evidence, its editors felt it inappropriate to permit the opposing position to be aired.¹⁰⁰³

What was the reason *The Skeptical Inquirer* embarked on a policy of suppression of Velikovsky's evidence? According to Wilson,

"Metamagical Themas by Douglas Hofstadter pp. 111–113;

"Hofstadter, a good friend and admirer of CSICOP's Super-star Martin Gardner, gives his version of Prof. Truzzi's exodus. Truzzi wanted to publish articles on both sides of the Velikovsky controversy . . . Gardner held that this would give Velikovsky 'undeserved legitimacy' and insisted on the one-sided attitude which now prevails in CSICOP. (As a libertarian, I must admit that it is incomprehensible to me. To attack a man's ideas, and then refuse to let him, or his defenders, answer the attack seems Idolatrous if not fascist. To say that *after attacking him*, allowing rebuttal gives him 'undeserved legitimacy' is a rationalization that I think, only the most Faithful can believe, or even discuss with a straight face.) Hofstadter defends Gardner as well as he can, calling Velikovsky 'obnoxious,' but ends up admitting that he personally would prefer open debate. He still supports CSICOP, however.

"CSICOP, meanwhile, isn't listening to Hofstadter. They still won't allow open debate in their journal. The heretical Jeffersonian view that even the 'obnoxious' have the right to be heard hasn't percolated their craniums yet."¹⁰⁰⁴

Truzzi states:

"I do not believe in most paranormal claims, but I refuse to close the door on discussion of them. The simple fact is that I think I have more confidence in science than, say Martin Gardner does. For example, Martin resigned as consulting editor for *Zetetic Scholar* when he was told that I planned to publish a 'Stimulus' article asking for a

¹⁰⁰¹Lewis M. Greenberg, "Editorial Statement," *KRONOS*, Vol. III, No. 4, (Summer 1978), p. 2.

¹⁰⁰²William H. Stiebing, Jr., *Biblical Archeology Review*, (December 1976).

¹⁰⁰³*KRONOS*, Vol. VII, No. 3, (Spring 1982), p. 71.

¹⁰⁰⁴Wilson, *op. cit.*, p. 47.

reconsideration of the views of Velikovsky Martin was invited to comment as were many critics [pro and con] of Velikovsky. But Martin felt that even considering Velikovsky seriously in the *Zetetic Scholar* gave Velikovsky undeserved legitimacy so Martin resigned."¹⁰⁰⁵

It appears to be quite clear that when Velikovsky's ideas and responses to criticism of him are suppressed, what we have in reality is a modern scientific Index like that created by the Inquisition. When Velikovsky's ideas and responses to negative criticism are suppressed by *The Skeptical Inquiries*, *Mercury*, *Science News* and the *Biblical Archeology Review*, a pattern of intellectual suppression is suggested, and indicates that, with Velikovsky, the game of journalism is rigged to promote and support his detractors and to suppress his defenders. And all of this is being carried out in the name of science and rationality by editors, journalists and scientists.

But Gardner also plays the "I believe in open debate" card when he states,

"In the last analysis, the best means of combating the spread of pseudo-science is an enlightened public, able to distinguish the work of a reputable investigator from the work of the incompetent and self-deluded."¹⁰⁰⁶

Torquamada, I am sure, must have thought similarly. What I believe Gardner really means by an "enlightened public" is one that is only allowed to get its information, as in the Soviet Union, through the official publication called *Pravda*—meaning "Truth" in Russian. But *Pravda*, in Gardner's terms, can only be supported by not allowing "open discussion" of both sides of ideas in the journals with which he associates. If anyone thinks that I am not giving an objective analysis of Gardner's outlook, then I recommend them to this statement by him:

"After the first three issues, edited by sociologist Marcello Truzzi, it became apparent that deep philosophical differences separated what Truzzi wanted the magazine to be from the desires of the other committee members. We wanted a magazine of open advocacy, one that would take a firm stand against the more preposterous forms of modern pseudoscience. Truzzi believed that even extreme cranks like Velikovsky should be treated with respect. He wanted the magazine to establish dialogues between skeptics and true believers, to present both sides of current controversies. In brief, he wanted a magazine with an objective, neutral tone, in contrast to what he derided as mere 'debunking'."¹⁰⁰⁷

What Truzzi wanted was to permit the persons being attacked the right to answer that attack in *The Skeptical Inquirer*. It was never a matter of tone, but of simple justice which Gardner and his associates will not allow. Because of this policy of attacking a man but suppressing his or his supporters' answers, I see these individuals as behaving like intellectual and scientific totalitarians and fascists. The evidence presented below will make this accusation abundantly clear. Now what do scientists say about such suppression? Ironically, Wilson shows in his

¹⁰⁰⁵Douglas Hofstadter, *Metamagical Themas*, (New York, 1985), p. 111.

¹⁰⁰⁶Gardner, *op. cit.*, p. 7.

¹⁰⁰⁷Martin Gardner, *Order and Surprise*, (Buffalo, N.Y., 1983), p. 213.

"May 1980, Conference of the American Association for the Advancement of Science, paper on 'Pathological Science' by Dr. Ray Hyman of Washington State University. He talks at length about what he calls the 'conspiracy' to suppress Dr. Velikovsky's books. He says the conspiracy was 'more pathological' than Dr. Velikovsky's heresies.

"Incidentally, Dr. Hyman is a member of CSICOP"1008

At that same meeting, Wilson elsewhere informs us: "The way to determine truth, Hyman went on . . . is to allow all viewpoints to be discussed."¹⁰⁰⁹

On the other hand, Martin Gardner apparently feels the suppression by the scientific community of Velikovsky's books is not "pathological." He feels that "the scientists who threatened to boycott the firm's textbooks unless it dropped Velikovsky from its list, were exercising their democratic privilege of organized protest. The issue is not a legal one, or even a political one. It is a question of individual responsibility."¹⁰¹⁰

The difference between protest and censorship is apparently lost on Gardner, as I fear is the difference between open debate and suppression. The question that naturally follows is, did *The Skeptical Inquirer* treat Velikovsky's evidence differently than that of Gauguelin? Here I suggest that the treatment of Velikovsky's work was far worse as will be documented below. Given this introduction to the philosophy of *The Skeptical Inquirer*, the reader will discover that, in the pages of that journal, the meanest and most disreputable forms of journalism have been perpetrated again and again against Velikovsky; and it is only now that the other side will expose the depths to which Martin Gardner and Paul Kurtz's excesses have lead.

* * * * *

JAMES E. OBERG AND THE BIG ERASER

"With just enough learning to misquote"¹⁰¹¹

James E. Oberg's criticism of Velikovsky's concepts and predictions appeared in the Fall 1980 issue of *The Skeptical Inquirer*. Oberg is a well-known, highly respected science writer whose articles have appeared in *Astronomy* and *Omni Magazine*. He has written four books on Soviet space science, as well as others on science, and is a space operations specialist at NASA's Mission Control in Houston, Texas. In his criticism Oberg states:

¹⁰⁰⁸Wilson, *op. cit.*, p. 69.

¹⁰⁰⁹Robert Anton Wilson, *Right Where You Are Sitting Now*, (Berkeley, Calif., 1992), p. 74.

¹⁰¹⁰Gardner, *op. cit.*, p. 5.

¹⁰¹¹Lord Byron, *English Bards and Scotch Reviewers*, (1809), line 66.

"In fact, Velikovsky *did* have his antecedents, however much his followers refused to face up to such previous analogues. In three separate cases, lengthy books had chronicled ancient legends as proof of interplanetary catastrophes, often with uncanny parallels to Velikovsky's reconstructions. Yet, although Velikovsky must have read these books (they were available at the Columbia library) and used them as source materials, NO MENTION OF THEM HAS EVER BEEN MADE IN HIS WRITINGS—perhaps because their authors have been totally discredited as crackpots. But they have not been forgotten by many of Velikovsky's critics, who saw him as another spiritual successor to previous crank theorists. (Capitalization added)

"The first was William Whiston, a British clergyman and mathematician, who in 1696 published *New Theory of the Earth*. In it, Whiston claimed that before Earth's disastrous encounter with a comet, there were exactly 360 days in a year and 30 days in a month (Velikovsky claimed this as well). The comet came by in 2349 B.C., causing the Noachian flood and the changing of the earth's orbit. Whiston found extensive evidence to support this theory, based on worldwide legends.

"In 1882, the American politician–reformer–editor–novelist–crank Ignatius Donnelly published *Ragnarok*, in which he used 200 pages of widely circulated myths to reconstruct a story of a comet causing a worldwide catastrophe, including the sun's standing still over Gibeon (Velikovsky also accounted for this by means of the passage of a comet)

"The third recycling of this extraterrestrial catastrophe concept was the Hoerbiger–Bellamy "World Ice Theory." First published in Germany . . . Bellamy books . . . did not have such a taint and were pure crackpottery, telling how a former Moon of Earth had crashed into the planet some 13,500 years ago, being soon afterwards replaced by the current Moon (which Velikovsky agreed was indeed captured at about that time)

". . . [H]is ideas differed from those of Whiston, Donnelly, Hoerbiger, and Bellamy. But he [Velikovsky] had good reason, as we have seen, for portraying himself as or the *originator* of these theories and for neglecting to draw attention to his predecessors."¹⁰¹²

Oberg has told us "that no mention of them [Whiston, Donnelly or Hoerbiger–Bellamy] has ever been made in his [Velikovsky's] writings, although Velikovsky used them as source materials." Is there any truth to Oberg's assertions and condemnation of Velikovsky for this alleged omission? The answer is that there is not one scintilla of truth in Oberg's remarks on this matter. In Velikovsky's book *Worlds in Collision*, he discusses Whiston's work fully in three instances informing the readers who really read the book of the catastrophist concepts of Whiston. Velikovsky specifically states,

". . . I found that W. Whiston, Newton's successor at Cambridge and a contemporary of Halley, in his *New Theory of the Earth* (the first edition of which appeared in 1696) tried to prove that the comet of 1680, to which he (erroneously) ascribed a period of 575½ years, caused the biblical Deluge on an early encounter."¹⁰¹³

¹⁰¹²James E. Oberg, "The Velikovsky Affair," *The Skeptical Inquirer* (Fall 1980), pp. 22-23.

¹⁰¹³Immanuel Velikovsky, *Worlds in Collision*, (New York, 1950), p. 42 (in the footnote).

"W. Whiston, in *New Theory of the Earth*, (1696), expressed his belief that before the Deluge the year was composed of 360 days. He found references in classic authors to a year of 360 days, and as he recognized only one major catastrophe, the Deluge, he related these references to the antediluvian era."¹⁰¹⁴

"W. Whiston wrote in his *New Theory of the Earth*, [6th ed., (1755), pp. 19–21] concerning the wonder of the sun standing still:

"'The Scripture did not intend to teach men philosophy, or accommodate itself to the true and Pythagoric system of the world.' [And again:] 'The prophets and holy penmen themselves . . . being seldom or never philosophers, were not capable of representing these things otherwise than they, with the vulgar, understood them.'¹⁰¹⁵

In complete contradiction to what Oberg has told us regarding Velikovsky's writing making no mention of William Whiston, one can see that Oberg has misrepresented the facts. Velikovsky indeed discussed Whiston directly and forthrightly in *Worlds in Collision*. Why did Oberg misrepresent Velikovsky claiming he never mentions Whiston?

What then of Donnelly? Did Velikovsky also fail to mention Donnelly as Oberg claims? Here also, in complete contradiction to Oberg, Velikovsky writes in *Worlds in Collision*,

"I. Donnelly, author, reformer, and member of the United States House of Representatives, tried in his book *Ragnarok* (1883) to explain the presence of till and gravel on the rock substratum in America and Europe by hypothesizing an encounter with a comet, which rained till on the terrestrial hemisphere facing it at that moment. He placed the event in an indefinite period, but at a time when man already populated the earth. Donnelly did not show any awareness that Whiston was his predecessor. His assumption that there is till only in one half of the Earth is arbitrary and wrong."¹⁰¹⁶

Once again one can see Oberg totally misrepresented the facts when he claimed Velikovsky never mentions Donnelly. Again, why did he do such a thing?

What then of Hoerbiger–Bellamy? Can one trust Oberg's statement that Velikovsky never mentions them in his writing? With respect to Hoerbiger and Bellamy Velikovsky writes in *Earth in Upheaval*,

"Not so long ago an explanation of the mystery of Lake Titicaca and of the fortress Tiahuanacu on its shore was put forward in the light of Hoerbiger's theory. A moon circled very close to the Earth, pulling the waters of the oceans toward the equator; by its gravitational pull, the moon held, day and night, the water of the ocean at the altitude of Tiahuanacu: 'The level of the ocean must have been at least 13,000 feet higher.'¹⁰¹⁷ Then the Moon crashed into the earth and the oceans receded to the poles,

¹⁰¹⁴*Ibid.*, p. 330, fn. 1.

¹⁰¹⁵*Ibid.*, pp. 39-40, fn. 2.

¹⁰¹⁶*Ibid.*, p. 42 (in the footnotes).

¹⁰¹⁷H. S. Bellamy, *Built before the Flood: The Problem of the Tiahuanacu Ruins*, (1947), p. 14.

leaving the island with its megalithic city as a mountain above the sea bottom, now the continent of the tropical and subtropical Americas. All this happened millions of years before our moon was caught by the earth, and thus the ruins of the megalithic city Tiahuanacu are millions of years old, that is, the city must have been built long 'before the Flood.'"

"The theory is bizarre."¹⁰¹⁸

Again Oberg is caught with his proverbial pants down with respect to his misrepresented claim that Velikovsky never mentions Hoerbiger–Bellamy in any of his writings. In each and every instance regarding this question Oberg has misrepresented and distorted the facts.

Velikovsky presented these precursors in his work; he presented their books, and he presented the very same information about their theories from their books that was presented by Oberg as criticism. It is as if Oberg read Velikovsky's statements about these precursors and then used this information to attack Velikovsky. Of course, this is not known, but there can be no doubt that there is no truth to Oberg's accusations regarding this question.

Furthermore, Oberg has gone on to say, "Velikovsky did have his antecedents however much his followers refuse to face up to such analogues." But again, if Oberg had done his homework he would know that this is also untrue. Livio Stecchini, a supporter of Velikovsky in the book, *The Velikovsky Affair*, published in the 1960's, many years before Oberg wrote, states:

"*One of the precursors of Velikovsky as to the general thesis of the catastrophic past of the Earth, to whom he refers in his work, was William Whiston (1667–1752) . . . Whiston . . . submitted to his master the manuscript of a book entitled *New Theory of the Earth* . . . This book contended that the cataclysm described in the Old Testament as universal Deluge was caused by the impact of a comet at the end of the third millennium B.C., and that up to the Deluge the solar year had the duration of 360 days only, yet the new calendar of 365 days had to wait to be introduced by Narbonassar (in 747 B.C.). These contentions were based mainly on historical evidence . . .*"¹⁰¹⁹
(Emphasis added)

Stecchini then goes on for eight more pages describing Whiston's work and how it was greeted. In addition, David Stove, a follower of Velikovsky, also wrote in 1976 that Isaac Newton had a "terrible falling out with his former protege, Whiston, who ascribed the Noachian deluge to the close approach of a huge comet."¹⁰²⁰ But Oberg tells us Velikovsky's "followers refuse to face up to such analogues."

Kendrick Frazier, the Editor–In–Chief at *The Skeptical Inquirer*, never took a moment to check these misrepresentations which he helped to publish. But in his own criticism he cites scientists who say Velikovsky's books "contain more falsehoods in a paragraph than one can refute in a chapter."¹⁰²¹ But how does Frazier defend these obvious misrepresentations appearing in *his* journal dedicated to rationality and the advancement of science over pseudoscience? And by not owning up to the fact that Velikovsky was clearly misrepresented by Oberg in that journal,

¹⁰¹⁸Immanuel Velikovsky, *Earth in Upheaval*, (New York, 1955), pp. 83-84.

¹⁰¹⁹Livio Stecchini, "The Inconstant Heavens," *The Velikovsky Affair*, ed. A. de Grazia, R. E. Juergens, Livio Stecchini, (New Hyde Park, N.Y., 1966), pp. 91-92.

¹⁰²⁰David Stove, "The Scientific Mafia," *Velikovsky Reconsidered* (New York, 1976), p. 12.

¹⁰²¹Kendrick Frazier "The Distortions Continue," *The Skeptical Inquirer*, (Fall 1980), p. 34.

one is led to wonder who is continuing to present "distortions," Velikovsky or the science writer, Oberg, and the editor-in-chief, Kendrick Frazier, or publisher, Paul Kurtz?

If all of this were not enough, Lewis M. Greenberg, published a letter on July 13, 1976 sent to *The Humanist*, while Paul Kurtz was then its editor, four years prior to Oberg's accusation. In that letter, published subsequently in *KRONOS*, Greenberg attacked L. Sprague de Camp for failing to be aware that "WILLIAM WHISTON (as readers of *Worlds in Collision* know) had published such a late theory in the *seventeenth* century . . ." ¹⁰²² (Capitalization added) Since the letter was sent to Kurtz, he had to know that Velikovsky's supporters were willing to acknowledge and publish material in his journal which claimed that William Whiston was one of Velikovsky's precursors. As the publisher of *The Skeptical Inquirer*, Kurtz knowing this still did nothing to remove Oberg's false accusation from his journal. He simply allowed this misrepresentation to appear in the pages of *The Skeptical Inquirer* without so much as a hint that he was privy to prior *published* correspondence which contradicted Oberg's assaults.

Finally, although Oberg, Frazier and Kurtz could not have known it at the time this criticism of Velikovsky was presented, Professor James E. Force, science historian at the University of Kentucky, clearly a non-Velikovskian, specifically stated in his book, *William Whiston Honest Newtonian*, "Velikovsky acknowledges Whiston's influence on his controversial reconstruction of world history in his *Worlds in Collision* (London Abacus, 1974), p 53n and p 316n. This work first appeared in 1950. Velikovsky is glad for Whiston's support of his theory of a cometary impact upon earth in ancient times and of the consequences of this impact upon the length of the year." ¹⁰²³

In the title of Professor Force's book is the word, "Honest." One wonders if critic Oberg, editor-in-chief Frazier, or publisher Kurtz will ever acknowledge that a science historian has stated that Velikovsky acknowledges Whiston as a precursor. This "Honest" evidence regarding Velikovsky's precursors will probably never be presented in *The Skeptical Inquirer* for their readers. Therefore, one must ask, in view of their behavior, do these men know what the word "honest" means?

Now, this misrepresentation might have been forgivable if the material was difficult to locate, but it simply was not. All Oberg, Frazier or Kurtz had to do to find out whether Velikovsky and his supporters had discussed these precursors and their theories was to read Velikovsky's books and *The Velikovsky Affair* or, at the very least, look for Donnelly, Whiston, and Hoerbiger-Bellamy's names in the indexes of these books, which was all I needed to do to refresh my recollection about these precursors. Apparently either Oberg did not read these books with this material to criticize Velikovsky and his supporters, or worse, had read this material but omitted this information. And what about Frazier and Kurtz, surely they could have availed themselves of this information? Kurtz already had this information!

Oberg, then satisfied with having credited himself with this critical analysis, goes on to suggest with respect to another concept, "Skeptics have suggested that the person most adept at erasing (for changing the meaning of) sentences in Velikovsky's books is Velikovsky himself." ¹⁰²⁴ I find this self-serving disregard of the actual facts on this matter by Oberg quite unique. It is the height of hypocrisy to suggest Velikovsky never mentions his precursors when, in fact, he did so, and then chastise Velikovsky for the error, or more accurately, for the misrepresentations which Oberg himself perpetrated. Oberg offering that "the most adept person at erasing (for changing the meaning of sentences) in Velikovsky's books is Velikovsky himself" is in reality presenting an open and shut case against Oberg's own credibility with such an inadequate attempt not at criticism, but at smear and propaganda.

Alexander Kohn in his fine book of fraud and error in science, *False Prophets*, has a chapter titled "Criticism or Slander." In it he addresses the type of criticism that has just been exposed with regard to Oberg:

"During the past few years certain incidents have been brought to the attention of the scientific community and the general public involving scientists who have been accused of misconduct, be it propagation of a falsehood . . . , but against whom

¹⁰²²Lewis M. Greenberg, "L. Sprague De Camp: Anatomy of a Zetetic," "To the Editor of *The Humanist*," *KRONOS*, Vol. III, No. 1, (Fall 1977), p. 46.

¹⁰²³James E. Force, *William Whiston Honest Newtonian*, (Cambridge, Eng., 1985), p. 169.

¹⁰²⁴Oberg, *op. cit.*, p. 25.

evidence for such misconduct has not been sufficient for condemnation. In such cases, publication of the accusation, be it in scientific journals or in the mass media, before they have been substantiated can do irreparable harm to the reputation of the scientist in question. This is particularly so when accusations are raised against a scientist who is no longer alive and cannot refute inaccurate statements."¹⁰²⁵

Velikovsky was in his grave about a year when Oberg presented these misrepresentations in *The Skeptical Inquirer*. According to Kohn, this means of attack is not criticism but merely the propagation of slander and falsehood to do irreparable harm.

But Oberg's focus is upon Velikovsky's predictions wherein he states that,

". . . many of his [Velikovsky's] most famous predictions were allegedly based on reinterpretations of what he had originally written, which turned out to be false or were ambiguous enough to be reinterpreted to mean practically anything. Specific examples seem to bear out his criticism.

"Take Venus, for example: Velikovsky predicted it would be giving off heat because of its short, violent history. Although it turned out to be hotter than scientists had guessed (mainly due to the unpredictably massive atmosphere), *it is not giving off more heat than it gets from the sun, nor is it cooling off* (as Velikovsky also predicted). On both counts, Velikovsky's predictions were in error. He also vigorously denounced the 'runaway greenhouse theory' for the high surface temperature, asserting repeatedly that such a mechanism was impossible—if it was possible (as the latest space probes indicate), then his major prediction would have been blown away."¹⁰²⁶ (Emphasis added)

Elsewhere, Oberg states:

"*Actually, the heart of Velikovsky's prediction was that Venus would be giving off more heat than it receives from the sun and that it would still be cooling off.* Space probes and recent terrestrial measurements have both provided voluminous evidence *against* these claims, and the Pioneer–Venus data in 1978 showed that the 'runaway greenhouse effect' is still very much in the running."¹⁰²⁷ (Emphasis added)

Oberg has made it abundantly clear in *The Skeptical Inquirer* that Venus "is not giving off more heat than it gets from the sun." He further claims "voluminous evidence" to support this view has been provided by the data of "space probes" to the planet. What we have is Oberg's statement backed up by not a single reference to the scientific literature. Because Oberg is a highly respected science writer, he apparently feels he does not have to provide evidence for this assertion other than his own word. The question can only be resolved by finding out what the space probes to Venus actually found in 1978 and prior to Oberg's presentation in 1980.

¹⁰²⁵Alexander Kohn, "Criticism or Slander," *False Prophets*, (New York, 1986), p. 117.

¹⁰²⁶Oberg, *ibid.*

¹⁰²⁷James E. Oberg, "How Good Were Velikovsky's Space and Planetary Space Predictions, Really?" *The Universe and Its Origins*, ed. S. Fred Singer, (New York, 1990), p. 39.

In this respect, the University of Arizona compiled all this 1970's data in a monograph review of the Pioneer Venus and Venera evidence which in part dealt with these 1970's findings. Ronald Greeley calls this book titled *Venus*, "an excellent collection of papers on all aspects of Venus known" ¹⁰²⁸ The data used to establish whether or not Venus is giving off more heat were measured by *Pioneer Venus* in December 1978 to 1979 and by *Veneras 9, 10, 11 and 12*, between October 1975 and December 1978. ¹⁰²⁹ That is, all this voluminous data was available before Oberg made his unsubstantiated claim! What did these space probes to Venus actually show? There were three sets of measurements made by *Venera* and *Pioneer Venus* probes to determine whether or not Venus is giving off more heat than it receives from the sun. If Oberg's contention is correct that Venus is not giving off more heat than it receives from the sun, the readings by the probes will show thermal balance; that is, the amount of sunlight measured entering Venus' atmosphere will be equivalent to or in balance with the amount of infrared radiation or heat emitted at that altitude in Venus' atmosphere; it will show thermal balance. If Velikovsky's contention is correct, then the readings by the probes will show thermal imbalance; the planet itself will be emitting more heat than can be provided by sunlight at all levels.

Three sets of measurements were carried out: one set at the cloud tops; a second set between the cloud tops and down to about seven miles above the surface by four *Pioneer Venus* probes. The third set was carried out by *Veneras 9, 10, 11 and 12* probes at the surface. What did each set of measurements show with respect to thermal balance touted by Oberg, or thermal imbalance offered by Velikovsky?

F. W. Taylor, *et al.*, in discussing four of the best measurements at the Venus cloud tops by probes, found they showed thermal imbalance. "Clearly, the Pioneer measurements of emission and reflection are not consistent with each other if net radiative balance applies," ¹⁰³⁰ they wrote. What Taylor and his co-investigators did was reject all the readings that showed thermal imbalance stating they did this "WITH THE ASSUMPTION OF GLOBAL ENERGY BALANCE . . . THIS IS THE MOST PROBABLE VALUE." ¹⁰³¹ (Capitalization added)

If we throw out all the readings and measurements which contradict thermal energy balance at the cloud tops on the assumption that Venus *must not* show this, then, in a cockeyed sense of rationality, Venus is in thermal balance. But what about the second set of readings made by the four *Pioneer Venus* probes below the clouds? What did they find? Did they support Oberg's assertion of thermal balance or Velikovsky's theory of thermal imbalance? Again Oberg's views are totally contradicted by the data. According to Richard A. Kerr, the editor of the journal, *Science*,

"When *Pioneer Venus* probes looked at the temperature, *each one found more energy being radiated up from the lower atmosphere than enters it as sunlight . . .*" ¹⁰³²
(Emphasis added)

The book, *Pioneer Venus*, an official publication of NASA, reinforces this evidence of thermal imbalance found by the four *Pioneer Venus* probes.

"The measured, infrared [heat] fluxes [upward from Venus] show several anomalies, the origin of which is still being debated. *Taken at face value the anomalies suggest that parts of the atmosphere are transmitting about twice the energy*

¹⁰²⁸Ronald Greeley, *Planetary Landscapes*, 2 ed., (New York, 1994), p. 134.

¹⁰²⁹*Ibid.*, p. 135.

¹⁰³⁰F. W. Taylor, *et al.*, "The Thermal Balance of the Middle and Upper Atmosphere of Venus," *Venus*, ed. Donald M. Hunten, Lawrence Colin, Thomas M. Donahue, V. I. Moroz, (Tucson, 1983), (Univ. of Arizona), p. 658.

¹⁰³¹*Ibid.*

¹⁰³²Richard A. Kerr, "Venus: Not Simple or Familiar, but Interesting," *Science*, Vol. 207, (1980), p. 289.

*upward than is available from solar radiation at the same level.*¹⁰³³ (Emphasis added)

What then of the four Venera probes that measured this data at the surface of Venus? Did these at least find thermal balance as Oberg claims? No, not in any way! According to Seiff,

"The heating rates needed to warm the atmosphere from the Day probe [Pioneer Venus] profile to that of *Venera 9* integrated over altitude . . . is 45 times the midday solar heat absorbed at 30 degrees latitude . . . This is also true for the *Venera 10, 11, and 12* data relative to the large probe data, for the necessary heating rates integrated over altitude are < [somewhat less than] 40 times the mean dayside solar input for the albedo of 0.71."¹⁰³⁴ (Emphasis added)

There was an average of somewhat less than 40 times more heat than is provided by sunlight given off by Venus. Again, unable to accept these measurements of thermal imbalance at the surface as with the other readings, the scientists decided that "heat . . .—must be ascribed to other processors"¹⁰³⁵ or to faulty instrumentation.

Thus, each and every single set of readings or measurements, and I emphasize ALL sets, showed thermal imbalance from the cloud tops to the middle atmosphere to the surface in complete defiance and contradiction to Oberg's claim about space probe measurements. Yet Oberg does not hesitate to submit, as a fact, that probes to Venus produced measurements showing Venus' atmosphere "is not giving off more heat than it gets from the sun." And then Oberg claims that Velikovsky is adept at changing the meaning of sentences about his prediction by "erasing sentences." Well, if changing every set of the probes' measurements from "imbalance" to "balance" isn't accomplished by *erasing* "im" from "imbalance," then someone is adept at doing something! It was not Velikovsky who changed imbalance to balance by erasing "im," but a certain critic of Velikovsky who has a religious fate so great in the 'runaway greenhouse effect' that he can change all the measurements to fit his belief system.

As Warwick Bray says with respect to the topic of scientific measurements, "Justification in terms of scientific methodology is in part self-deception, for *when the figures turn out wrong the true believer will always shuffle the figures.*"¹⁰³⁶ (Emphasis added) And that is what has been done with all the three sets of measurements that showed thermal imbalance. Such behavior by the scientists who changed the figures or ignored them, or by Oberg who misrepresented them does not serve science, or ethical journalism, or ethical criticism!

Typical of Oberg's refusal to face facts contrary to his assertions is this statement by him in *The Skeptical Inquirer* about Velikovsky's claim that remanent magnetism on the Moon would be discovered.

"The traces of magnetism in the lunar lava, which *did* surprise geologists, was in Velikovsky's view the result of the rocks melting and recooling only thousands of years ago However, the actual magnetic orientations of the surface are randomly directed, indicating that they were formed in connection

¹⁰³³*Pioneer Venus*, NASA SP-461, ed. Richard O. Fimmel, Lawrence Colin, Eric Burgess, (Washington, D.C., 1983), p. 127.

¹⁰³⁴A. Seiff, "Thermal Structure of the Atmosphere of Venus," *Venus*, ed. Donald M. Hunten, Lawrence Colin, Thomas M. Donahue, V. I. Moroz; (Tucson, 1983), (Univ. of Arizona), p. 226.

¹⁰³⁵*Ibid.*

¹⁰³⁶Warwick Bray, "The Paleoindian Debate," *Nature*, Vol. 332, (1988), p. 107.

with individual cratering events. They do not show a planetwide consistent orientation as they presumably would in the case of an externally imposed field" [created by Venus].¹⁰³⁷

Oberg, again without one citation to support his assertion, claims that the Moon has a patchy surface magnetic field with magnetic orientations pointing every which way, and this does not show a "planetwide" global magnetic field. This statement by Oberg is false and it was pointed out to him twice!¹⁰³⁸ Thus, I will point it out to him a third time. There are actually two types of magnetic fields on the Moon. According to Zdenek Kopal:

"The sub-satellite of Apollo 15-17 missions orbited at altitudes substantially lower than that of Explorer 35 and detected local magnetic fluctuations between 20 and 30 gammas. These fluctuations turned out to be correlated with specific formations (craters) on the lunar surface overflowed by the magnometer. The carriers of this field are brecciated rocks produced by impacts. At any rate, *the origin of this field which fluctuates with topography can only be skin deep and has nothing to do with the deep interior.*

"*The same is not true of the remanent magnetism of lunar crystalline rock which are found below the surface.*"¹⁰³⁹ (Emphasis added)

Kopal tells us there are two kinds of magnetism of the Moon, a surface splotchy magnetic field and a subsurface crystalline magnetic field. Kopal in complete contradiction to Oberg's statement explains that "the magnetism of these [subsurface lunar rocks] is stable and suggests a prevalence of much stronger fields (100-1000 gammas) at the time of solidification."¹⁰⁴⁰ In this respect, Peter Cadogan, also contrary to Oberg, states:

"Magnetic features as small as a mile across would simply not be detected from [high] orbit if they were randomly distributed. The magnetic measurements at different altitudes imply that crystal magnetism may in places be coherent down to depths of at least several thousand feet. ONLY A GLOBAL MAGNETIC FIELD . . . COULD POSSIBLY ACCOUNT FOR THIS COHERENCE."¹⁰⁴¹ (Capitalization added)

Oberg having been confronted by these statements by scientists has simply been stonewalling this evidence and has still been unable to admit the simple fact that the Moon appears to possess a global magnetic field just as Velikovsky predicted. These facts and others show that James Oberg misrepresented the scientific evidence about Velikovsky and his work again and again in *The Skeptical Inquirer*. *The Skeptical Inquirer* will, of course, never consent to present these *facts* in that journal. And this is all supposedly "scientific," "rational" and "ethical" journalism. I find I must agree with David W. Miller of Columbia University in New York who described Oberg's criticism on another topic as a "boorishly

¹⁰³⁷Oberg, *The Skeptical Inquirer*, *op. cit.*, p. 25.

¹⁰³⁸Charles Ginenthal, "The Moon in Upheaval," *The Velikovskian*, Vol. I, No. 1, (1993), pp. 94-98; and Charles Ginenthal, "Oberg's Unscientific Method," *The Velikovskian*, Vol. I, No. 4, (1993), pp. 32-35.

¹⁰³⁹Zdenek Kopal, *Realm of the Terrestrial Planets*, (New York, 1979), p. 52.

¹⁰⁴⁰*Ibid.*

¹⁰⁴¹Peter Cadogan, *The Moon – Our Sister Planet*, (Cambridge, Eng., 1981), p. 316.

ill-mannered piece . . . [which] displays less than complete regard for the truth." [Miller requests] "a rational argument based on the weight of the evidence is needed. Ranting and raving is not enough."¹⁰⁴² Nor is suppression of this evidence by *The Skeptical Inquirer's* scientists, journalists and editors in any way ethical science or journalism, nor can it ever be made the case.

* * * * *

KENDRICK FRAZIER AND DISTORTION

"If you look at the preface of his book [Thomas Kuhn's *The Structure of Scientific Revolutions*] it is describing what it was like when he was a research student. I have reread the book quite recently with new eyes, to use it in philosophy of science lectures. What has struck me about it is that in the preface there is an image of normal scientists who are very nasty men. All they want to do is force nature into conceptual boxes provided by their professional education. When a threatening innovation comes along they try to suppress it . . ."¹⁰⁴³

"Thus a study of Mach not only introduces us to a wonderful person and a fascinating philosophy of science; it also teaches us an interesting lesson about the nature of scholarship: 'experts' frequently do not know what they are talking about and 'scholarly opinion' more often than not is uninformed gossip."¹⁰⁴⁴

"Conflicts occur when your findings clash with accepted scientific dogma. This is the second path to scientific discovery, but it is generally not a pleasant experience. Those who question the prevailing point of view are usually regarded as outsiders, as mavericks, as renegades. Their behavior is often seen as dogmatic or pathological. And the scientific community responds for either

¹⁰⁴²David W. Miller, "Letters," *The American Spectator*, (December 1993), p. 86.

¹⁰⁴³Jerome R. Ravety in *Vistas in Astronomy*, Vol. 117, ed. A. Beer, K. A. Beer, (New York, 1975) p. 129.

¹⁰⁴⁴Paul Feyerabend, *Farewell to Reason*, (London, Eng., 1987), p. 16.

turning on you or away from you. The history of science is riddled with examples."¹⁰⁴⁵

On April 20, 1980, *New York Times Book Review* presented the following ad:

"Velikovsky"

"The Controversy Continues"

"In 1950 the publication of *Worlds in Collision* created one of the most heated controversies in the history of science. Challenging many of the accepted views about the order of the universe, Immanuel Velikovsky's book became a #1 bestseller [*sic*]. It was attacked by experts as well as by vested interests as heresy. It was enthusiastically received by other renowned scholars and general readers. Now thirty years later, the controversy continues and Velikovsky's work is the subject of books, magazines and articles as well as symposia held throughout the world.

"Operating on the basis of his central idea—that our solar system has undergone catastrophic changes in historical times—Dr. Velikovsky presented unorthodox claims about the nature of the planets and interplanetary space. For example, he claimed that space is not a void, but filled with electromagnetic fields and radiations. This claim completely contradicted the accepted view. But the first satellites encountered magnetic fields, solar winds and radiation and 'heresy' became fact. Similarly Velikovsky's assertion that Venus is a young planet expelled from Jupiter only thousands of years ago, has received strong support from the evidence of the Pioneer probe which revealed so little surface erosion and such an abundance of primordial argon-36 that scientists now speculate that it appears Venus was formed more recently than the other planets and by a different process. These findings upset their uniformitarian view of the solar system.

"In the decades since the publication of *Worlds in Collision*, dozens of Velikovsky's claims have been verified. Indeed, William Plummer wrote in *Science*, 'Some of the least expected discoveries made by planetary astronomers in recent years were correctly predicted by Velikovsky. He argues that Jupiter should be a strong source of radio waves, that the Earth should have a magnetosphere, that the surface of Venus should be hot, that Venus might exhibit an anomalous rotation, and that Venus should be surrounded by a blanket of petroleum hydrocarbons. All except the last of these predictions have been verified, most of them by accident.' In fact, in the time since this

¹⁰⁴⁵Louis A. Frank and Patrick Huyghe, *The Big Splash*, (New York, 1990) P. 31.

was written in 1969 the last of these has been verified. Methane (hydrocarbons) has been found on Venus.

"In the preface to his yet-to-be published *The Test of Time*, Velikovsky looks back on his career and writes, *I was compelled by logic and by evidence to penetrate into so many premises of the house of science. I freely admit to having repeatedly caused fires, though the candle in my hand was carried only for illumination.* Immanuel Velikovsky died last November, hard at work on that process of illumination. Though he is gone now, he left behind the manuscripts of several new books which will doubtless fuel the debate about his work, stimulate original thinking, and interest intelligent and open-minded readers in search of an understanding of the forces that have shaped our world . . . DOUBLEDAY & COMPANY."

The ad is right in many ways; in particular, it stirred up a hornet's nest at *The Skeptical Inquirer*. Kendrick Frazier, the Editor-In-Chief, and several well respected astronomers and other scientists descended from their lofty scientific perches like eagles on this advertisement of Velikovsky's book. In order to combat the ad, Kendrick Frazier "decided to send copies of this ad to a number of noted planetary scientists and ask them about, its assertions."¹⁰⁴⁶ Frazier assures us these are real "scientists" and the implication is left standing that whatever real scientists say must be accepted because they are the experts and *the* authorities. Therefore, if they overwhelmingly agree that the substantive claims in the ad were wrong, then the scientists must be right. However, I hasten to remind the reader what Cyrus H. Gordon, a philologist, historian and wise-old-bird scholar (now deceased) once cautioned: "Scholars are not expected to take anyone's word on faith."¹⁰⁴⁷

What Gordon meant, of course, is don't trust statements by anyone—scientists, editors, included, unless their statements are backed up by real evidence, not by authority. When such statements are presented by scientists and editors, unsupported by evidence that one can check, what is going on may be dishonest and even fraudulent. As we saw with Oberg, he made statements again and again unsupported by any evidence from the scientific literature that turned out to be misrepresentations!

Tom Van Flandern, a well respected astronomer says that such an approach to scientific questions is in reality the "unscientific method" of debate.

"Most scientists recognize unscientific approaches when they see them. But one approach, the 'appeal to authority,' is so commonplace that scientists suppress their knowledge that it is unscientific, even when challenged on that point. The excuse is often that science is complex, and one cannot look into everything for oneself. . . .

"The problem arises when we cross the line from respecting their superior knowledge in an area to accepting their judgment as superior."¹⁰⁴⁸

¹⁰⁴⁶Kendrick Frazier, "The Distortions Continue," *The Skeptical Inquirer*, (Fall 1980), p. 33.

¹⁰⁴⁷Cyrus H. Gordon, *Riddles in History*, (New York, 1974), p. 11.

¹⁰⁴⁸Tom Van Flandern, *Dark Matter, Missing Planets and New Comets*, (Berkeley, Calif., 1993), p. 359.

I have cautioned the reader for what is about to come and ask him/her to see what Frazier is up to. See, if one can find an iota of solid documented evidence or semblance of documented evidence in the following statements presented as evidence against the ad. See if it is evidence or authoritarian propaganda.

"'The ad is thoroughly dishonest,' said A. G. W. Cameron of the Harvard-Smithsonian Center for Astrophysics. 'The ad, like the Velikovsky books it is promoting, contains more falsehoods in a paragraph than one can refute in a chapter,' said Edward Anders, a University of Chicago cosmochemist who has specialized in chemical studies of the solar system. 'No reputable scientist now speculates that. . . . Venus was formed more recently than the other planets', said Anders.

"'The statements [in the ad] are not accurate,' said geophysicist William M. Kaula of the University of California at Los Angeles. 'As a Pioneer Venus co-investigator, I am not aware of any support in the scientific community for Venus being a young planet. The Pioneer probe did not reveal anything conclusive about surface erosion; its relevance to origin is very slight, in any case. The excess of argon-36 suggests, if anything, that Venus is older, since the solar wind swept volatiles out of the nebula with time.'

"'Several of the statements in the ad are outright lies,' said University of Hawaii astronomer David Morrison, particularly the assertion that scientists now speculate that Venus is a young planet. While the large amount of argon-36 discovered by Pioneer has indeed upset current ideas about the conditions (particularly pressure) in the solar nebula when the planets formed, a recent birth of Venus is not in anyway indicated.'

"'Anyway,' said UCLA astronomer George O. Abell, 'Velikovsky never mentioned argon-36.'¹⁰⁴⁹

This is the flavor of Kendrick Frazier's attack. He has presented no evidence or sources which the reader could check to verify these criticisms. He goes on to quote, chemist Vance I. Oyama of NASA's Ames Research Center that argon-36 is consistent with Venus' position in the solar system and its age. He cites Michael B. McElroy, a Harvard University atmospheric chemist and specialist in planetary atmospheric studies that the noble gases on Venus may make the planet only slightly younger, perhaps a million years. That is, all these scientists suggest, without any documentation, in rather strong and positive terms, that Venus' crust is at least 4.3 billion years old and, since the planet cooled from an earlier molten state, the planet is about 4.5 billion years old, as their theory of solar system formation requires.

However, the reader, I'm sure, has noted the indignation and vilification presented in these attacks which are merely these scientists and Frazier's crude attempt at insulting Velikovsky and his supporters. On the other hand, I would add the following comment on Frazier's "appeal to authority" as stated by Van Flandern.

"The important corollary [related to appeals to authority] is that a scientific expert has that status by virtue of the facts he commands in his area of

¹⁰⁴⁹Frazier, *op. cit.*, p. 34.

specialization. Nothing about his expert status implies that his judgment is in any way better than average, or better than our own. . . . An expert who withholds his facts so that we have no choice but to rely on his judgment is not to be trusted. An expert who asks that we respect his judgment simply because he is an expert is being naive and egotistical."¹⁰⁵⁰

But this is in reality the entire thrust of these criticisms. We are asked to rely on the authoritative judgments of Frazier and his scientists.

How is one to determine the truth about what Frazier and these scientists have reported? Did he or they present a single citation that the readers of *The Skeptical Inquirer* can go back to and check? Of course not! Frazier is simply asking his audience to respect these scientists' authoritarian judgments because they are the experts. Van Flandern has told us such an expert is "not to be trusted" and that those who employ this tactic are "naive and egotistical." There is no doubt that each of the scientists cited by Frazier are highly respected members of the scientific establishment and do speak with authority. But just how good have such scientific experts been with their own predictions about the planets? For all of Frazier's puffing of their exulted status as planetary authorities, theirs and their colleagues predictions about the nature of the planets before the space age were simply dismal. Eric Burgess' remarks sum up aptly the arrogance underlying Frazier's reliance on such authorities.

"Since planetary probes have observed other planets at close range, virtually all generally accepted ideas about the planets had to be changed. Without exception every exploration of a planet by a spacecraft produced surprises of such a radical nature that all existing astronomical textbooks became completely erroneous in their information about the planets."¹⁰⁵¹

It is to such experts with such a record Frazier now commends us. But I suggest that if their record has been so erroneous about these planetary bodies in the past, should we not exercise a good deal of cautious reserve now regarding their angry pronouncements? Frazier's presentation leaves a distinctly different impression.

The overall impression given by all this is that Velikovsky's ideas have a very unpopular status within the scientific establishment. That is in great measure all that this approach of Frazier actually implies. As John C. Briggs states,

"I think . . . dividing people into groups and saying you've got so many people in this group and for this theory, . . . is like trying to prove a hypothesis on the basis of its popularity or by vote. This of course is a very nonscientific procedure that should be avoided. . . . This business of popularity of concepts is not the scientific process."¹⁰⁵²

¹⁰⁵⁰Van Flandern, *op. cit.*, p. 361.

¹⁰⁵¹Eric Burgess (A), *By Jupiter: Odyssey to a Giant*, (New York, 1982), p. 9.

¹⁰⁵²William Glen, compiler of debate, "Epilogue: A Panel Discussion on the Mass-Extinction Debates," *The Mass Extinction Debates: How Science Works in a Crisis*, ed. William Glen, (Stanford, Calif., 1994), p. 276.

Nevertheless, Frazier, even denying he used such an approach, has made it his business to employ popularity—a very nonscientific procedure that should be avoided—as one of the major processes of arguing against the validity of Velikovsky's views. The argument goes: if none of these scientists agree with Velikovsky, his theory must be wrong. This is only a political and polemical procedure in place of the scientific one which requires only evidence as the criteria of proving or disproving anything.

However, these scientists did, indeed, make statements about erosion and argon-36, albeit, unsupported by citations and evidence. It is with regard to these statements—recast as conclusions—with which I wish to take issue. The argument runs as follows: Primordial argon-36, which the Doubleday & Company ad suggests was outgassed by Venus, in support of Velikovsky's hypothesis of a young planet was, according to the scientific experts, degassed by the planet which was emitted by the early sun and captured by Venus as it formed and subsequently degassed. The closer the planet is to the sun, the greater should be the build-up of the gas in particles that formed the planet. Hence, it is suggested that argon-36 is clear, unimpeachable evidence that contradicts Velikovsky's theory.

In the 1500's, Galileo, using the telescope, showed the professors of his time that the Moon was not a perfect body, but had imperfections, such as mountains, valleys, etc. over its surface. These learned men, unable to deal with this fact which contradicted their dogma, claimed that, although there were mountains and valleys on the Moon, the entire Moon also had a coating of invisible material covering its surface features and, therefore, the Moon was still a perfect sphere. Frazier and his authorities, when confronted by argon-36, created a theoretical process so that they could hold onto their own dogma. By inventing such a theoretical process to "save the appearances" of their dogma, they do not have to face the grave implications argon-36 poses to their theory. They have a theory, but is it really all they claim it to be, or is it the same stuff with which the professors of Galileo's time covered the Moon? I, again, must point out, however, that this theory is far from having been proven. Argon-36 could just as plausibly have been recently vented from Venus' interior via volcanic processes, if Venus were a new planet.

As Zdenek Kopal explains, "¹⁰⁵³³⁶Ar comes mainly from the deep interior of a planet." So Venus' argon-36 would have to have been derived from its interior by volcanism. Now Frazier and his experts have informed us that they know that Venus' argon-36 is derived from the sun and then outgassed via volcanism. This is, of course, based on their modeling of the origin and evolution of Venus' atmosphere. The only problem with such an assertion is that the origin and evolution of Venus' atmosphere is fraught with very serious problems. These were fully acknowledged, discussed and analyzed by T. M. Donahue and J. B. Pollack. They state:

"None of the theories [of the origin and evolution of Venus' atmosphere] discussed above is free of serious deficiencies in explaining the origin of Venus' atmosphere, particularly when called upon to account for the volatiles on Earth and Mars, models in which the planets grew in a gas-rich environment naturally account for the gradient in . . . Ar abundance with distance from the sun, *although this is not clearly the case for gaseous protoplanets*. . . . [T]hey have a fundamental problem in explaining the overall depletion of noble gases relative to other volatiles and the departure of the abundance ratios of the noble gases, and, in some cases, of isotopic ratios from the solar pattern on all planets. . . . *It also has a problem in explaining how argon can be more efficiently lost than carbon and nitrogen, or why the argon to krypton and argon to xenon ratios become more nearly solar the closer the planet is to the sun.*"¹⁰⁵⁴ (Emphasis added)

¹⁰⁵³Zdenek Kopal, *Realm of the Terrestrial Planet*, (New York, 1979), p. 41.

¹⁰⁵⁴T. M. Donahue, J. B. Pollack, "Origin and Evolution of the Atmosphere of Venus." *Venus*, ed. D. Hunten, *et al.*, (Tucson, 1983), p. 1013.

That is, three years after Frazier and his experts proclaimed that their explanation for argon-36 is well explained as being captured from the early solar nebula and then outgassed, other authorities say that other gases, krypton and xenon, tell a completely different story related to argon-36 and that for early gaseous protoplanets this capture process "is not clearly the case."

This is explained in the NASA Publication, *Pioneer Venus*, which specifically states, "In the grain accretion model there is no reason to expect the enrichment of one rare gas to be greater than another."¹⁰⁵⁵ However, we were told that the gases Krypton and Xenon, which are rare gases, are found in very different amounts compared to the other known gases. In the most basic terms, the theory espoused by Frazier and his authorities is well-known to be wrong based on this evidence!

To further make the point that the explanation promoted by Frazier and his experts is a sham, three years after they wrote their criticism of Velikovsky it was fully admitted

"The main unresolved question. . . .

"Why is the Venus atmosphere enriched markedly in nonradiogenic isotopes of inert gases, ²⁰Ne ³⁶Ar, Kr and probably Xe as compared with Earth's atmosphere?"¹⁰⁵⁶

Here it is fully admitted that the cause of the gradient of gases from Venus to Earth and even Mars is an "unresolved problem." Why did Frazier and his authorities cover up this problem? And this statement, also made three years after Frazier's authorities made this *faux pas*, is made in a highly respected book published by a highly respected university.

In other words, Frazier's experts were not telling the readers the truth regarding their analysis. Their theory, which contains "serious deficiencies," is being passed off as if it were a proven fact and they buried the problems contradicting their concept. Why? I suggest that it is because they were dealing with Velikovsky and felt they could get away with this maneuver. So much for expert testimony in *The Skeptical Inquirer*, which turns out to be nothing but uninformed "gossip," as Feyerabend suggested!

But if Venus is truly as old as Frazier and his authorities present, based on their interpretation of argon-36, shouldn't this be supported by well measured findings of the other form of argon to conform with this old aged planet?

Regarding this point, argon-40 should also give a clear indication that Venus is an ancient body. Argon-40 is a radioactive decay product of the breakdown of potassium 40. Over time, argon-40 should increase in amount in Venus' atmosphere to levels comparable to that of the Earth. Since Venus is as ancient as the Earth, it is a very reasonable expectation that argon-40 should exist in Venus' atmosphere in amounts similar to that of the Earth's. If Venus is a very young planet, it should have much less of this material. But contrary to this expectation of an old planet, Venus' argon-40 is in extremely short supply. Billy P. Glass informs us that, "the ratio of the mass or radiogenic Ar 40 to the mass of Venus is smaller by a factor of 15 to the value for Earth."¹⁰⁵⁷ That is, a factor of 15 means Venus has over a trillion times less argon-40 than should be found. Doesn't this indicate Venus is a younger planet than the Earth? Eric Burgess further informs us, "Since the abundance of potassium and the potassium-to-uranium ratio are both Earthlike [*sic*] in the Venus rock samples so far examined, an initial shortage of potassium cannot be the reason for less argon-40 today."¹⁰⁵⁸

David Morrison, in order to explain this dearth of argon-40, has chosen to put a halt to Venus' outgassing via volcanism. He suggests, "There must be more Ar 40 trapped in the planet's crust and mantle waiting to escape. The fact that it hasn't suggests that the crust of Venus has indeed become frozen at an early stage of its evolutionary

¹⁰⁵⁵*Pioneer Venus*, NASA SP-461, (Washington, D.C., 1983), p. 147.

¹⁰⁵⁶L. M. Mukhin, "The Problem of Rare Gases in the Venus Atmosphere," *Venus*, D. M. Hunten, *et al.*, eds. (Tucson, 1983), p. 1037.

¹⁰⁵⁷Billy P. Glass, *Introduction to Planetary Geology*, (New York, 1982), p. 314.

¹⁰⁵⁸Eric Burgess, *Venus an Errant Twin*, (New York, 1985), p. 141.

development."¹⁰⁵⁹ What Morrison's suggestion requires is that for some ad hoc reason, Venus ceased all or almost all its volcanic activity billions of years ago because, if Venus had normal volcanic activity, it would also have outgassed approximately, by comparable mass, an amount of argon-40 similar to that of the Earth.

According to NASA's official publication, *Pioneer Venus*,

"The argon results were extremely surprising. . . .

"It is [now] assumed that Venus derived the greater part of its atmosphere from the protoplanetary nebula. Earth (and Mars) captured relatively little gaseous material from it, whereas most of their atmospheres were outgassed from their interiors. . . ."1060

David Morrison has stated that argon-36 was captured by Venus from the early "solar nebula." *Pioneer Venus* tells us that this argon-36 was outgassed from the planet's interior subsequent to Venus' formation. Here, then, is the double talk Morrison has presented. Morrison has Venus volcanically generating its abundant argon-36 from the interior of the planet and, at the same time, he has stopped the planet from outgassing via volcanism to halt argon-40 from entering the Venusian atmosphere. How Venus is both outgassing argon-36 and at the same time not outgassing argon-40, Morrison never quite explains. But he calls the Doubleday ad "outright lies," while offering such stunning logic. The impression Morrison is giving, I suggest, is not very rational.

As one can see, one must somehow invent a twofold concept to literally shut down, and at the same time keep in full operation, Venus' volcanic activity for billions of years. The problem ensuing is this: How does Venus vent or emit its internal heat? To completely stop Venusian volcanism, I humbly suggest, is highly implausible. A crust losing heat by radiation would develop hot spots which ineluctably lead to volcanism. According to Roger J. Phillips and Michael Malin, "hot spots . . . would take over the heat transfer formerly carried out by seafloor [*sic*] spreading. The hot spots would grow in vigor, frequency, and magnitude to provide the required heat transfer. Regions such as Iceland, the North Atlantic Gravity High, and Hawaii would be more common; features larger than these might be expected, and there would be a denser background of small-scale hot spots (capped by individual volcanoes)."¹⁰⁶¹ These scientists say that if there were no plate tectonic motions and Venus' crust became frozen, as Morrison suggests, then large amounts of magma would rise and work its way to the surface to form hot spots and also lots of volcanoes. Venus is, of course, covered by tens of thousands of volcanoes. In any case, shutting down volcanism in Venus' early period is not explained, and even if it were to occur for some strange reason, over time, hot spot volcanism would follow thereafter. Volcanism will outgas the internal gases locked in the crust and mantle, and among these gases would be argon-40 and argon-36.

But let us accept and examine more closely Morrison's view that Venus' crust froze billions of years ago so that argon-40 could not escape the interior. This also means that argon-36 could not escape from the interior at about the same time the crust froze over. Of course, the argon-40 would survive because it is an inert gas which does not react to form compounds. Although argon-36 is the same, it is radioactive and decays away. Therefore, after Venus' crust froze, no more or very little argon-36 entered the atmosphere and, over the few billion ensuing years, the argon-36 would have decayed away, and there would still be very, very little of this gas in Venus' atmosphere. The atmosphere would contain only about as much argon-36 as the Earth based on these conditions. What Frazier, Morrison and the other scientists have completely ignored and refused to squarely face is the fact that one cannot have both forms of argon in their present amounts if Venus' crust froze, nor could they have them in their present amounts if Venus' crust produced volcanism comparable to the Earth. If Venus' crust froze halting emissions of argon-40 and argon-36, then argon-36 would decay away and there should be very little of it in the atmosphere compared to what was found. If Venus' crust

¹⁰⁵⁹David Morrison, Tobias Owen, *The Planetary System*, (New York, 1988), p. 254.

¹⁰⁶⁰R. Z. Segdeev, V. I. Moroz, T. Breus, "Results of Soviet Studies of Venus," *Pioneer Venus*, NASA SP-461, (Washington, D.C., 1983), p. 174.

¹⁰⁶¹Roger J. Phillips, Michael C. Malin, "The Interior of Venus and Tectonic Implications," *Venus*, ed. D. M. Hunten, *et al.*, (Tucson, 1983), p. 213.

did not freeze over and volcanism was on-going over billions of years, there would be, based on the argon-36 capture concept, plenty of argon-36 available, but Venus would also have outgassed as much argon-40 as the Earth's atmosphere contains. Frazier, Morrison and the other scientists are in a hopeless situation and are free to invent any makeshift, *ad hoc* theory to salvage their views of an ancient Venus. But the evidence is undoubtedly in full support of Velikovsky's theory.

In fact, there are approximately equal amounts of the gases ^{36}Ar and ^{40}Ar in Venus' atmosphere¹⁰⁶² as though Venus simply outgassed these in equal amounts recently. The possibility of Venus outgassing exactly the same amount of argon-40 as it supposedly captured from the early solar nebula after billions of years is so highly improbable as to make the suggestion that Venus could have done so scientific hand-waving. It is abracadabra science based on a clearly impossible coincidence. But neither Kendrick Frazier nor his authorities ever mention this! Galileo's critics are apparently still with us.

As one can see, argon-40 also contradicts Venus' ancient age just as does argon-36. Let us, nevertheless, allow for a moment that Venus is an ancient planet and examine the evidence regarding its missing planetary soil or regolith discussed in the Doubleday ad. Kaula has argued that surface erosion, as relevant to Venus' origin, "is very slight." Why, one may ask. Only because he told us so. It is also quite interesting how quickly Frazier maneuvered the discussion away from the evidence of erosion to focus our attention to argon-36. I respectfully must disagree with Kaula's authoritative judgment because erosion may actually be crucial to understanding the age of Venus.

Therefore, let us examine the evidence of this lack of a regolith or planetary soil covering Venus which, I suspect, the scientists clearly wish to avoid facing. I suggest that this evidence is contrary to the scientists' dictates and quite crucial, and that these authorities may be so disturbed by it that they cannot squarely face it and its clear implications. Perhaps that is why they have not discussed and literally avoided it in their attacks on Velikovsky. I hope, at the very least, if they will not change their minds about Velikovsky's young Venus' concept, they may change their minds about the question of Venus' missing regolith.

Over a hundred years ago, the question of the age of the Earth was waged in a scientific war between William Thomson, better known as Lord Kelvin, and Charles Darwin. Thomson, following Helmholtz's research in 1854, concluded that the solar system was some 20 million years old, based on the length of time it would take for the newborn sun and the molten Earth to cool to their present temperatures.¹⁰⁶³ Over the following decades his conclusions varied up and down between 20 and 50 million years. This estimate ran directly counter to Darwin's theory of evolution which required great spans of time and, since Thomson's prestige was so great, opposition to his scientific claim was dismissed as unscientific.

What Darwin did to argue against Thomson's time restraint (as I will do with respect to Venus' age) was to present a *gedanke experiment* (thought experiment) related to the basic concept of erosion of a geological structure of the Weald in Kent, England. According to Digby J. McLaren,

"The strata involved range in age from low in the Cretaceous to mid-Tertiary. Darwin's estimate of the time taken for erosion to remove the 300 or more meters [about 1,000 feet] of sediments from the anticline was about 300 million years, with an uncertainty of about 200 million each way. He arrived at this figure by estimating the speed of coastal erosion and the activity of the rivers of the Weald. The object of the 'experiment' is not so much a serious attempt to make a reasonably close estimate, but rather to suggest an order of magnitude for a known geological event that plainly represented a very short time interval relevant to the total geological time scale. 'I have made these few remarks

¹⁰⁶²Burgess, *Venus ...*, *op. cit.*, p. 145.

¹⁰⁶³Digby J. McLaren, "Impacts and Extinctions: Science or Dogma?" *The Mass-Extinction Debates: How Science Works in a Crisis*, ed. William Glen, (Stanford, Calif., 1994), pp. 123-124.

[Darwin wrote] because it is highly important for us to gain some notion, however imperfect, of the lapse of years.' Fully aware of Thomson's views of the age of the Earth, he was attempting to quantify the evidence for the slowness of geological processes.

"Presumably because of Thomson's unquestioned authority, opponents of Darwin's speculation made little attempt to demonstrate that erosion must take place at a pace many orders faster than he had suggested, if all geological history was to be fitted into Thomson's time scale."¹⁰⁶⁴

Darwin's erosion analysis was regarded as erroneous because of the *expert testimony* of Thomson. Other scientific authorities assumed Thomson was correct and evaded and ignored the possibility that erosion could possibly contradict physics. "Darwin advised his fellow naturalists to be slow in admitting the conclusions of astronomers, and to be cautious in trusting mathematicians."¹⁰⁶⁵ This, I suggest, is excellent advice, as erosional evidence relates to Venus' age. And Darwin was right in the long run. Erosion turned out to be fully commensurable with the later findings in physics. "He expected opposition from nonscientists, especially theologians, "but he had not anticipated, [. . . according to D. L. Hull, *Darwin and His Critics*, (Cambridge, MA 1973)] 'the vehemence with which even the most respected scientists and philosophers of his day would denounce his efforts as being not properly 'scientific.'"¹⁰⁶⁶ According to David L. Fisher, "Darwin's own calculation, based on geological processes, of a 300-million-year age for the Earth was attacked by several leading geologists as 'geologically absurd' and 'amateurish' . . ." ¹⁰⁶⁷ Doesn't this all sound like Frazier's experts on the matter of Venus' age *vis-à-vis* erosion?

Therefore, I will also attempt to demonstrate scientifically, much as Charles Darwin did so long ago, that erosional or, more properly, regolithic evidence on Venus, is clearly contrary to everything the present noted scientific authorities have presented with respect to the age of that planet.

One of the basic arguments against Velikovsky's predictions about Venus is that the planet is heated by a runaway greenhouse effect and not by internal planetary heat. A tenet of the greenhouse history is that, in its early history, Venus had an ocean. David Morrison presents this conclusion in his co-authored book.¹⁰⁶⁸ He explains that on Venus:

"A runaway greenhouse can raise the temperature throughout the lower atmosphere, giving water [in Venus' oceans] free access to high altitudes Because of the large mass of Venus, only the light hydrogen atoms escape into space. The oxygen remains behind to combine with rocks on the planet's surface and with other gases that have been produced by the intense heating. The runaway greenhouse leads to elimination of water from a planet in a perfectly natural way."¹⁰⁶⁹

¹⁰⁶⁴*Ibid.*, pp. 124-125.

¹⁰⁶⁵*Ibid.*

¹⁰⁶⁶*Ibid.*

¹⁰⁶⁷David L. Fisher, *The Birth of the Earth*, (New York, 1987), p. 48.

¹⁰⁶⁸David Morrison, Tobias Owen, *op. cit.*, pp. 251-252.

¹⁰⁶⁹*Ibid.*, p. 252.

An ocean, it is assumed by the scientists, existed on Venus' surface. The question is, for how long? According to James Pollack, a major advocate of the runaway greenhouse concept, "runaway conditions were not reached on Venus until halfway through its history."¹⁰⁷⁰ This is so because the early sun was "approximately 40% less luminous than we see it today and its surface temperature 10% lower . . . the early climactic conditions on Venus could not have been much warmer than they are in the terrestrial tropic today, and had there once been oceans on Venus, very little of their waters could have evaporated."¹⁰⁷¹ So the greenhouse theorists posit oceans on Venus for about half of its history before the sun's heat intensified enough to start the runaway greenhouse effect. Venus would have formed its oceans at about the same time as the Earth did some 3.5 billion years ago based on the age of the earliest sedimentary rocks that are formed in seas.¹⁰⁷²

Therefore, if as the scientific experts Frazier presents are correct that Venus is some 4.5 billion years old, then about one billion years later it had an ocean which would have been in place half way through Venus' existence as a planet to about 2.25 billion years ago. In other words, Venus had an ocean from 3.5 to 2.25 billion years ago, or for about 750 million years. Now what do oceans do on Venus for over 750 million years? If they behave like other oceans on Earth, they *erode* the shore lines around their basins and produce sands, or soils or muds. Furthermore, oceans have a tendency to evaporate and create clouds of water vapor which are carried by winds over land where they condense and generate rain which creates, rivers and lakes of all sizes which also act to *erode* the surface rock. Water, I need not remind Cameron, Anders, Morrison, Kaula or Frazier and these other authorities, is a very powerful eroding agent. That is, for almost a billion years, the surface of Venus was eroded just as it is on the Earth and since that is the case, as it certainly must have been, than the entire planet would have been covered with a thick layer of planetary soil, and some areas would probably have formed deserts. Untold, billions upon billions of tons of detritus would have been created all over the planet. The question that naturally arises in this context is: *Where is all this regolithic material today? Did it all disappear* to leave Venus with an extremely bare surface? As David Harry Grinspoon tells us:

"But there is something quite strange, almost unnatural, about the Venusian craters. Nearly all of them appear pristine, as if planted there recently. Indeed, VIRTUALLY EVERY GEOLOGICAL FEATURE OF THE PLANET APPEARS BRAND-NEW even though the surface is quite old."¹⁰⁷³ (Capitalization added)

He goes on to add:

"In effect, Venus looks much like an earth that is arid and *devoid of erosion*. (Emphasis added)

"The absence of erosion, however, goes only partway toward explaining the strangely fresh appearance of the crater population. The problem is this: A surface less than a billion years old is still unusually young by planetary standards. Any craters that existed in the planet's earlier days seem to have

¹⁰⁷⁰James E. Oberg, *New Earths Restructuring Earth and Other Planets*, (Harrisburg, Pa., 1981), p. 205.

¹⁰⁷¹Zdenek Kopal, *Realm of the Terrestrial Planets*, (New York, 1979), p. 193.

¹⁰⁷²"Oceans, Development of," *Encyclopedia Britannica, Macropedia*, Vol. 13, (Chicago, 1982), p. 477.

¹⁰⁷³David Harry Grinspoon, "Venus Unveiled," *The Sciences*, (July/August 1993), p. 24.

been obliterated by some geological process. But what process could it be? Not erosion certainly."¹⁰⁷⁴

Henry S. F. Cooper, Jr. tells us Magellan showed that:

". . . all the bedrock [of Venus] is exposed. The entire planet is like a great big road cut . . . what we're looking at is total outcrop, total exposure of everything that happened to it."¹⁰⁷⁵

It is quite clear that whatever removed the assumed other ancient craters did not create regolith. If lava covered them, Grinspoon adds, then there should be a large number of craters partially covered by lava. "Yet on Venus only about 4 percent . . . are partly covered by lava flows."¹⁰⁷⁶ But nowhere have I found a scientist or a science writer who has dealt with all the erosional materials that should be somewhere on Venus created by approximately a billion years of water erosion. Is it really possible and probable, or even plausible, that almost a billion years of erosional products simply vanished from Venus' surface without leaving a trace?

In order to deal with this, so far as I have read, the problem is simply ignored. It would be interesting to learn from Frazier and his learned authorities just what happened to approximately a billion years of erosional materials. Are these scientists now going to say that Venus did not condense from the same kind of nebula materials as the Earth and, therefore, never had water? Are they also going to suggest that in order to start a greenhouse effect, there wasn't any water at all on or in Venus? In order for them to deny this type of erosion as described above, that is exactly what they must do.

Nevertheless, I don't think this evidence will be sufficient for these experts because they can simply respond that Venus never had an ocean based on some assumed concept like its ancient surface froze *ad hoc* geophysical condition in the early solar system. And I am perfectly willing to go along with this suggestion with the comment that it does seem to create problems with the nebula theory of planetary formation and with the established greenhouse effect. Nevertheless, let us cast aside and deny about a billion years of erosion by water and allow that Venus never had water in large amounts, or, say, that the upper crust completely melted when the greenhouse began, or that the planet was turned inside out by an impact 300 million to 500 million years ago. What follows?

Water erosion, it must now be pointed out, is not the only process for forming a regolith. Heat and atmospheric acid in Venus' atmosphere can and do erode rock. According to Eric Burgess, "The rocks of Venus undergo different types of weathering [breakdown]. Chemical weathering would be expected to decompose olivines, pyroxenes, quartz and feldspars into magnesite, tremolite, dolomite, sulfides and sulphates. Mechanical weathering would be expected to disintegrate rocks by spalling and preferential chemical weathering and possibly by wind erosion."¹⁰⁷⁷ According to Bruce Murray, Michael C. Malin and Ronald Greeley, "there can be little doubt that chemical weathering must be very effective on Venus' surface."¹⁰⁷⁸ Paolo Maffei explains that "the atmosphere of Venus also contains—although in small amounts—hydrogen chloride and hydrogen fluoride which, reacting with sulfuric acid could form fluorosulphuric acid, a very strong acid capable of attacking and dissolving almost all common materials, including most rocks."¹⁰⁷⁹ Patrick

¹⁰⁷⁴*Ibid.*

¹⁰⁷⁵Henry S. F. Cooper, Jr., *The Evening Star Venus Observed*, (New York, 1993), p. 120.

¹⁰⁷⁶David H. Grinspoon, *loc. cit.*

¹⁰⁷⁷Burgess, *Venus ...*, *op. cit.*, p. 141.

¹⁰⁷⁸Bruce Murray, Michael C. Malin, Ronald Greeley, *Earthlike Planets*, (San Francisco, 1981), p. 70.

¹⁰⁷⁹Paolo Maffei, *Beyond the Moon*, (Cambridge, Mass., 1978), p. 44.

Moore states that the region below 20 miles in Venus' atmosphere, "is still very corrosive."¹⁰⁸⁰ Bruce Murray, *et al.*, states:

"Seen from the Venera 9 and 10 spacecraft were surfaces littered with rocks and boulders . . . some were angular, some were rounded, suggesting weathering and active surface processes on the planet . . . Apparently Venus' surface is . . . affected by more surface processes than anticipated."¹⁰⁸¹

Roman Smoluchowski states, "Most of the rocks seen in the Venera pictures have smooth and rounded edges, suggesting strong erosion (and corrosion)."¹⁰⁸² Joseph Veverka states that on Venus, "One would expect that under present conditions, (high pressure, temperature, and acidity) weathering would be rapid."¹⁰⁸³ From all of this it is quite apparent erosion is a well observed, well understood process occurring on Venus as all these and other scientists forthrightly state.

Therefore, if Venus did not have an ocean but had a hot-acid atmosphere for nearly all of its history, what would happen? The hot acids would eat away at or erode the rock and again create a regolith. Let us allow a very tiny rate of erosion on Venus due to these erosional processes, say a millionth of a meter per year. Since Venus' surface must have solidified at least about 4.2 billion years ago, this process would have created a depth of 4.2 kilometers or 2.6 miles of regolith. This comes to about 13,800 feet, of planetary soil. If we cut the erosion rate down to half, or one half of a millionth of a meter per year and we still generate 6,900 feet or 1.3 miles of planetary soil. Let us cut this rate down even to half of this, to one quarter of a millionth of a meter per year; we still produce 3,450 feet of regolith; at one eighth of a millionth of a meter per year we erode 1,725 feet of the surface rock; at one sixteenth of a millionth of a meter per year, 431 feet of surface rock; at one thirty-second of a millionth of a meter per year, 215 feet of surface rock; at a one hundred twenty-eighth of a millionth of a meter per year, 107 feet of surface rock is eroded; at a two hundred fifty-sixth of a millionth of a meter per year, 53 feet of planetary soil is created.

Are Frazier and his scientific authorities still going to claim that erosion on Venus is not important in evaluating the age of the planet? Even if as some scientists suggest, Venus' surface for some ad hoc, unknown reason completely melted half a billion years ago, there would still exist a regolith at about one eighth of these computed amounts of planetary soil.

What becomes painfully obvious is that the scientists must assume that for some very special ad hoc reason, acid erosion and heat erosion simply do not erode the rocks on Venus ever! Is that also to be considered logical, scientific, rational or even plausible? I, for one, think not.

But again how do these scientific authorities explain this away? So far as I have read, none of the scientists has actually explained this problem. If Venus did not have an ocean to create water erosion, surely there had to have been some acid or heat erosion. Let us then simply be outrageous. Let us now suggest that not only did Venus never have water erosion and further that it also never had acid or heat erosion, if that is what is required to allow Venus its 4.5 billion year old status. In fact, let us now accept the idea Venus *never* had any kind of erosion throughout its entire history if that will explain the absence of its regolith to suit Frazier and his scientific authorities. But does that mean that Venus should also lack a regolith? Unfortunately, even by going along with the absurd concept that Venus never had erosion on its surface, a regolith must still form by another geological process.

What is that process that is non-eroding yet will build up a soil or regolith? The process is called explosive volcanism. N. Chan, L. Esposito and their colleagues reported in the *Journal of Geophysical Research* that an analysis

¹⁰⁸⁰Patrick Moore, *The Unfolding Universe*, (New York, 1982), p. 58.

¹⁰⁸¹Murray, Malin, Greeley, *op. cit.*, p. 58.

¹⁰⁸²Roman Smoluchowski, *The Solar System, the Sun Planets and Life*, (New York, 1983), p. 43.

¹⁰⁸³Joseph Veverka, *Planetary Geology in the 1980's*, (NASA SP-467), (Washington, D.C., 1985), p. 97.

of observations of sulfur dioxide and haze materials or tiny particles had twice declined by large amounts from Venus' atmosphere.¹⁰⁸⁴ To explain the growth and decay of these materials in Venus' atmosphere, Esposito states,

"The best explanation right now for the decrease is that from time to time major volcanic eruptions inject sulfur dioxide gas to high altitudes. The haze comes from particles of sulfuric acid, which is created by the action of sunlight on sulfur dioxide . . . Being heavy the particles gradually fall out of the upper atmosphere, letting conditions return to normal between eruptions.

"My calculations show that this eruption of the late 1970's was at least as large as the 1883 eruption of Krakatoa. The explosion, equal to a 500-megaton H-bomb, was the most violent of the last century or so, shooting vast quantities of gas into Earth's stratosphere."¹⁰⁸⁵

James Pollack describes these changes discussed by Esposito in Venus' atmosphere. "Such fluctuations might be due to episodic injections of SO₂ high in the atmosphere by powerful volcanic eruptions."¹⁰⁸⁶ Even David Morrison, who earlier attacked Velikovsky so virulently claims, "When these observations are combined with indications of volcanic topography and lightning discharges from possible volcanic plumes, the case for erupting volcanoes on Venus becomes rather strong."¹⁰⁸⁷ Yet in the same book he claims Venus' crust froze long ago to halt volcanism and with it the escape of argon-40, and yet he called the ad by Doubleday "outright lies." Somehow Morrison does not believe it is an outright lie to say "the crust of Venus became frozen at an early stage of its evolution to halt volcanism" while saying in the same book, evidence for "erupting volcanoes" is "rather strong." Doubletalk apparently comes easily to some scientists.

Let us examine this a little more deeply regarding the origin of argon-40 in Venus' atmosphere. The question is, where did Venus' atmosphere come from? According to Roman Smoluchowski, writing for the Scientific American Library:

"Why does Venus have so thick an atmosphere and how did it originate? Since the primary (primordial) atmosphere of Venus and of the other terrestrial planets was lost when the solar [stage] T-Tauri wind blew them off soon after the planets were formed, the present secondary atmospheres are of different origin. UNDOUBTEDLY THEY ARE THE RESULT OF VOLCANISM and of the subsequent heating by stored internal heat and solar radiation. Besides lava and rock, VOLCANOS EMIT LARGE AMOUNTS OF GASES"¹⁰⁸⁸
(Capitalization added)

¹⁰⁸⁴N. Chan, L. Esposito, T. Skinner, "International Explorer Observations of Venus SO₂ and SO," *Journal of Geophysical Research*, Vol. 95, (May 20, 1980), p. 7485.

¹⁰⁸⁵L. Esposito, "Does Venus Have Active Volcanoes," *Astronomy*, (July 1990), p. 45.

¹⁰⁸⁶James Pollack, "Atmospheres of the Terrestrial Planets," *The New Solar System*, 3 ed., J. K. Beatty, A. Chaikin, (New York, 1990), p. 93.

¹⁰⁸⁷Morrison, Owen, *op. cit.*, p. 235.

¹⁰⁸⁸Roman Smoluchowski, *The Solar System*, (New York, 1983) p. 45.

Venus has an atmosphere 90 times as massive as that of the Earth. Hence, it degassed either through Volcanism or heat, 90 times the amount of gaseous material into its atmosphere from inside the planet than did the Earth. Of course, it would have outgassed a tremendous amount of argon-40. However, Morrison claims Venus' crust froze solid preventing argon-40 from being degassed while he understands Venus outgassed 90 times more gaseous material from its crust than is contained in the Earth's atmosphere. Holding to such a concept is simply absurd.

There can be no doubt that Venus possesses an enormous number of volcanos. As Grinspoon describes from Magellan's photographs:

"Perhaps the most striking feature of the Venusian landscape is the prevalence of volcanism. About 80 percent of the surface is made up of volcanic terrain ranging from the curiously familiar to the downright bizarre."¹⁰⁸⁹

As one investigator said after looking at the Magellan photographs of Venus, "‘Everyone says Olympus Mons on Mars is the biggest volcano in the solar system,’ he said, . . . ‘It isn't. Venus is. The entire planet is one big volcano.’"¹⁰⁹⁰ Venus has about one hundred thousand volcanic structures and it seems almost certain that they produce powerful eruptions as they do on Earth, as Esposito and Morrison claim. However, volcanos not only erupt gases such as argon-40 and molten lava, but they also generate enormous amounts of dust and ash. For example, when Krakatoa erupted in 1883 it "threw into the air nearly 5 cu. mi. [cubic miles] . . . of rock fragments and large quantities of ash fell over 500,000 sq. mi."¹⁰⁹¹

Many of us have seen the film of Mount St. Helen's eruption and the tremendous amount of dust material thrown into the atmosphere and spread around the region of the American northwest. Let us take a conservative estimate, say 10 cubic miles of detritus is generated in this way per century. What do we find? In 4.3 billion years, Venus would have generated 430 million cubic miles of detritus. Given the surface area of Venus, according to Dr. Barry Frank, physicist of Concordia College, Montreal, Canada, Venus would have been buried under about 14,000 feet of volcanic detritus. If we cut this volcanic eruption rate to 5 cubic miles per century, we still generate 7,000 feet of regolith. At 2.5 cubic miles per century, 3,500 feet of regolith; at 1.25 cubic miles per century, 1,750 feet; at 0.625 cubic miles per century, 875 feet; at 0.3125 cubic miles per century, 437.5 feet of surface regolith.

That is, at a rate of explosive volcanism far lower than presently occurs on Earth, Venus would still have a regolith. Are Frazier and his scientific authorities now going to suggest that Venus, being as old as the Earth, had absolutely no explosive volcanism? Do they now wish everyone to accept the concept that Venus never had water erosion, or acid erosion and now also no explosive volcanism? Do they really think that anybody is willing to believe such an incredible scenario? And again, even if we reduce the time of explosive volcanism to half a billion years, we still derive 875 feet to over 50 feet of regolith from the estimates. What seems rather obvious is that any reasonable amount of explosive volcanism on Venus will generate a regolith. But where is this material to be found if Venus is 4.5 billion years old? Where has it gone?

And again, how do these scientific authorities explain this away? Again, so far as I have read, none of the scientists has actually explained why all this material is missing. Is it logical, scientific, rational or even plausible to suggest that with several tens of thousands of volcanoes spread over its surface and an age equal to about that of the Earth, that Venus never produced explosive eruptions that laid down even a few feet of material—dust and debris, not only lava—over the planet? Having a small area covered by volcanic material as an answer simply will not suffice. Most of the planet should be covered by a regolith. Again this is hardly credible. So what must one do to explain away this inconsistent evidence of volcanism? Let's bend over backwards to absurdity once again and do away not only with oceans causing erosion, and hot acid causing erosion but also get rid of explosive volcanism; though to do so just to suit the requirements of these scientific authorities strikes me as losing touch with reality. But, having done so, does this

¹⁰⁸⁹Grinspoon, *op. cit.*, p. 23.

¹⁰⁹⁰Henry S. F. Cooper Jr., *op. cit.*, p. 180.

¹⁰⁹¹"Krakatoa," *Encyclopedia Britannica Micropedia*, Vol. V, (Chicago, 1982), p. 909.

"save the appearances" that Venus has been in its present orbit for 4.5 billion years? Again, I'm afraid Frazier and his experts will not be happy to learn that in spite of all these strong measures to "save the appearances" for an ancient Venus, the planet would still have a regolith.

What planetary process, one may rightly ask, will somehow generate this material? This time it is dust from interplanetary space. As reported in *Sky & Telescope*, "Recently, the amount of dust swept up by the Earth and to a lesser amount by the Moon was found to be 40,000 metric tons per year based on rather decisive measurements."¹⁰⁹² Of course, if the Earth sweeps up 40,000 metric tons of space dust each year, it is quite clear that Venus must also sweep up a similar or comparable amount of dust, as well. Do Frazier and his scientific experts now wish us to believe that there was never interplanetary dust in space near Venus' orbit for the past 4.5 billion years? Do they really expect anyone to believe meteors and comets did not shower the inner solar system with dust, or that meteors never fell into Venus' atmosphere for eons creating detritus?

Furthermore, scientists contend that there are about 920 or so impact craters on Venus which represent half a billion years of impacts. That suggests that, over 4 billion years, about 8,000 major impacts occurred on Venus throwing billions of tons of dust and debris into the atmosphere which would have added to the regolith. And what about those comets that came close to Venus? Wouldn't they also add dust to its surface over 4.3 billion years? Even in a half billion years we should still end up with a few feet of planetary soil covering the planet.

Now these processes may not produce the kinds of depths of regolith created by water erosion, acid and heat erosion or volcanic eruptions which would lay down substantially greater regoliths. However, even this smaller amount of dust would produce a regolith several feet deep over a span of 4.3 billion years. *Again, I ask these scientific authorities: where is it?* Is it rational, logical, or plausible to say that dust comets or asteroids fall on the Earth from space, but somehow they managed to avoid falling on Venus? I again think not. Either there has been no dust, meteors, comets or asteroids in orbits crossing Venus' orbit in space for the past 4.5 billion years, or Venus is a newborn planet that has not had sufficient time to gather this dust. Under no rational analysis is it possible, probable or plausible to exclude this fundamental, simple evidence that even a simple lay person in science can understand. If Venus had been in its present orbit for 4.5 billion years, by all these processes discussed above, it would have a deep regolith. There is actually no process of recementing any planetary soil back to rock with the same appearance as rock. Bruce Murray had to resort to "*Unknown processes* of topographic renewal [that] evidently managed to outstrip degradation [erosion] and burial on Venus."¹⁰⁹³ (Emphasis added) The scientists, of course, will invent a theoretical model to "save the appearances" that Venus' regolith was recemented to look exactly like lava flows and rock formations. But this is simply creating anything and everything possible or barely possible to avoid the clear implication of a volcanic surface without a regolith on Venus. Darwin, I believe, was quite right in saying one should be "slow in admitting the conclusion of astronomers." As Digby McLaren states with respect to the case of erosion presented by Darwin,

"It demonstrates that there is an innate prejudice in favor of deduction from existing models, or even axioms, over observations from observed facts."¹⁰⁹⁴

This is what Frazier and his authorities have done and must do with erosional processes on Venus. They will create any process based on the innate prejudice of their deduction that Venus is ancient over the clear observational fact that Venus lacks a substantial regolith and appears to be a newborn planet!

In order to "save the appearances" of their theory that Venus is 4.5 billion years old, all these processes that create a planetary soil must be abandoned. To offer any *ad hoc* notion to escape squarely facing this fact is to be in a state of denial.

¹⁰⁹²"Earth's Cosmic Dusting," *Sky & Telescope*, (March 1994), p. 13.

¹⁰⁹³Bruce Murray, *Journey Into Space*, (New York, 1989), p. 126.

¹⁰⁹⁴McLaren, *op. cit.*, p. 124.

I must now suggest that Kendrick Frazier and his scientific authorities are really behaving as the Aristotelian professors did in the time of Galileo. Galileo showed those learned authorities of that time through his telescope, unimpeachable evidence that Venus does not have an orbit around the Earth, as they had so authoritatively proclaimed. But, to avoid what would have been a painful fact, they refused to look through the telescope. Others said the telescope did not show things correctly; as Frazier's experts say, erosion is not an important issue regarding Venus' age. But above all, they clung to any *ad hoc*, even absurd explanations to avoid dealing with the clear evidence that Galileo presented.

The simple fact that Venus should have a regolith covering nearly all of its surface, if Venus is an ancient body, is a fundamental and obvious datum that anyone, even an expert can understand, except those unalterably blinded by their theories and dogmatically opposed to the implication such a finding has for his or her deeply held beliefs that Venus *must be* an ancient body. What Frazier and his authorities have done is merely take their assumption that Venus is 4.5 billion years old and turn it into fact. But Billy P. Glass does write that their contention is only a theoretical assumption in that, "the geologic history of Venus . . . [is] based primarily on what we have learned about the other planets *and is necessarily highly speculative. We assume that Venus formed 4.5 X 10⁹ y. ago.*" (4.5 billion years ago).¹⁰⁹⁵ (Emphasis added)

This admission by Glass highlights the underlying problem for Frazier and his scientific authorities, as explained years ago by Alan E. Nourse.

"One of the most difficult problems that scientists face is removing the human factor from their work. Try as they will to keep their investigations strictly scientific, they seldom achieve this ideal. And there are few scientific disciplines where human or personal factors get in the way so badly as in the field of astronomy.

"Time after time we have seen a certain pattern repeated as our knowledge of the solar system and the planets has grown up bit by bit: the pattern of the 'fashionable' school of thinking.

"And we see the same pattern repeated again and again when we consider the question of Venus

"Part of the problem has been the very human (if not too scientific) temptation to make certain assumptions without any proof, and then to build up attractive theories on the basis of them as though they were not really assumptions at all but facts."¹⁰⁹⁶

Taking their 4.5 billion year old Venus "assumption as fact," Kendrick Frazier, as leading pallbearer, with the efforts of Edward Anders, A. G. W. Cameron, William M. Kaula, David Morrison, George O. Abell, Vance I. Oyama, and Michael B. McElroy, less than a year after Velikovsky's death, tried with might and main to *really* bury Velikovsky with unsubstantiated statements about Venus; but they failed to do so because one cannot bury a man or his ideas by these methods regarding Venus because Venus does not have a planetary soil with which to bury anything!

Frazier then goes on to cite Michael B. McElroy that, of all things, "A pronounced—'greenhouse effect' . . . has been strongly supported by the Pioneer Venus data."¹⁰⁹⁷ But as we pointed out with citations in the discussion of James Oberg, all the Pioneer Venus probe measurements showed that Venus is giving off more heat at all levels than is derived from sunlight. Like the attitude regarding the lack of a regolith on Venus, by ignoring this evidence, McElroy accepts

¹⁰⁹⁵Glass, *op. cit.*, p. 324.

¹⁰⁹⁶Alan E. Nourse, *Nine Planets*, (New York, 1960), pp. 90-91.

¹⁰⁹⁷Frazier, *op. cit.*, p. 35.

the changes or dismisses all probe measurements that showed only thermal imbalance. As a true believer, he apparently feels that one must not only disregard the probe's readings which support Velikovsky's thermal imbalance concept, (that Venus was the source of much or most of the heat), he must then say that the heat on Venus was the result of the greenhouse effect (a "pronounced greenhouse effect . . . has been strongly supported by the Pioneer Venus data"), which is clearly his fabrication of what the probe's readings actually measured. This is the second time this data was presented in *The Skeptical Inquirer*, which is a complete "distortion" of what was actually found! Frazier, of course, presents the claim that it is the Doubleday ad that is a "distortion."

This behavior is quite inexcusable and totally fraudulent. To destroy another man's ideas by the repeated presentation of disinformation is dangerous to science and is sheer "deceit"! Let us consider the case in which another scientist destroyed the work and reputation of one of his colleagues by the same kind of tactic. Robert A. Millikan, a physicist at the University of Chicago, won the Nobel prize in 1923 by determining the electric charge on the electron. He became a scientific giant in his time winning many prizes and honorary degrees. According to William Broad and Nicholas Wade in their chapter "Deceit in History,"

"As an unknown professor at the University of Chicago, Millikan published his first measurement of e , the electronic charge in 1910. The measurements, which depended on introducing droplets of liquid into an electric field and noting the strength of field necessary to keep them suspended, were difficult to make and subject to considerable variation. In strict accordance with the ethos that demands full disclosure of data, Millikan used stars to grade the quality of his thirty-eight measurements from 'best' to 'fair,' and noted that he had discarded seven entirely.

"The candor did not continue for long, Millikan's rival in measuring electric charge, Felix Ehrenhaft of the University of Vienna, Austria, immediately showed how the variability in Millikan's published measurements in fact supported Ehrenhaft's belief in the existence of subelectrons carrying fractional electronic charges. Battle was joined between Millikan and Ehrenhaft, and the question of subelectrons was discussed around the scientific world by leading physicists such as Max Planck, Albert Einstein, Max Born and Erwin Schrödinger.

"To rebut Ehrenhaft, Millikan published an article in 1913 full of new and more accurate results favoring a single charge for the electron. He emphasized, in italics, that 'this is not a selected group of drops but represents all of the drops experimented upon during 60 consecutive days.'

"On the face of it, Millikan had achieved a brilliant rejoinder to Ehrenhaft and had proved beyond a doubt the correctness of his measure of the electron charge—all through the sheer power of scientific precision. However, . . . Harvard historian, Gerald Holton went back to the original notebooks on which Millikan based his 1913 paper and found major gaps in the reporting of data. Despite his specific assurance to the contrary, Millikan had selected only his best data for publication. . . . The 58 observations presented in his 1913 article were in fact selected from a total of 140. Even if observations are counted only after February 13, 1912, the date the first published observation was taken, there are still 49 drops that have been excluded.

"Millikan had no need to worry that his deceit would be exposed, for as Holton notes, the 'notebooks belonged to the realm of private science Therefore he evaluated his data . . . guided both by a theory about the nature of electric charge and by a sense of the quality or weight of the particular run. It is exactly what he had done in his first major paper, before he had learned not to assign stars to data in public.'

"Across the Atlantic, meanwhile, Ehrenhaft and his colleagues assiduously published readings, good, bad and indifferent. The picture that emerged from their work did not support the notion of a single, indivisible electric charge. This view was contrary to prevailing theory at the time and, as Holton notes, 'from Ehrenhaft's point of view it was for just this reason, to be regarded as an exciting opportunity and challenge. In Millikan's terms, on the contrary, such an interpretation of the raw readings would force one to turn one's back on a basic fact of nature—the integral character of *e*. . . .'

"For Millikan, the battle ended in a Nobel prize (which also cited his work on the photoelectric effect); for Ehrenhaft, in disillusionment and eventually a broken spirit."¹⁰⁹⁸

Frazier and McElroy, in their attempt to argue against Velikovsky, had no qualms in presenting as true the idea that the greenhouse effect is strongly supported by the Pioneer Venus probes, in spite of the fact that all the probes' readings have contradicted the runaway greenhouse concept from the cloud tops to the surface. Broad and Wade suggest that such behavior has "alarming implications."

"The example of Millikan and the other adepts of science who cut corners in order to make their theories prevail contains some alarming implications. . . .

"History shows that deceit in the annals of science is more common than is often assumed. Those who improved upon their data to make them more persuasive to others doubtless persuaded themselves that THEY WERE LYING ONLY IN ORDER TO MAKE THE TRUTH PREVAIL. (Capitalization added) But almost invariably the real motive for the various misrepresentations in the history of research seems to arise less from a concern for truth"¹⁰⁹⁹

As I pointed out earlier, Cyrus Gordon said that scholars should not take anyone's word on faith. Looking at the statements by Oberg and McElroy about probes to Venus and their so called support for the runaway greenhouse effect, it appears Gordon knew well the games editors and science experts play.

One other point raised by Vance I. Oyama was that Velikovsky's claim that hydrocarbons are and would be found in significant amounts in Venus' atmosphere has been disproved by the mass spectrometer because hydrocarbons

¹⁰⁹⁸William Broad, Nicholas Wade, *Betrayers of the Truth*, (New York, 1982), pp. 34-35.

¹⁰⁹⁹*Ibid.*, pp. 35-36.

are found at "Such drastically low [small] levels . . ." on Venus.¹¹⁰⁰ However, the abundance of methane, according to T. M. Donahue and his collaborators, "characterized the findings [of large amounts of methane] as so surprising that they were loath to publish them *Scientists had known for years that the spectrometer had recorded a sharp rise in methane beginning about 14 kilometers above the surface of Venus.*"¹¹⁰¹ (Emphasis added) Like the findings of thermal imbalance at all levels in Venus' atmosphere which were then cast aside, Vance I. Oyama with the other scientists, realizing that this reading supported Velikovsky, by finding a sharp rise in methane on Venus, decided to cast it aside as well. But now these same scientists have found that this reading is indeed correct.

What they have done is claim that the Venus probe just happened to come down propitiously over a volcano which emitted a great deal of methane. "Donahue estimates that a volcano eruption spewing out the amount of methane found by the Pioneer Venus would occur only about once every 100 million years."¹¹⁰² Now scientists have a strong aversion to supporting concepts that are highly improbable. William H. Stiebing, Jr., a critic of Velikovsky, relying on Carl Sagan's criticisms stated, "Scientists are used to evaluating explanations of data in terms of probability rather than possibility, so the evidence would have to be clear and unambiguous to convince them that a theory like Velikovsky's is valid."¹¹⁰³ But this probability injunction has not stopped the scientists involved from suggesting that a Pioneer Venus probe with a mass spectrometer just happened to descend into Venus' atmosphere right near a volcano which would only erupt massive amounts of methane once in 100 million years. Is this approach to evidence rational? I think not.

Donahue adds, "It is embarrassing to invoke such a wildly unlikely event as a chance encounter between an entry probe and a rare, geologically confirmed methane plume," ¹¹⁰⁴ But no one could ever embarrass Kendrick Frazier or Vance I. Oyama by pointing out the high improbability of what is being proposed.

To provide for their view that Venus is ancient, the scientists have ignored the erosion by Venus' early oceans, the erosion of its surface by hot acids, explosive volcanism creating a regolith and interplanetary dust falling from space to produce a regolith. To save the runaway greenhouse effect they cast aside every reading by the Venus probes that show Venus is in thermal imbalance. And now, to save their contention that since large amounts of methane have been found in Venus' atmosphere, the Venus Pioneer probe miraculously came down over a methane spewing volcano that would only degas this large amount of methane just once every 100 million years. As scientific authorities, I suggest they are not creating a very good impression by such unrealistic and bombastic suggestions.

Kendrick Frazier adds:

"Even if there had been any serious questions about Venus' age, they would have been settled this past spring. On May 28, scientists announced that Pioneer Venus satellite's radar indicated the presence of an apparently ancient supercontinent on the planet, with a crust that could be 4 billion years old."¹¹⁰⁵

According to Frazier, Venus' crust *could be* 4 billion years old without erosion and contains a supercontinent seemingly of the same age. Now this supercontinent has two very large high regions named Ishtar and Aphrodite. These high regions are several thousand feet above the lower continental planes. But what Frazier has very carefully omitted is that the great heat on Venus and its surface gravity would have removed the continental high regions billions of years ago if Venus was as ancient as he claims. According to George E. McGill, *et al.*, in a discussion of how heat and gravity cause high regions to flow gradually to lower levels,

¹¹⁰⁰*Ibid.*, p. 36.

¹¹⁰¹*Science News* (September 12, 1992), p. 172.

¹¹⁰²*Ibid.*

¹¹⁰³William H. Stiebing, Jr., *Ancient Astronauts, Cosmic Collisions and Other Popular Theories About Man's Past*, (Buffalo, N.Y., 1984), p. 60.

¹¹⁰⁴*Science News* (September 12, 1992), *op. cit.*

¹¹⁰⁵Frazier, *op. cit.*, p. 35.

"Creep rates of rocks are strongly temperature dependent. If the high surface temperature of Venus implies a much hotter crust . . . then the entire crust of Venus may be much less resistant to creep than the crust of the Earth Cordell and Solomon . . . *et al.*, have shown that large features such as impact basins will suffer essentially complete relaxation [flattening] of their topographic relief in times on the order of 1 Gy [1 billion years] even if the [Venusian] crust is dry. . . .

"THIS SUGGESTS THAT PRESENT ELEVATED REGIONS ARE YOUNG (Capitalization added) or continuously renewed, and that (unless we illogically assume that the relief of Venus is uncharacteristically high at present) elevated regions would have risen and spread to oblivion many times during the history of Venus."¹¹⁰⁶

In this scientific analysis of the evidence from Venus, McGill and his colleagues say in complete contradiction to Frazier that it is quite "illogical" for Venus' crustal regions of high altitude to have existed in this condition for billions of years. Viscous flow of rock would have removed these high regions billions of years ago. And they say this, in spite of not knowing the water content or other volatiles in the crust which would make these regions flatten even more rapidly. To make matters worse, for this interpretation that Venus is an ancient planet, R. J. Phillips, *et al.*, make this explicit statement regarding the viscous nature of the Venusian crust. They state that if the high surface "features such as Venus' high continents were held up by passive means and not internal convection [upwelling], creep within the lithosphere requires that they could not be 4.5 billion years old *but at most 10 million years.*"¹¹⁰⁷ (Emphasis added)

Let us put Frazier's supposition into perspective. If Venus formed a crust 4.2 billion years ago, then based on Phillips' analysis, the continental structures on the planet had to rise thousands of feet and afterward flow away to oblivion 420 times! Is it rational, reasonable or plausible to suggest that entire continents, untold millions of cubic miles in volume, did this 420 times? If we double the rate of relaxation to 20 million years, we still have continents rising thousands of feet and flowing to oblivion 210 times; at four times the rate of relaxation or 40 million years, the continents rise and flow away 105 times; at eight times the rate of relaxation or 80 million years, the continents rise and flow away 52 times; at sixteen times the rate of relaxation or 160 million years, the continents rise and flow way 26 times. And if we assume Venus was covered over with lava 300 to 500 million years ago, the continents would have flattened in one-eighth of all these values. This, I humbly point out, is not rational, reasonable or plausible, nor scientific. Frazier's geological argument requires just such irrational scenarios. Of course, *ad hoc* theories which make Venus' surface rock more rigid have now been invented to "save the appearances" that Venus' continents are ancient.

For the formation of continents to take place, either there must be some immense force pushing upward or immense lava flows. If Venus is an ancient planet, as Frazier and his scientific authorities suggest, it would have a very thick crust, and once the continents had flowed away they would not reform again. They must rise faster than they flow away! The suggestion that such lifting forces or lava flows exist is simply hand waving! Whatever devices Frazier and his scientific authorities invent to "save the appearances" that Venus' crust is ancient should be just as entertaining and just as out of touch with reality as the rest of their thoughts on Venus! And to have Venus' high regions rise and flow to oblivion, over and over again, to explain its ancient age, I, once again, suggest is not logical, plausible or scientific! Frazier has simply ignored this well-known process to suggest that the surface of Venus is ancient. This process flattens any such suggestion!

¹¹⁰⁶George E. McGill, *et al.*, "Topography Surface Properties and Tectonic Evolution," *Venus*, ed. D. M. Hunten, *et al.*, (Tucson, 1983), pp. 95-96.

¹¹⁰⁷R. J. Phillips, *et al.*, "Tectonics and Evolution of Venus," *Science*, Vol. 212, (1981), pp. 879-887.

In order for Venus to be an ancient planet, we would have to believe Venus never had oceans, that Venus' surface was never eroded by hot acid; that Venus never had explosive volcanism to vent argon-40 or generate dust; that no interplanetary dust fell or large comets, meteors or asteroids struck the planet for 4.5 billion years; that all the Venus probe readings which showed that Venus is giving off more heat than sunlight can provide are completely wrong. That is, we must discard those readings. We must also assume that the Venus probes that measured high contents of methane, a hydrocarbon gas in the atmosphere, miraculously descended on a volcano (that theoretically should not be generating dust and argon-40) which would emit methane in large amounts only once in 100 million years, that gravity and heat did not cause the Venusian crust to rise and flow away again and again so that the high regions could survive for billions of years. All this is supposed to be rational, scientific, plausible—and correct.

To add insult to injury, Frazier then adds,

"A NASA research scientist prominently involved in the Pioneer Venus mission said, 'Quite frankly, I don't want to get involved in anything to do with the subject of Velikovsky. . . .' As for the Doubleday ad, this scientist said simply, 'I don't know where they got some of their claims. There were a number of misstatements.'¹¹⁰⁸

After claiming he or she does not want to be involved with anything to do with Velikovsky, what does the prominent anonymous Pioneer Scientist do? He/she gets involved by saying, again without scientific support of any kind, that the Doubleday ad contains "a number of misstatements." Frazier has allowed an anonymous attack on the veracity of the Doubleday ad to be published in *The Skeptical Inquirer*. We are not told what the mistakes are or who is making the accusation, which is simply a political smear technique. No court in the United States would allow such hearsay to be presented, but Frazier is careful to present just such hearsay.

Frazier then cites Walter M. Elsasser who claims, "I do not think that Velikovsky as a person had any intent to defraud. . . . He is undoubtedly a man of phenomenal intelligence; and it is a tragedy to see such talent being used in an utterly asocial manner as he did trying to give his personal fantasies a 'scientific validation.'¹¹⁰⁹

Here Frazier makes the typical gesture while still savaging Velikovsky, that the work he has promulgated is not a personal attack on Velikovsky the man. But how, by calling his work "asocial" and given this diatribe can the two be separated?

William Glen, the science historian, has this to say about the level of nastiness carried on by modern scientists which clearly applies to the establishment scientists' behavior toward Velikovsky:

"Most of the sociology and social psychology of science is not different from that of the butcher's or baker's shop, except for one exception. Scientists have a much greater personal investment. Much more of their dignity and esteem are tied up in what is at stake here, and their reactivities are so much stronger than those of the less deeply invested. . . .

"Of course the whole thing is mainly civil in print, almost gracious at points. When you go around talking to people involved you can see how deeply concerned they are. It's not so much a matter of attacking people; what you see is almost the phenomenon of a person having an idea who ceases to be a

¹¹⁰⁸Frazier, *op. cit.*, p. 37.

¹¹⁰⁹Frazier, *op. cit.*, p. 38.

person—he becomes that idea. Very often the adversaries are not badly disposed to the person as such; it's the *idea* that's so terribly threatening."¹¹¹⁰

The tone of vituperation in Frazier's piece reflects well on how deeply disturbed the scientists are and how their politeness masks their barely contained, petty hostility toward Velikovsky.

Therefore, permit me to cite a scientist *by name* who understands well this type of criticism. He is not speaking about the Velikovsky issue, but about the level to which modern science and modern scientists have descended in order to carry on debates. R.A. Lyttleton states,

"In order to be a famed . . . scientist and belong to the inclusive club of fully accepted . . . scientists in their *unknown* thousands, one must kneel on the hassock and swear allegiance to certain tenets regardless of any scientific consideration. . . .

"These [tenets] must be held with religious fervor, dissenters are just not to be tolerated, the devotees feeling it their right and indeed duty, to defend the creed against all criticism by any means of chicanery and of sharp-practice within their power, however crude and improper, so long as they judge they can get away with it, but all the time representing themselves to the world as acting with judicial calm in the best interests of science."¹¹¹¹

With regard to Kendrick Frazier's ugly criticism and the ugly remarks of his scientific authorities, Lyttleton's description of how scientists and their supporters in the press behave has hit the nail right on the head. As the historian of science, George Sarton wrote about such behavior, ". . . there is no excuse for lies, and the lies used to illustrate a false idealism are of all lies the worst ones."¹¹¹²

* * * * *

CHINA AND VELIKOVSKY

"Faithfulness to the truth of history involves far more than a research, however patient and scrupulous, into special facts. Such facts may be detailed with the

¹¹¹⁰*The Mass Extinction Debate: How Science Works in a Crisis*, (Stanford, Calif., 1994), ed. William Glen, pp. 284–285.

¹¹¹¹Raymond A. Lyttleton, "Geophysics the Sick Man of Science", *I.S.C.D.S.*, [International Stop Continental Drift Society], Vol. 5, (December 1984), p. 3.

¹¹¹²George Sarton, *Ancient Science Through The Golden Age of Greece*, (New York, 1993), Dover reprint, p. 426.

most minute exactness and yet the narrative taken as a whole, may be unmeaning or untrue.¹¹¹³

"He [the Higher Man] requires that in what he says there should be nothing inaccurate.¹¹¹⁴

"It has been said that though God cannot alter the past, historians can."¹¹¹⁵

In the spring of 1987, *The Skeptical Inquirer* presented a critique of Velikovsky's historical research respecting ancient China by Henrietta Lo, a reference librarian at Meriam Library, at California State University, at Chico. Ms. Lo has examined the historical documentation of Chinese history and has attempted to employ it to illustrate her contention that Velikovsky's catastrophic scenario is contradicted by this evidence. This would indeed, be a serious blow to Velikovsky's historical reconstruction of the past if Lo's case were solidly based. Unfortunately, Lo's research is so desperately ignorant, inadequate, sloppy and laden with errors that its value is good only as anti-Velikovsky propaganda. It is in terms of its propaganda value that it displays its worth as an example of how honest research should not be done. The point I wish to emphasize is that *The Skeptical Inquirer* apparently will publish anything resembling research, no matter how shoddy or error ridden, if it purports to show Velikovsky's work is mistaken. Therefore, let us not delay longer and get down to the criticism.

Lo claims that reference to her footnote number 9, "Velikovsky states that the *Book of Historical Documents* was rewritten from memory or hidden manuscripts after the burning of books by Shi Huang-Di (the First Emperor, r. –246 to –211), who unified China under the Qin Dynasty." (9)¹¹¹⁶ Reference (9) is to *Worlds in Collision*, page 116.¹¹¹⁷ Velikovsky suggested these documents were later found in a grave.

I too will use the same pocketbook edition as did Lo in this article. In the other articles I will use the hard cover Doubleday edition.

Lo then states, "If he [Velikovsky] had made an effort to verify this allegation, he would have learned that the First Emperor's Edict of 213 B.C. which aimed at enforcing 'thought control' to keep people ignorant, applied to the books owned by commoners only. The books that were in the state archives or in the collections of court scholars remained intact."¹¹¹⁸ Therefore, according to Lo, there were no hidden documents that later came to light or found in a grave; but Lo then, in the very next sentences, discusses hidden documents of this same *Book of Historical Documents* which came to light after the death of Shi Huang-Di of the Qin Dynasty.

"Documents . . . surfaced decades after the Qin Dynasty [these were] . . . replaced by the Han (206 B.C.), were found to differ from the edition adopted . . . The various pieces in the *Documents* were gathered mainly around the –8th

¹¹¹³Francis Parkman, "Introduction," *Pioneers of France in the New World*, (1865).

¹¹¹⁴Confucius in Will Durant, *Our Oriental Heritage*, (New York, 1954), p. 669.

¹¹¹⁵Samuel Butler in Rush Limbaugh, *See, I Told You So*, (New York, 1993), p. XV.

¹¹¹⁶Henrietta Lo, "Velikovsky's Interpretation of the Evidence Offered by China in his *Worlds in Collision*," *The Skeptical Inquirer*, Vol. 11, (Spring 1987), p. 284.

¹¹¹⁷*Ibid.*, p. 290.

¹¹¹⁸*Ibid.*, p. 284.

century, and the collected work was later edited by Confucius . . . according to Chinese tradition."¹¹¹⁹

Elsewhere Lo states,

The *Annals* . . . compiled during the early -200's and found in his grave in +281."¹¹²⁰

Does this sound as if the *Annals* were not discovered in a grave?

Thus after saying there were no hidden documents found in a grave to refute Velikovsky, Lo tells us that "Documents .. surfaced decades after the Qin Dynasty," which were different than the earlier ones and were edited into the earlier material also found in a grave of later times. The logic seems to be that according to Lo, there were no hidden documents to attack Velikovsky in spite of her admission that unknown, meaning "hidden," documents surfaced later. What Lo has carefully omitted is what Velikovsky actually stated even earlier on this matter. He stated that the "STORY PERSISTS that a few remnants of the old literature were again put into writing from the memory of an old man; SOME [documents] WERE SAID TO HAVE BEEN FOUND HIDDEN in the sepulcher of Confucius and ARE ASCRIBED to his pen."¹¹²¹ (Capitalization added)

When Velikovsky used the introductory statements such as the "story persists that," some documents "were said to have been," or "are ascribed to" other sources which stated these ideas, it becomes clear that Velikovsky did not make these allegations regarding these documents but that others did. Velikovsky's two pages later cited by Lo then repeated this statement that books were "rewritten from memory or from some hidden manuscripts"¹¹²² and omitted the earlier caveats, but he originally claimed that these historical concepts were stories, attributions etc. Then why didn't Velikovsky know that the First Emperor, Shi Huang-Di of the Qin Dynasty, did not burn the books in the state archives or those of scholars? The answer is found in Lo's reference number (10) which states,

"Ch'ien Ssu-ma, 'History of the First Emperor,' in the *Memoirs*. Even though the Qin state archive was burnt when a rebel leader fired the palace about -206, copies of ancient texts in official and even private collections did survive."¹¹²³

In other words, this book was in the Qin state archive where it could be used by later scholars according to Lo. But at the same time this book in the Qin state archive could not be used by later scholars because the archive had been burned down according to Lo. So what is the actual source of Lo's evidence that this document was in the "collections of court scholars," "in official and even private collections"? There is no source or page number given either in the article on page 284 where she made the allegation, or in footnote 10 where she repeated the allegation. Lo's proof for what she stated on this matter is herself! Thereafter, Lo claims Velikovsky's work "is ridden by . . . the problem of scholarship . . ." This does give one pause not only to doubt Lo's accuracy, it gives one pause to doubt the integrity of Lo's intentions of dealing fairly and ethically with Velikovsky.

¹¹¹⁹*Ibid.*

¹¹²⁰*Ibid.*

¹¹²¹Immanuel Velikovsky, *Worlds in Collision*, (New York, 1950), p. 114.

¹¹²²*Ibid.*, p. 102.

¹¹²³Lo, *op. cit.*, p. 290.

To be more specific, Lo goes on to point out that Velikovsky's chronology must be in error because the Emperor,

"Yao preceded the first celestial war [presented by Velikovsky in *Worlds in Collision*] by nine centuries. Velikovsky's synchronizing efforts [of having a Chinese catastrophe correlate with records of other catastrophes from other parts of the world] would not work, particularly in this case, because there is simply no standing room for Yao in the middle -1000. The millennium-old Chinese practice of ancestor worship required that the Chinese carefully record their genealogies, especially those of the priest - kings. The *Memoirs* compiled by Ssu-Ma-Chien (ca. -145 to -86) who, as the Han official historian, had access to the state archives, is widely recognized as a reliable source even for the Shang period trad[itionally dated] -1623 to -1027, as proven by oracle bone and bronze inscriptions, excavated around the early + 1900's. It contains a complete genealogy of the Shang monarchs. Yao does not appear on the list."¹¹²⁴

This is a fascinating piece of logic on Lo's part. Velikovsky has maintained that the historical chronology of ancient China as known in 1950 was not correct nor known to be accurate beyond 776 B.C. He, therefore, offered that the Emperor Yao who came *before* the Shang Dynasty, reigned around 1403 B.C. However, Lo, who raised this issue, cited the information regarding this king's list of the Shang Dynasty to her reference (11) Kwang-chih-Chang, *Shang Civilization*,¹¹²⁵ However, if Lo had taken a moment to reflect this source for her king's list, she should have noted it was published in 1980—one year after Velikovsky's death! What Lo says is wrong with Velikovsky's 1950 book is that he failed to understand that in 1980, a year after he died, there would appear a book with the ancient genealogy of Shang Emperors that does not contain the name of Yao.

But what was actually known of ancient Chinese history in 1950? Here is what one historian, Will Durant, in 1954 concluded as the *consensus* for judging the precision of Chinese history back to the very ancient past around the period when Velikovsky wrote *Worlds in Collision* in 1950:

"China has been called 'the paradise of historians.' For centuries and millenniums it has had official historiographers who recorded everything that happened, and much besides. We cannot trust them further back than 776 B.C."¹¹²⁶

This is a consensus statement by a consensus historian which means, that in general, historians of ancient China had concluded in 1954, on the basis of the best available evidence known at the time that one could not know with anything resembling real precision, the history of China beyond 776 B.C. But Lo, along with the editors of *The Skeptical Inquirer*, knew in the 1950's that all these historians of this consensus statement by Will Durant had "misinterpreted and even abused the evidence provided by China." These historians in the 1950's should have been able to foretell that in

¹¹²⁴Lo, *op. cit.*, p. 284.

¹¹²⁵K. C. Chang, *Shang Civilization*, New Haven University Press, (1980), p. 6.

¹¹²⁶Will Durant, *Our Oriental Heritage*, (New York, 1954), p. 642.

some future time historians and archaeologists would present new evidence on this problem of ancient China's history beyond 776 B.C.

The Skeptical Inquirer makes manifest its view of paranormal phenomenon as sheer nonsense. But then what do its editors do? They demand that in the 1950's historians should have known, that is, foretold, the future of new interpretations of the history of China, and that Velikovsky, after he died, should have known from the grave in 1980 or read this interpretive material. Wouldn't the occultists be surprised by this kind of criticism by CSICOP's official organ dedicated to rationality and debunking paranormal claims. The claim that Velikovsky is guilty and to be condemned for his 1950 work by such paranormal criticism is intriguing.

When the framers of the United States Constitution, way back in the late 1700's, wrote this document, they were very much concerned with paranormal phenomena of foretelling future events. They were, in fact, real debunkers of this sort of occultism. To back up this belief that no one could foresee future events, they placed a clause in the Constitution regarding *ex post facto* laws. This is a law which is enacted on one day but is enforced retroactively. For example some monarchs had passed laws on one day and then accused individuals who knew nothing about this law prior to its enactment of having violated it prior to its decree. This poor person was charged with breaking the law and was imprisoned or fined for having done so. But men like Madison, Washington and their American colleagues who drew up the Constitution, knew that only tyrants would abuse such *ex post facto* laws and forbade their enactment. They knew there was no such thing as paranormal seeing into the future by anyone. They had real scientific integrity and put a stop to this abusive behavior. But Lo and the editors at *The Skeptical Inquirer* condemned Velikovsky for not knowing the future and not reading the new research from his grave!

But how secure is the ancient history of China based on the king's list presented by Lo? Lo's support for this evidence is similar to the ones raised about Velikovsky's historical reconstruction of the Near East. Other kings' lists were resurrected by historians such as those of Manetho and Berosus as evidence against Velikovsky's historical reconstruction of ancient chronology. But R. H. Hall maintains that, to be acceptable as an adequate or accurate record, any king's lists "would be most unsafe to trust . . . [as] data unless' it is confirmed by other data."¹¹²⁷ Therefore, one may rightly question the support for the Emperor's list of the Shang Dynasty. Here then is what one scholar had written in 1987 the year of Lo's critique.

"The history of the Shang Dynasty is still awaiting rewriting, owing to new evidence unearthed by archeological excavations However, based on the earliest extant Chinese texts, it has been confirmed that the dynasty had thirty kings whose collective reign lasted for about 600 years. Nevertheless, inconsistencies exist among the ancient texts regarding the dating of the dynasty, which has been a heated debate among modern-day experts on ancient China. The most influential dates are -1766 to -1122, -1711 to -1111, and
-1623 to -1027."

And who does one think wrote this statement that the dating of the Shang Dynasty is inconsistent and in "heated debate"? Why it is none other than Henrietta Lo herself.¹¹²⁸

The question more pertinent of course, is not only what the documents of ancient China state, but what have the archeological digs shown about the placement in time of the Shang Dynasty? In 1986, one year prior to the publication of Lo's criticism of Velikovsky's chronology, K. C. Chang edited a collection of papers on the archeological evidence of

¹¹²⁷R. H. Hall, "Egyptian Chronology," *Cambridge Ancient History*, Vol. I, p. 167 in Immanuel Velikovsky, *Peoples of the Sea*, (New York, 1977), p. 208.

¹¹²⁸Lo, *op. cit.*, p. 290.

Shang history. In particular is the question of properly dating artifacts found from the "Fu Hao tomb" at An-yang, which was the ancient city of Yin, the capital of the Shang Dynasty. Since the relics found in the Fu Hao tomb exhibited the entire range of artistic development of the Shang, Chang suggested, "the developmental history of Shang is to be looked at AS MORE ACCELERATED than we used to think."¹¹²⁹ (Capitalization added) What Chang means is that the Chinese history of the Shang Dynasty must be closer to the dynasty that followed it or compressed in time to much less than 600 years. This would be entirely expected if, in fact, the dating of this entire period was brought down much closer in time to the Chou Dynasty. That is, the precise dating is still not known regarding this early historical time in China, in spite of Lo suggesting favored interpretations as if they were facts.

Lo then turns to demonstrate to her own satisfaction that there was no immense flood in China in Yao's time as described by Velikovsky from ancient documents. As her proof, she offers Derek Bodde's interpretive suggestion that "the Yu flood [Yu was the engineer who came sometime after Yao who drained the flooded lands of China] is not a localized memory of any particular flood but part of the universal-flood theme symbolizing primeval chaos."¹¹³⁰ This book by Bodde was published in 1981, two years after Velikovsky had died and 31 years after *Worlds in Collision* was published. Now apparently Lo and the editors of *The Skeptical Inquirer* believe that like Nostradamus or Edgar Cayce, Velikovsky could look into the future and could know from the grave that Bodde was going to suggest that the mythological and/or historical documents should be *interpreted* to fit his beliefs or interpretive guess. Because, at bottom, that is what Lo has presented. To support this *guess*, Lo suggests,

"Similarly, Yu, who allegedly succeeded in draining of the flood waters and opening up waterways, personified the course of struggling against nature a society goes through to attain agrarian economy The flood is not remembered by the Chinese because of its unusual catastrophic impact generating a 'collective amnesia,' as rationalized by Velikovsky. It is remembered rather as a symbol of Yu's selfless service for the people."¹¹³¹

Thus, according to Lo, there was no unusual flood in Yao's time which lasted for years, but at the same time she says Yu, the engineer who years later followed Yao as emperor, "allegedly succeeded in draining off the flood waters." Do the Chinese remember a flood in their ancient past? According to H. A. Giles, "but for Yu," say the Chinese, 'we should all have been fishes.'¹¹³² Does this sound as if China does not remember this ancient time as a period without an immense flood? Velikovsky made his case quite explicit in terms of the ancient Chinese literature.

"In the days of Yehou, the event occurred which separates the almost obliterated and very dim past of China from the period that is considered historical: China was overwhelmed by a catastrophe.

"At that time the miracle is said to have happened that the Sun during a span of ten days did not set, the forests were ignited, and a multitude of abominable vermin was brought forth."¹¹³³

¹¹²⁹K. C. Chang, editor, "Foreword," *Studies of Shang Archeology*, (New Haven, 1986), p. XIII.

¹¹³⁰Lo, *op. cit.*, p. 285 discussing Derek Bodde's "Myths in Ancient China," *Essays on Chinese Civilization*, (Princeton, 1981), pp. 45-48.

¹¹³¹*Ibid.*

¹¹³²H. A. Giles, *Gems of Chinese Literature: Prose*, (Shanghai, 1923), p. 72.

¹¹³³*The Shu King, the Canon of Yao*, (Trans. J. Legge, 1879) See also C. L. J. de Guignes, *Le Chou-king*, (1770), Pt. 1, Chap. 1 and J. Moryniac, *Histoire Générale de la Chine* (1877), I, 53.

"In the lifetime of Yao [Yahou] the sun did not set for ten full days and the entire land was flooded.¹¹³⁴

"An immense wave 'that reached the sky' fell down on the land of China. 'The water was well up on the high mountains, and the foot hills could not be seen at all.'¹¹³⁵

"'Destructive in their overflow are the waters of the inundation,' said the emperor. 'In their vast extent they embrace the hills and over top the great heights, threatening the heavens with their floods.' The emperor ordered that all efforts be made to open outlets for the waters that were caught in the valleys between the mountains. For many years the population labored, trying to free the plains and valleys of the waters of the flood by digging channels and draining the fields. For a considerable number of years all efforts were in vain. The minister who was in charge . . . was sentenced to death because of his failure—'For nine years he labored, but the work was unaccomplished'—(*The Shu King*)—and only his son Yu succeeded in draining the land. This achievement was so highly rated that Yu became emperor of China after King Shun, first successor of Yahou. This Yu was the founder of the new and notable dynasty called by his name.

"The chronicles of modern China preserve records of one million lives lost in a single overflow of the Yellow River¹¹³⁶ Was not the catastrophe of the time of Yahou one of the major inundations of rivers, as modern scholars suppose it to have been? But the fact that this catastrophe has been vivid in traditions for thousands of years, while . . . the overflow of the Yellow River, when a million people perished . . . plays a conspicuous part in the recollections of the nation, is an argument against the established interpretation.

"Rivers do not overflow in the form of a sky-high wave. The overflowing rivers of China subside in a few weeks, and the water does not remain on the plains until the following spring, but flows away, and the ground dries in a few more weeks. The flood of Yahou required draining for many years, and during all this period water covered the lower part of the country."¹¹³⁷

Only by ignoring this clear-cut evidence presented by Velikovsky can Lo or Bodde offer the supposition that there was no flood at that time.

As part of her documentation Lo tells us:

¹¹³⁴J. Hübner, *Kurze Fragon aus der politischen Historie*, (1729).

¹¹³⁵*The Shu King, the Canon of Yao*, (transl. Legge 1879).

¹¹³⁶Andree, *Die Flutsagen*, p. 36.

¹¹³⁷Immanuel Velikovsky, *Worlds in Collision, op. cit.*, pp. 101-102.

"Besides translations of ancient Chinese texts, Velikovsky also relies on tertiary Western works, some of which are obviously worthless and hence categorically ignored in this article. The Chinese texts [Lo] used are the *Book of Historical Documents*, the *Memoirs of the Grand Historian*, and the *Annals of the Bamboo Books*."¹¹³⁸

First, Lo tells us SOME of the Western works are obviously worthless but not all. She gives no reason to reject them other than that they are tertiary works. However, Lo then categorically relies on a tertiary source to refute Velikovsky. She claims, "Bob Forrest has presented evidence in his *Velikovsky's Sources* to shatter Velikovsky's unfounded assertion[s] . . ." ¹¹³⁹ about China.

Here then is Forrest's source for the debunking of the flood in China in Yao's time. "Yong Yay and Arthur Cotterell write in their book *Chinese Civilization*, 1977,"¹¹⁴⁰ Now the reader is directed to two points made by Forrest. The first is that this book was published in 1977 twenty-seven years after Velikovsky wrote *Worlds in Collision*. So in this case, Velikovsky was again required to be clairvoyant and know in 1950 what new *interpretation* of the flood story would be written twenty-seven years later. Secondly, the book Forrest relies upon is also a *tertiary source* and therefore should be questionable. But it is not a questionable tertiary source because it agrees with Lo's biased interpretation. In fact, the very source Lo herself relied upon regarding the flood evidence, Derek Bodde's "Myth's of Ancient China," in his *Essays on Chinese Civilization*, is also a *tertiary source*. But this tertiary source also agrees with Lo's suppositions regarding the flood. Therefore, when a tertiary source agrees with Lo's suppositions it is valuable and useful, but when a tertiary source disagrees with Lo's assumptions it is "obviously worthless and *hence categorically ignored* . . ." As Edward Bulwer Lytton admonished, "That should be a warning to you never again to fall into the error of the would-be-scholar—namely, quote second-hand."¹¹⁴¹ In fact, this approach to evidence clearly seems to be the approach of the editors of *The Skeptical Inquirer*. They will publish attacks on Velikovsky's work based on evidence, no matter how distorted or unfounded, but will not publish defenses of these attacks based on evidence, no matter how accurate and well founded they may be. Again we run into the question of objective standards employed by Lo. Lo has simply set up a double standard to argue her case; tertiary sources that support her opinions are evidence, tertiary sources that contradict her opinions are "obviously worthless."

Typical of the sloppiness and double standards of Lo's references is this statement:

"The 'rare' phenomena Velikovsky seeks can be found easily in the catalogs compiled by the above mentioned European astronomers. One such occurrence, a meteor shower, did fall on March 23, -687, and the entry 'Stars fell like rain' is based on, says Velikovsky, 'old Chinese sources ascribed to Confucius,' but Velikovsky fails to name the *authoritative* sources. Apparently, Velikovsky cannot stretch such a terse statement, 'stars fell like rain' too far."¹¹⁴²

As her source for this, Lo gives citation 22, which states "W in C, page 241." I went to *Worlds in Collision* to page 241, cited by Lo, and read it carefully several times to find this discussion of the entry, "stars fell like rain." Here,

¹¹³⁸Lo, *op. cit.*, p. 284.

¹¹³⁹*Ibid.*

¹¹⁴⁰Bob Forrest, *A Guide to Velikovsky's Sources*, (Santa Barbara, 1987), p. 61.

¹¹⁴¹Edward Bulwer Lytton, *My Novel*, (1983), Chapter 19.

¹¹⁴²Lo, *op. cit.*, p. 286.

Velikovsky cites the following: "The five planets went out of their courses. In the night *stars fell like rain*. The earth shook." (Emphasis added) Now Lo specifically stated that Velikovsky "fails to name the *authoritative* sources" for "Stars fell like rain." But it is on page 241 that the information to the authoritative source *is given* in Velikovsky's footnote 5. (*The Chinese Classics*, [transl. and annot. by J. Legge, Hong Kong ed.], [Vol.] III, Pt. 1, [page] 25.) His authoritative source is a Chinese classic, *a primary source!* Velikovsky also presented this same information from other sources as well on the same page 241.

"In the book of Edouard Biot, *Catalogue générale des étoiles filantes et des autres météores observés en Chine après le VII de siècle avant J. C.*, [Paris 1846] the register begins with this statement:

"The year 687 B.C., in the summer in the fourth moon, in the day *sin mao* (23rd of March) during the night, the fixed stars did not appear, though the night was clear [cloudless]. In the middle of the night *stars fell like rain*. (Emphasis added)

"The date, 23rd of March, is Biot's calculation. The statement is based on old Chinese sources ascribed to Confucius. In another translation of the text by [Abel] Rémusat,¹¹⁴³ the last part of the passage is rendered as follows: 'Though the night was clear, a star fell in the form of rain.' (Il tombé une étoile en forme de pluie.)"¹¹⁴⁴

What Velikovsky did was show, in both primary and secondary sources, that during the year –687 there was an event which made the stars appear to fall as rain. He then referred to an Emperor Kwei where a similar event occurred. One of his sources, Biot, cites Confucius for the –687 date while Velikovsky cites the *Annals* for the reign of Emperor Kwei in which the stars fell like rain. What Lo then argues is that Velikovsky cannot use the Kwei source as evidence for the –687 date. "He [Velikovsky] therefore makes a most amazing leap He proclaims that since the same statement was used by different sources to describe the occurrences, the latter (more ancient) record must have referred to one and the same event."¹¹⁴⁵

Even Lo states,

"Even if Yao could have been fitted into Velikovsky's –1450 time slot, King Kwei, being the eighteenth monarch following Yao, would need to reign at least 600 years after Yao (assuming 30 years to be the average span of each monarch's reign). This would place King Kwei in the –800's, still two centuries too early for the cosmic phenomena."¹¹⁴⁶

What must be borne in mind is that the ancient history of China is not a settled problem, as Lo's assumptions, taken as facts, imply. But even if Velikovsky was wrong in equating the events of Kwei's reign, the evidence of

¹¹⁴³*Catalogue des bolides et des aéroïtes observés à la Chine, et dans les pays voisins* (1819): "On a beaucoup discuté sur ce texte de Confucius" (p. 7).

¹¹⁴⁴*World in Collision, op. cit.*, p. 241.

¹¹⁴⁵Lo, *op. cit.*, p. 286.

¹¹⁴⁶*Ibid.*, p. 287.

Confucius can still clearly suggest that meteor shows occurred at both eras. But it does not remove the date of Confucius'—687 description, nor the association of this date with the others from around the world.

As her own caveat, Lo remarks that in translating the *Annals*,

"The *Annals*, dealing mainly with ancient Chinese chronology was compiled for the ruler of the *Wei* state during the early -200's and found in his grave in +281. Unfortunately, it was lost during the Southern Sung Dynasty (+1126 to +1279), and forged texts appeared in Ming times (+1368 to +1643). From passages cited in some pre-Sung works, fragments of the authentic *Annals* were collated by modern scholars. In general it should be used with great care as a source of historical facts. The *Annals'* account of Yao's mythical birth—his mother was impregnated by a red dragon—should have kept Velikovsky from including this source in his selective editing."¹¹⁴⁷

There is a real reason why Velikovsky did not use this kind of material in his book, although he does quote the *Annals* as a document. The reason is that this statement cited by Lo is an exegetical *note* written over many hundreds of years after the history was originally compiled. Velikovsky did not cite the notes of the more recent Chinese compilers because they are only fanciful analyses which are fraught with the later beliefs and superstitions of the Chinese people of the later period. What Velikovsky did was stick directly to the historical text which contains none of these later fanciful inclusions by later scribes and exegetists. That is, Velikovsky did just as Lo cautioned must be done. He employed the historical document "with great care as a source of historical facts," but not the exegetical notes.

But does Lo also use the same care in dealing with this very same document? Again, I must sadly report, she does precisely what she calls Velikovsky's "selective editing."¹¹⁴⁸ She specifically cites the exegetical notes to disprove Velikovsky's work.

"In fact, when Velikovsky quotes from the *Annals* that 'a brilliant star appeared,' to imply that the newborn comet Venus was causing the cataclysm of Yao's time, he must have noticed that two lines below that sentence the *Annals* records, 'the five planets looked like threaded pearls.' What China considered as the five planets had always been: Jupiter, Mars, Venus, Mercury and Saturn. Hence, Venus already existed as a planet in Yao's time, trad. 2400 B.C."¹¹⁴⁹

Velikovsky *cited* the *Annals* text and not the exegetical note "a brilliant star appeared"¹¹⁵⁰ which also appears as "a brilliant star issued from the constellation Yih," in the exegetical notes. In the second sentence down from the *text* of the *Annals*, we do not find the statement, "the five planets looked like threaded pearls." Instead, the text states two lines down: "In his 53rd year, he sacrificed near the Loh."¹¹⁵¹

¹¹⁴⁷Lo, *op. cit.*, p. 284.

¹¹⁴⁸*Ibid.*

¹¹⁴⁹*Ibid.*, p. 286.

¹¹⁵⁰Legge's *Annals*, p. 112.

¹¹⁵¹*Ibid.*

But does the exegetical note Lo cites on the next page really state this planetary interpretation as a fact? To analyze this, I asked Lorraine Lai, a postgraduate university friend, fluent in Mandarin Chinese, to read the exegetical note and tell whether the note states what the scribe included but which is not in the text. After reading it carefully, she said it was "pure poetry," "fanciful imagery" and had "*nothing to do with the planets.*" Here then is her translation of the exegetical note:

"When the emperor had reigned 70 years a brilliant star issued from Yih. In the sky stars formed, wings phoenix-like, which were reflected in the courtyard of the palace on the pearly grasses bedewed with drops of water. These reflections appeared as shining gems below the starry phoenix-like sky wings. The light of all these taken together appeared lovely like five stars strung on a thread."

Below is the original Chinese for those who wish to examine this translation by a university trained reader of Mandarin Chinese.

What the exegetical scribe did was add his analysis of his own era as the interpretation of the text. Needless to say, by that later time the "five planets" were well-known and that is how he interpreted "five stars." Even if the scribes' citation was accurately translated by Lo, which it isn't, Lo knew that this exegetical interpretation did not originate in the earlier era, or over several hundred years prior to the exegetical interpretation, or it would not have required an analysis or special interpretive note. Lo used a secondary source's interpretation yet again to attack Velikovsky. Who can trust such a person?

What Lo has done, as have so many others who criticized and condemned Velikovsky's work, is assume that the generally accepted *standard interpretations* of history, mythology and folklore are not interpretations but solid facts. But what we find is that this leads to narrow, sloppy research which in case after case creates its own biases and "selective editing" used as evidence. As Adolph Hitler pointed out in his book, *Mein Kampf*, "All propaganda has to be popular and has to adapt its spiritual level to the perception of the least intelligent of those toward whom it intends to direct itself."¹¹⁵² As serious criticism, Lo's work in *The Skeptical Inquirer* simply fails. As popular propaganda aimed at the least intelligent, like so much of the critical material directed against Velikovsky in that journal, it fits that analysis aptly and does its job superbly. Perhaps that is why Martin Gardner, their defender of truth and science, called Lo's criticism an "excellent article on 'Velikovsky's Interpretation of the Evidence Offered by China' in his *Worlds in Collision*. Ms. Lo finds V's [Velikovsky's] book so riddled with errors of fact and logic that historians were fully justified in refusing to take V's clumsy efforts to reconstruct world history."¹¹⁵³ Gardner clearly knows the value of Lo's propaganda and lauds it to the skies.

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¹¹⁵²Adolph Hitler, *Mein Kampf*, "Chapter 6," p. 232.

¹¹⁵³Martin Gardner, *The New Age*, (Buffalo, N.Y., 1991) P. 71.

THE NEW CATASTROPHISM?

"You must not say that this cannot be, or that is contrary to nature. You do not know what Nature is, or what she can do; and nobody knows, not even Sir Roderick Murchison, or Professor Owen, or Professor Sedgwick, or Professor Huxley, or Mr. Darwin, or Professor Faraday, or Mr. Grove They are very wise men; and you must listen respectfully to all they say: but even if they should say, which I am sure they never would, 'That cannot exist. That is contrary to nature,' you must wait a little and see; for perhaps even they may be wrong."

Charles Kingsley

The Water-Babies

in Bernard Heuvelmans'

On the Track of Unknown Animals,

(New York, 1958), p. 9

"Facts are stubborn things."

Alain Rene Le Sage

Gil Blas, Book X, Chapter 1

"Facts are contrary 'z mules."

James Russell Lowell

Bigelow Papers, Series II, (1862), No. 4

Since extraterrestrial catastrophism has now become a major scientific concept of astronomy, geology and evolution, *The Skeptical Inquirer* published an article (Winter 1990) by David Morrison, Chief of the Space Division of the NASA Ames Research Center in California, and Clark R. Chapman, a planetologist with the Planetary Science Institute in Tucson, Arizona, to set the record straight, especially with respect to Velikovsky. The reason they have done so is to disentangle modern scientific catastrophism from pseudoscientific catastrophism out of which it has finally emerged and been made "respectable." That is, Morrison and Chapman admit, "Several decades before scientists had become aware of accumulating evidence for violent and catastrophic events in the solar system . . . Velikovsky . . .

claimed, long before it was fashionable to do so, that there have been collisions and near collisions among the planets, and that the history of the Earth had been marred by violent events of cosmic origin."¹¹⁵⁴

These scientists wish to show that Velikovsky made no contribution of any worth to the concept of modern scientific catastrophism. They ask:

"Is he [Velikovsky] perhaps the unrecognized prophet of the new catastrophism, a person who overcame the prejudices of his time and leapfrogged into a new conception of geology and astronomy? Some people claim so, but we are not among them. The fact is that Velikovsky . . . was almost entirely wrong in his ideas about Earth history."¹¹⁵⁵

In the previous discussion of Kendrick Frazier's piece, "The Distortion Continues," Morrison, in a more vitriolic tone, attacked the ad by Doubleday promoting Velikovsky's books as "outright lies."¹¹⁵⁶ Hopefully we will not find "outright lies" in Morrison and Chapman's criticism. Years ago Chapman actually wrote a few pages of comment on Velikovsky which are a far cry from the piece in *The Skeptical Inquirer*. For example, Chapman states in that work his earlier opinion and consideration of Velikovsky as follows:

"I would be amiss in discussing solar system catastrophism not to mention Immanuel Velikovsky. Inspired by professional interests in psychoanalysis, he performed a solitary, unparalleled synthesis of the implications of biblical and other ancient myths and legends for solar system history. Although based less on rigorous logic than on analogy and circumstantial evidence his results were truly revolutionary

"His monograph *Worlds in Collision* was published in 1950 over objections of many astronomers who tried to censor his ideas Velikovsky has since been shunned by the scientific establishment Although Velikovsky is an extreme catastrophist, is his work unscientific? The most frequent criticisms of him are that his assumptions and reasoning are unsound, or simply that he is wrong. But many scientists reason by analogy, unfounded assumptions, and circumstantial evidence, and yet they are not barred from publishing in establishment journals. Nor has being wrong been an obstacle to publication: most articles published in decades past seem wrong to us now. Truth is hard to come by."¹¹⁵⁷

Hopefully Chapman and Morrison in their criticism will not be found to employ only reason by analogy, unfounded assumptions and circumstantial evidence to discredit Velikovsky's work. For example these scientists state that:

¹¹⁵⁴David Morrison, Clark R. Chapman, "The New Catastrophism," *The Skeptical Inquirer*, (Winter 1990), p. 148.

¹¹⁵⁵*Ibid.*

¹¹⁵⁶Kendrick Frazier, *The Distortion Continues*, *The Skeptical Inquirer*, (Fall 1980), p. 34.

¹¹⁵⁷Clark R. Chapman, *The Inner Planets*, (New York, 1977), pp. 39-40.

"The evidence, he [Velikovsky] said, was in these [ancient] writings. If the theories of modern physics and astronomy were not consistent with such celestial events, then the astronomers and physicists had better modify their theories."¹¹⁵⁸

However, on this question of astrophysics, Chapman stated with respect to Velikovsky:

"Do we really know that it is physically impossible for Venus to be in its present orbit, apparently stable for millions to billions of years, if it grazed the Earth only a few thousand years ago? When pressed, most specialists in orbital theory admit that theorems of orbital stability in systems as complex as the solar system have yet to be rigorously proven. And many of our other facts about the solar system are also built on assumptions not quite so fundamental as Newton/Einstein mechanics. We all ought to be more aware of our assumptions and more open to questioning them should contradictions appear. Or, at the very least, while most scientists pursue research guided by currently accepted paradigms, we should be tolerant of the few that march to different drummers."¹¹⁵⁹

But when it comes to Velikovsky in *The Skeptical Inquirer*, Chapman and Morrison are not aware of this *assumption* of solar system stability because, "According to Velikovsky . . . —Venus—was formed within the same short span of time [within the past few thousand years] How are we to reconcile this with the 4.5-billion-year age of the solar system?"¹¹⁶⁰ Perhaps by following Chapman's advice that we all ought to be more aware of our assumptions of orbital stability in systems as complex as the solar system which have not yet been rigorously proven. However, this is the thrust of their criticism of Velikovsky. As we pointed out earlier with Billy P. Glass who stated about the age of Venus, "the geologic history of Venus . . . is highly speculative. We assume that Venus was formed 4.5×10^9 y ago [4.5 billion years ago]."¹¹⁶¹ (Emphasis added)

Ralph Abraham, Professor Emeritus at the University of California at Vera Cruz, claims this entire solar system stability argument is based on "dogma" and "faith" and wrote in 1994:

"The mathematical problem of [solar system] stability rested . . . when a reincarnation of William Whiston appeared. Immanuel Velikovsky had much in common with both William Whiston [who claimed the Earth was struck by a comet causing the biblical flood] and Giordano Bruno [who was burned at the stake by the Church for suggesting an unstable solar system]

¹¹⁵⁸Morrison and Chapman, *op. cit.*, p. 149.

¹¹⁵⁹Chapman, *op. cit.*, p. 41.

¹¹⁶⁰Morrison, Chapman, *op. cit.*, p. 151.

¹¹⁶¹Billy P. Glass, *Introduction to Planetary Geology*, (New York, 1982), p. 324.

"The record shows that astronomers hold rigidly to a peculiar dogma, not much advanced from Laplace's nebular hypothesis or the biblical story of creation; that the solar system has remained essentially unchanged since it was created eons ago. Their assumption has of necessity predetermined the views of geologists and historical biologists. This dogma, basically of theological and not scientific nature, as Galileo and Laplace pointed out, is grounded on fear. The dogma is groundless but the fear is real, and was the principal reason for a prolonged emotional outburst against Velikovsky, in which almost the entire scientific community of the 1950's took part

"In 1974 the American Association for the Advancement of Science met specifically to condemn Velikovsky. The storm that began with the publication of his book in 1950 had not yet died down, and apparently it is still necessary for scientists to deny Velikovsky's thesis solely on the grounds of the [solar system] stability dogma. Several books denouncing Velikovsky were published between 1950 and 1977. Famous astronomers, such as Harlow Shapley and Carl Sagan, condemned Velikovsky on the mistaken grounds that Newton's model does not allow chaos They believed, religiously, in the stability of Newton's mathematical model of the solar system."¹¹⁶²

Abraham goes on to state,

"If a comet passed [a planet], the orbits could be shaken loose, and take a sharp turn. This remains uncontested after a century of mathematical effort."¹¹⁶³

He concludes:

"According to mathematics alone, one cannot contradict Velikovsky. The dogma of stability [of the solar system] remains just an article of faith."¹¹⁶⁴

Dr. Robert Bass, who apprised me of this material in Abraham's book long ago, wrote, "'Proofs' of the stability of the solar system," in *KRONOS*, which showed that solar system stability was not only a dogma, but a myth based on the careful work of Dr. W. M. Smart, Regis Professor of Astronomy in the University of Glasgow who showed that solar system stability was never rigorously proven. As Bass stated, Velikovsky's life's work "should not be hastily dismissed upon mere 'group consensus' about the validity of obsolete ideas, which true experts have long ago dismissed as illusions."¹¹⁶⁵

¹¹⁶²Ralph Abraham, *Chaos, Gala, Eros*, (San Francisco, 1994), pp. 194-195.

¹¹⁶³*Ibid.*, p. 196.

¹¹⁶⁴*Ibid.*, p. 196.

¹¹⁶⁵Robert W. Bass, "'Proofs' of the Stability of the Solar System," *KRONOS*, Vol. II, No. 2., (1976), p. 44.

Again it is suggested to Chapman and Morrison that they look at their assumptions, dogmas, illusions, and faith, not only of the orbital stability of the solar system, but at their *assumption* of the age of Venus.

They raise the same issue in that "Earth and Mars [would also] experience major orbital changes in the past 4,000 years."¹¹⁶⁶ But what do other scientists say with regard to this concept of orbital solar system stability for Earth and Mars? The late Thomas A. Mutch, Professor of Geological Sciences at Brown University, with four of his scientific associates in their book, *The Geology of Mars*, actually admit it is "prejudice" *plain and simple* that leads to the assumption of orbital stability as opposed to the concept of "catastrophic interplanetary collisions." Morrison and Chapman tend to deny "Velikovsky . . . overcame the *prejudices of his time* and leapfrogged into a new concept of geology and astronomy." However, Mutch and his colleagues, after examining the geology of Mars, state: it is, indeed, prejudice that is at the heart of this rejection.

"The same seeds of doubt germinate when someone proposes that large cometary bodies struck Earth many years ago, causing disruption of continents and oceans

"It is interesting to note how fashions change. Five hundred years ago the cosmological models that are now regarded with such disinterest would have been heresy. How might OUR PREJUDICES change in the future? *Today we refuse to believe that CATASTROPHIC INTERPLANETARY COLLISIONS* [Velikovsky's theory] have warped Earth's history. A hundred years hence, when large impact scars on other planets are familiar landscapes will we feel the same?"¹¹⁶⁷ (Capitalization and emphasis added)

While Morrison and Chapman accuse Velikovsky of, "reject[ing] all other evidence according to its consistency with his preconceived conclusions, . . ."¹¹⁶⁸ they reject all Velikovsky's concepts of catastrophic, interplanetary collisions according to its consistency with their dogmas of orbital solar system stability and with *prejudices*, wherein they refuse to believe that catastrophic, interplanetary collisions have warped Earth's history. Having turned their orbital solar system assumption into dogma, they can argue that their prejudices and assumptions are facts which, of course, is hardly the case.

But in order to discredit Velikovsky's concept they turn to "facts."

"However, Velikovsky's 'facts' have often turned out to be suspect. He was highly selective in his choice of quotations from ancient writings, and he often used translations from older sources long-since revised by modern scholars. When these texts are examined in detail, they frequently contradict Velikovsky's interpretation."¹¹⁶⁹

¹¹⁶⁶Morrison, Chapman, *loc. cit.*

¹¹⁶⁷Thomas A. Mutch, Raymond E. Arvidson, James W. Head, III, Kenneth L. Jones, R. Stephen Saunders, *The Geology of Mars*, (Princeton, 1976), p. 93.

¹¹⁶⁸Morrison, Chapman, *op. cit.*, p. 151.

¹¹⁶⁹*Ibid.*, p. 149.

They argue that Velikovsky's historical chronology of the ancient Near East "placed him in direct confrontation with the evidence of archeology."¹¹⁷⁰ Now each of these statements is completely unsupported by a solitary footnote or source that the reader can check. We are told that it is a "fact" that their statements are true. However, upon detailed examination, it will be shown that Morrison's and Chapman's "facts" turn out to be "suspect" and highly selective and are, in "fact," contradicted by the literature Velikovsky cited with respect to catastrophism in Near Eastern archaeology and also with Near Eastern chronology from archaeology.

In regard to catastrophism destroying ancient civilizations, Morrison and Chapman simply fail to acknowledge the "fact" that one of the world's leading archaeologists concurs with Velikovsky that ancient Near Eastern civilizations and great periods of history came to a close that could only be interpreted as global or near global catastrophism. The archaeologist who presented this evidence is Claude F. A. Schaeffer, in his great work of archaeology, *Stratigraphie comparée et chronologie de l'Asie Occidentale*, published in 1948 by Oxford University Press. Velikovsky discussed and quoted Schaeffer's work in *Earth in Upheaval*, pages 193 through 201. For example, Velikovsky cites Schaeffer's statement of one such destruction all across the Near East. ". . . it was an all encompassing catastrophe The initial and real causes must be looked for in some cataclysm over which man had no control."¹¹⁷¹ Claude F. A. Schaeffer,¹¹⁷² is also quoted by Velikovsky thus:

"The great activity of international trade which, during the Middle Bronze Age, had been characteristic of the eastern Mediterranean and most of the lands of the Fertile Crescent, suddenly stopped in all this vast area In all the sites in Western Asia examined up to now a hiatus or a period of extreme poverty broke the stratigraphic and chronological sequence of the strata" [This occurred at] ". . . all sites that are stratigraphically examined."

"Our inquiry has demonstrated that these repeated crises which opened and closed the principal periods of the third and second millennia were caused not by the action of man."¹¹⁷³

Was Schaeffer not telling the truth, and are his "facts" suspect? Here, IN FACT, is what Schaeffer wrote to Velikovsky:

"I hope you [Velikovsky] will go on with your research. You are working in the right direction and time will help to show the reality of global or near global catastrophes. Already continental, or near continental catastrophes cannot be doubted as I showed in my stratigraphical work in the Near East. It will take time for your findings and mine to be acknowledged."¹¹⁷⁴

While Morrison and Chapman claim Velikovsky's "facts" are suspect, we find that a world renowned archaeologist claims, in complete contradiction to these scientists, that he is working in the right direction and global or

¹¹⁷⁰*Ibid.*

¹¹⁷¹Immanuel Velikovsky, *Earth in Upheaval*, (New York, 1955), p. 195.

¹¹⁷²Claude F. A. Schaeffer, *Stratigraphie comparée et chronologie de l'Asie Occidentale, (III^e et II^e millénaires)*, (Oxford University Press, 1948), pp. 534–567.

¹¹⁷³*Ibid.*, pp. 196-198.

¹¹⁷⁴Immanuel Velikovsky, *Stargazers and Gravediggers*, (New York, 1983), p. 318.

near global catastrophes in recent times "cannot be doubted," based on rigorous archaeological work. But Morrison and Chapman say Velikovsky's hypothesis, "placed him in direct contradiction with the evidence of archeology."¹¹⁷⁵

Not only did Schaeffer claim immense cataclysms closed entire ages of different civilizations, but M. Finley also found archaeological evidence for the European eastern Mediterranean region of the same catastrophic nature.

"Archaeological research reveals cataclysms, but it cannot tell us the causes or even who the participants were. Catastrophes occurred at regular intervals explaining the five clearly separated layers."¹¹⁷⁶

Again, the archeological evidence points to the same conclusion of major catastrophes closing entire periods of civilizations which are separated by layers of destruction. But Chapman and Morrison act as though such evidence does not exist. This would be understandable if this evidence did not exist, if this evidence was not part of Velikovsky's book, *Earth in Upheaval*. But Schaeffer's research is clearly presented there by Velikovsky. Thus, the question is, why did Morrison and Chapman claim that the archeological evidence places Velikovsky in "direct confrontation" with it when in "fact" Morrison and Chapman completely ignored the evidence of archeology Velikovsky did produce? While Morrison called the Doubleday ad "outright lies," what have he and Chapman presented when they attacked Velikovsky's theory on the basis of archeology, but omitted Schaeffer's archaeological evidence that Velikovsky had presented? "Outright distortion!"

Velikovsky, of course, claimed that the historical chronology of the Near East was at least 500 years younger than presented. What have the archaeologists found about the ancient chronology? What they have unearthed is that about 700 years of Near Eastern history simply does not exist. Professor Gunnar Heinsohn of Bremen University published a paper in the 6th International Congress of Egyptology of a major archeological finding of this "fact."

"The scholarly world is not usually confronted with the above stratigraphic evidence in the ground but is offered excavation reports that add periods to the strata actually found. This stretching of the sites' historical duration is done to satisfy the [establishment's] textbook chronology ideas that the excavators have on their minds before their work begins. Instead of testing the chronographers' ideas by their own archaeological results, they usually try to adjust their finds to fit these preconceived dates

"When, in February 1988, the author first published his stratigraphy-based equation of Hyksos and Old Akkadians,¹¹⁷⁷ the German archeologist, Wilfried Pape, excavating at Tell Munbaqa/Syria was the first to take up the challenge [that Heinsohn claimed there was no 750 year break there, as opposed to the claim of establishment historians that a 750 year break existed] W. Pape devoted the first special sounding [archaeological dig] to this 'gap' but could not confirm the hiatus between ca. 2250 and ca. 1475. On the contrary, he found clear-cut architectural continuity. This observation was written to the

¹¹⁷⁵Morrison, Chapman, *op. cit.*, p. 149.

¹¹⁷⁶M. Finley, *Early Greece: The Bronze and Archaic Ages*, (London, 1981), p. 10.

¹¹⁷⁷[G.] Heinsohn, "Auswirkungen der mesopotamischen Evidenzdaten auf die ägyptische Chronologie und die Lösung des Hyksos-Problems" in, *Gesellschaft für die Rekonstruktion der Menschheits—und Naturgeschichte/Bulletin*, Vol. 5, No. 1.

author on November 22, 1988. In 1989, three special soundings were brought down to test Munbaqa's Akkad-Mitanni-gap. A geologist specializing in sediments and aeolic [windblown] layers confirmed the work of the archaeologist. There is no hiatus between the Old-Akkadian and Mitanni/Hurrian strata in Munbaqa.¹¹⁷⁸ Moreover, Old-Akkadian cylinder seals remained in use for business contracts in the Mitanni/Hurrian stratum . . . another indication of the uninterrupted continuity between both periods, i.e., for the very absence of a hiatus of some 750 years."¹¹⁷⁹

Hence, basic archeological evidence exists to show that immense catastrophes closed major periods in ancient Near Eastern civilizations at the same time, according to Schaeffer and Finley, and that the chronology based on archeological evidence of the Near East is at least 750 years shorter in duration. But Morrison and Chapman argue Velikovsky's catastrophic analysis of ancient history is in direct confrontation with archaeology. They suggest that Velikovsky's shortened chronology is in error, but the archeological digs just described *now* prove otherwise. Though they could not know of the 1993 report, the other evidence was clearly available to them.

Finally, they pose that "His chronology wreaked similar havoc with the accepted notions of classical history, displacing the siege of Troy, by Mycenaean Greeks to the period of the Argolid tyrants."¹¹⁸⁰ That is, Velikovsky claimed that there was no 500 year Dark Age in Greek history. But now, even other historians are beginning to say just that!

Peter James and some of his colleagues have presented a volume on the evidence for a hiatus, *Centuries of Darkness*, published in London in 1991. Herbert A. Storck, an historian who earlier was very skeptical of this evidence writes,

"In general, I was very impressed with James' mass of data drawn from the four quarters of the ancient world to document the two century gap. He certainly makes a good case. And we must begin to wonder whether this 'time,' so to speak, was ever really there. What I mean to say is that we have created a chronology that comprises these empty centuries that the material evidence will not support. Accordingly, it is time to ask the question: Do these centuries exist anywhere except in our ivory towers of learning? Are they little more than theoretical centuries based on modern interpretation of ill-fitting pieces for which we have no master plan? . . . How long will we search for empty strata and non-existent kings based on modern day interpretations of ancient chronology?"¹¹⁸¹

This commendation is not proof that the problem of the Dark Age of Greece fulfills Velikovsky's chronology, but it indicates that the research actually being carried out tends to support parts of Velikovsky's historical analysis. It is, I think, rather shallow and unscholarly for Morrison and Chapman to suggest otherwise without presenting any evidence at all to support their judgment. Like their attack on solar system stability and chronology, the scholars doing the

¹¹⁷⁸U. Rosner, Institut für Geographie der Universität Erlangers-Nurnberg, preliminary report, August 23, 1990.

¹¹⁷⁹Gunnar Heinsohn, "Who Were the Hyksos? Can archaeology and stratigraphy provide a solution to the 'enigma of world history?'" *Sesto Congresso Internazionale Di Egittologia*, Vol. II, (Torino, Italy 1993), pp. 210-211.

¹¹⁸⁰Morrison, Chapman, *op. cit.*, p. 149.

¹¹⁸¹Herbert A. Storck, "Peter James, *Centuries of Darkness*, A Review," *Aeon*, Vol. III, No. 2, (May 1993), p. 98.

research say the assumptions and prejudices underlying the interpretation of ancient history "may not exist anywhere except in our ivory towers of learning" or "instead of testing the chronographers' ideas by their own archeological truth, they usually try to adjust their finds to fit these preconceived dates." And I strongly suggest that all Morrison and Chapman have done so far is present untested, preconceived ideas, assumptions, suppositions, and biases as facts to reject Velikovsky's concepts.

Finally, M. D. Coe claims that in the Americas, a "mighty cataclysm" destroyed the first Olmec civilization, termed the San Lorenzo culture, around 900 B.C. At that time, Coe claims "an actual period of marked cultural change" took place at most Mesoamerican sites.¹¹⁸² The cause of the cultural collapse and sudden change in culture from one type to a starkly different type, as that outlined by Schaeffer, is still unknown. However, Lowe significantly states it is clearly catastrophic, like those disclosed by Schaeffer in the Near East. "The end of the San Lorenzo Phase occupation at San Lorenzo was preceded by a thorough destruction."¹¹⁸³ At some Olmec sites, including San Lorenzo, there is evidence of violence at the end.¹¹⁸⁴ Like the earlier San Lorenzo, LaVenta was deliberately destroyed in ancient times. Its fall was certainly violent.¹¹⁸⁵

Were all these cultures destroyed around or, more probably, "at" the same time in the Near East and America so violently that one culture completely disappeared and another, quite different culture, replaced it by coincidence? In *Current Archeology*, for October, 1989, it is reported that archaeologists claim the Xia Dynasty of China also ended because of a global catastrophe during the Bronze Age. Are all these archaeologists in need of correction by Morrison and Chapman for writing that archeological evidence corroborates Velikovsky's view of global destruction? Perhaps Morrison and Chapman will show them the error of their ways!

A typical example, finally, of their scientific evidence against Velikovsky's hypothesis is their statement, "We now know that [the] Venus clouds are composed of sulfuric acid."¹¹⁸⁶ What is the evidence for this pronouncement? Again, just their word is their evidence. Is what "we know" a "fact"?

What we have encountered over and over are statements by the editors, writers and scientists in *The Skeptical Inquirer* backed up by no evidence. This "we know" is an authoritative approach, based on the assumption that if authorities state something it must be true. When the Royal Society of London was founded in 1600, it proclaimed as its motto, "*nullius in verba*," loosely translated, "take nothing on faith." Statements by scientists are required to be supported by evidence. The rational demand of this society was to avoid propaganda and "distortion of facts." That this injunction is ignored again and again in *The Skeptical Inquirer* is, I think, clear evidence that an ethical or rational approach to Velikovsky has been abandoned by these individuals. But let us examine what other scientists say with regard to their "fact" that Venus' clouds are composed of sulfuric acid. According to John S. Lewis of MIT.

"The clouds of Venus have been a favorite topic of controversy, and here the matter is still in a very uncertain state. Half a dozen species [of gases] are currently favored by different individuals as making up the visible clouds. Among the most widely advertized are water or ice, silicate and carbonate dusts, ammonium chloride, compounds of the volatile elements mercury, arsenic, etc., carbon suboxide and its polymers, hydrochloric acid solution or solid hydrates of HCl, ferrous chloride dihydrate, etc. Each species [of gas] has more detractors than supporters."¹¹⁸⁷

¹¹⁸²M. D. Coe, *Mexico*, 4 ed., (London, 1988) p. 71; see also, G. W. Lowe, "Eastern Mesoamerica," *Chronologies in New World Archeology*, R. E. Taylor and C. M. Meighan eds., (New York, 1978), p. 358.

¹¹⁸³Lowe, *loc. cit.*

¹¹⁸⁴J. A. Tainter, *The Collapse of Complex Societies*, (Cambridge, Eng., 1990), p. 12.

¹¹⁸⁵Coe, *op. cit.*, p. 75.

¹¹⁸⁶Morrison, Chapman, *op. cit.*, p. 150.

¹¹⁸⁷John S. Lewis, "The Atmosphere, Clouds and Surface of Venus," *The Solar System and its Strange Objects*, (Los Altos, 1981), p. 93.

What must be the matter with this scientist? Doesn't he know that the clouds of Venus, as Morrison and Chapman state, are now known to be made up of "sulfuric acid?" Here is what Billy P. Glass has to say about the cloud composition of Venus:

"The nature of the clouds [of Venus] has been a question of great interest for a long time. Speculative interpretations of the principal constituent of the clouds include: water drops, ice, frozen carbon dioxide, carbon suboxide, mercury, halides, ammonium nitrate, ammonium chloride, silicate, dust particles, carbonate particles, formaldehyde, hydrocarbon droplets, partially hydrated ferric chloride and hydrochloric acid."¹¹⁸⁸

What could be the matter with this scientist? Doesn't he understand that Morrison and Chapman have informed us that it is now known as "a fact" that the clouds of Venus are composed of sulfuric acid? Patrick Moore and Gary Hunt say with respect to Venus' cloud composition:

"There appears to be a haze layer overlying the main cloud-deck. This haze is simply the outer cloud layer observed by telescopic workers on Earth. It has been identified as being made up of sulfuric acid droplets . . . *but the haze layer may not be permanent*; it has been seen to appear and vanish again over periods of several years."¹¹⁸⁹ (Emphasis added)

If this is the case, then the deeper clouds which are the real clouds may not be made up of sulfuric acid. Zdenek Kopal states,

"The reader should, however, not be encouraged to take these current cosmochemical scenarios as gospel truth. Although the sulfuric acid hypothesis accounts satisfactorily for many optical properties of the clouds surrounding Venus, it cannot explain one important property—the yellowish color of the planet. The color must be produced by some substance that absorbs in the blue and ultraviolet regions of the spectrum. Sulfuric acid does not meet the requirement, nor does any likely substance which could be considered in this connection."¹¹⁹⁰

Doesn't Kopal know that in spite of this fundamental contradiction to the sulfuric acid cloud hypothesis for Venus, Morrison and Chapman now know such basic evidence may be ignored to justify *their* "fact" that the clouds of Venus are composed of a non-absorbing, ultraviolet material (a new special physics in spectroscopic research) to allow Venus to possess clouds of sulfuric acid? Now if all this has not convinced Morrison and Chapman that what they are

¹¹⁸⁸Billy P. Glass, *op. cit.*, p. 310.

¹¹⁸⁹Patrick Moore, Gary Hunt, *The Planet Venus*, (London, 1982) P. 133.

¹¹⁹⁰Zdenek Kopal, *The Realm of the Terrestrial Planets*, (New York, 1979), p. 186.

suggesting is highly questionable, I refer them to that brilliant astronomer and Chief of the Space Division of the NASA Ames research center in California, David Morrison, who engagingly informs us:

"Space probes that have passed through the clouds [of Venus] have given us a picture . . . of discrete cloud layers. Clouds are seen extending from 30km to 60km [19 miles to 38 miles] above the surface. But what are the various clouds made of? Are they all sulfuric acid as are the visible [topmost] layers [which disappear]? Only the Soviet probes have attempted compositional measurements and their results have been contradictory. Sulfur or possibly chlorine compounds of some sort are indicated, but *their exact identities are unknown*.¹¹⁹¹ (Emphasis added)

Perhaps Morrison should have a good talk with this fellow Morrison who has the temerity to suggest the exact identities" of Venus' clouds "are unknown" when "we know" otherwise. These "facts" are certainly interesting.

Now, if Morrison and Chapman refuse to believe Morrison on the nature of the cloud composition of Venus, perhaps Morrison and Chapman will believe Chapman about the cloud composition of Venus. Chapman tells us,

"There would seem to be something in the clouds [of Venus] the thermodynamic behavior of which enables minor changes in temperature to change it from a clear gas to an opaque cloud. Among the candidate compounds proposed have been pure sulfur, bromine dissolved in hydrobromic acid, and—more recently—sulfur dioxide, chlorine gas or nitrosylsulfuric acid. Nobody finds any suggestion entirely convincing . . . "¹¹⁹²

Therefore, we have both Morrison and Chapman admitting that make-up of the main cloud cover of Venus is "unknown" and "nobody finds any suggestion [about their composition] entirely convincing."

Fact 1. The clouds of Venus are made of sulfuric acid, according to Morrison and Chapman.

Fact 2. Sulfuric acid does not absorb in the ultra- violet part of the spectrum to produce the yellowish color in Venus' topmost clouds.

Fact 3. The Soviet space probes to Venus give contradictory results that cannot be used to support the sulfuric acid model, according to David Morrison. And these are the measurements that would be crucial to this discussion.

Fact 4. Scientists suggest several different gases explain the composition of Venus' clouds, which is an unsettled question in science.

¹¹⁹¹David Morrison, Tobias Owen, *The Planetary System*, (Reading, Mass., 1988), p. 236.

¹¹⁹²Clark R. Chapman, *Planets of Rock and Ice*, (New York, 1982), pp. 101–102.

Fact 5. The top cloud layer is seen to disappear and vanish over periods of several years, so the assumed sulfuric acid, of which this top cloud layer is supposedly composed, must settle into the deeper cloudbanks.

Fact 6. Morrison says the exact identity of the clouds is "unknown," while Chapman calls their composition "unconvincing."

All these scientific "facts" opposing their "we know" views seem to prove a point. It is certain that something is highly "suspect" about this problem. As scientists, these authorities, like those presented by Frazier are, I believe, making quite an impression. I wouldn't call Chapman's and Morrison's work on this point "outright lies," as Morrison did the ad by Doubleday, but I think the word "absurd" may not be far off the mark nor out of place.

And I further suggest that the motto of the Royal Society of London, "take nothing on faith," clearly is an excellent warning to be applied to Morrison and Chapman's assertion that the make-up of the Venusian clouds are known. They are not known and this is frankly admitted by their colleagues and by Morrison and Chapman, as well!

Morrison and Chapman have also stated, "We now know that Venus has no hydrocarbons . . ." ¹¹⁹³ as Velikovsky predicted. Again the unsubstantiated, authoritative statements "we know" is stated as a "fact." But as was pointed out earlier, Donahue found a sudden and large spike in the spectrometer reading of methane in Venus' atmosphere. And, of course, it is easy to believe that the Venus Pioneer probe just coincidentally landed on an erupting volcano which would emit this large amount of methane once every 100 million years.

Furthermore, Morrison then goes on to repeat the assertion "he [Velikovsky] predicted that Venus, having been incandescent a few thousand years ago, would be radiating more heat than it received from the sun . . . [This] prediction . . . [was] flatly refuted by spacecraft data." ¹¹⁹⁴ Now, this is the third time this criticism was leveled against Velikovsky's prediction in *The Skeptical Inquirer*; once by James E. Oberg, once by Kendrick Frazier's authority Michael B. McElroy, and now by Morrison and Chapman. As I quoted the real authorities in this research and in those articles that stated all the sets of probe readings from the cloud tops to the surface showed thermal imbalance, I need not repeat these citations once again. But I do have a "suspicion" about this constantly repeated misrepresentation. I find it extremely difficult to believe that all these space researchers are ignorant of these basic spacecraft findings. They have undoubtedly been spreading this disinformation over and over again. I do not believe that there will ever be any attempt to admit that what they have presented to their readers is denied and contradicted by these same probes to Venus which all showed sets of readings of thermal imbalance. Again I quote Chapman:

"Of course, Velikovsky probably is wrong. It is impossible to imagine that one man could single-handedly refute most twentieth century science without slipping up somewhere. And his supporter's claims that many of his 1950 predictions have been verified by subsequent research is simply false. The reason establishment scientists reject Velikovsky is that they believe him to be *so* wrong that they aren't interested in any research bearing on his hypothesis. Of course, that is not a very open-minded position to take by those who are supposedly searching objectively for truth." ¹¹⁹⁵

But isn't it actually worse than close-minded to attack Velikovsky's prediction by saying the sets of probes to Venus flatly refuted a thermal imbalance when, as a matter of "fact," all sets of measurements confirmed thermal

¹¹⁹³Morrison, Chapman, *op cit.*, p. 150.

¹¹⁹⁴*Ibid.*

¹¹⁹⁵Chapman, *op. cit.*, p. 40.

imbalance?! One can only feel saddened and disgusted at this form of unethical scientific journalism in *The Skeptical Inquirer* which is supposed to be attempting to provide support for "rationalism." I think this kind of behavior is worse than irrational; I believe it is indefensible and immoral journalism.

In 1928 Adolph Hitler appointed Joseph Goebbels as the director of the Nazi party. Goebbels had worked as a journalist and knew the power that a lie, told over and over again by influential individuals to the public, could accomplish. Apparently, Paul Kurtz and Kendrick Frazier also published the "Big Lie" that Velikovsky's prediction that the planet Venus is radiating more heat than it receives from the sun was "flatly refuted by spacecraft data." It was never flatly refuted by spacecraft data but was completely supported by the data; Morrison, Chapman, Kurtz, McElroy, Oberg and Frazier apparently have learned Goebbels' lesson quite well.

Morrison has a long history of raising issues which were dealt with by Velikovsky. In 1981 in *Physics Today*, he attacked Velikovsky thus:

"What is the evidence for the encounter with Venus and Mars in about 1500 and 800 B.C.? The physical indications of the cataclysmic events cited by Velikovsky should be abundant. Does the volcanologist see evidence of widespread eruptions near those dates? Do tree-ring analyses indicate a major disruption of climate then? Is there paleomagnetic evidence for fluctuations or reversals in the Earth's magnetic field? Do the ocean sediments preserve records of global temperature change or do the coasts display the scars of flooding or large changes in sea level? Are there meteorite craters on Earth dating from these times? Do the lunar rocks brought back by Apollo indicate widespread melting of the lunar surface during the past few thousand years? All the above should be the case if Velikovsky is correct, but the answer in every case is a resounding 'no'. In the face of such evidence, arguing about what might or might not be possible seems rather pointless."¹¹⁹⁶

To these attacks Shulamit Velikovsky Kogan, a physicist, pointed out that much of the evidence had already been fully presented.

"But the 300 pages of *Earth In Upheaval* are an unbroken record of exactly this.

"To answer Morrison's question by way of example we will quote from the section 'Dropped Ocean Level' from *Earth In Upheaval* by . . . Velikovsky

"R. Daly observed that in a great many places around the world there is a uniform emergence of the shoreline of 18 to 20 ft. . . . Daly proceeds 'Marine terraces indicating similar emergence are found along the Atlantic coast from New York to the Gulf of Mexico . . . along the coast of eastern Australia; along the coast of Brazil, southwest Africa, and many islands of the Pacific, Atlantic, and Indian Oceans; in all these and other published cases . . . the emergence seems to have been simultaneous on every shore' (Daly, *Our Mobile Earth*, p. 178) . . . P. H. Kuenen (*Marine Geology*, 1950, p. 538) . . . writes 'The time of the movement was estimated by Daly to be probably some 3,000 to 4,000 years ago'."

¹¹⁹⁶David Morrison, "More on Velikovsky," *Physics Today*, (April 1981).

"For reversals of the Earth's magnetic field we could quote from the section 'Magnetic Poles Reversed' (*Earth In Upheaval*, pp. 143-147).

"For sediments see the section 'The Floor of the Seas' (*Earth In Upheaval*, pp. 104-107), and for later sediment findings see Velikovsky's article in *The Velikovsky Affair*, (University Books, 1967, pp. 241-243).

"For sudden climatic changes see Chapter XI, 'Klimasturz', (*Earth in Upheaval*, pp. 173-187). In this chapter, there is also a section on 'Tree Rings'.

"The last melting of the moon was discussed by Velikovsky in his memoranda to H. H. Hess of July and August 1969, and in his article 'When was the Lunar Surface Last Molten?' (both reprinted from *Pensée* in *Velikovsky Reconsidered*, Doubleday, 1976).

"Morrison's criticisms of Velikovsky's work, with reference to *Earth In Upheaval*, is a measure of the [lack of] serious consideration Velikovsky has received from . . . Morrison . . ." (*Ibid.*)

As we can see, Morrison is simply oblivious to the evidence presented and goes back on the attack now by raising the question of tree-rings.

Let us now return to Morrison and Chapman's further criticism:

"An especially clear indication that the Velikovskian global disturbances are fictitious is provided by the Bristlecone pines from the arid mountains of the California-Nevada border, [the White Mountains] which provide a continuous tree-ring record that goes back at least to 3435 B.C. These tree rings reveal no climate anomalies at the times of Velikovsky's supposed catastrophes."¹¹⁹⁷

What Morrison and Chapman have this time failed to discuss are two fundamental "facts" or aspects of these Bristlecone pine trees. The first "fact" they have omitted is that the chronology of these California trees is well-known to be uncalibrated by comparing it with a known period of modern climate data and is, therefore, "suspect," as providing evidence of anything.

As John Gribbin explains:

"First . . . the dendrochronologists [tree ring scientists] have to establish that in living trees the [oxygen -16 and oxygen -18] isotopes do not migrate across the tree rings, garbling the temperature record. This can be done by calibrating the outer rings of living trees against the historical record of temperature variations, before using inner rings to find out temperature changes in the more distant past. And among other things, THAT MEANS THROWING

¹¹⁹⁷Morrison, Chapman, *op. cit.*, p. 150.

OUT THE DATA FROM THE FAMOUS BRISTLECONE PINES . . . OF CALIFORNIA, WHERE THERE ARE NO DECENT RECORDS OF MODERN TEMPERATURES TO ESTABLISH THE CALIBRATION."¹¹⁹⁸ (Capitalization added)

But Morrison and Chapman present the validity of the "fact" of this tree-ring chronology which Gribbin tells us, that without decent records of modern temperatures to establish the calibration this record should be *thrown out*. Morrison and Chapman, rather than admit that the record is invalid because it is uncalibrated and on that basis should be thrown out, throw this "suspect" tree-ring record at Velikovsky. Not only is this evidence no decent record of temperatures, it is no decent record as evidence because of the second omission.

The second omission of Morrison and Chapman regarding this evidence is the "fact" that Bristlecone pine trees, during years of climate stress, simply stop producing tree-rings. E. Schulman makes this "fact" explicit.

" . . . such trees [Bristlecone pines] cautiously add no more than an inch to their girth in a century. With so little tissue to nourish they can afford to shut up shop almost entirely during lean years."¹¹⁹⁹

"There is something a little fantastic in the persistent ability of a 4,000 year old tree to shut up shop almost everywhere throughout its stem in a very dry year and faithfully to reawaken to add many new cells in a favorable year."¹²⁰⁰

Therefore, any stress such as darkness or climate stresses posited by Velikovsky would cause any of the surviving few Bristlecone pines to simply shut up shop and stop producing tree-rings. Hence, we now have Morrison and Chapman claiming that Velikovsky's catastrophes should have left signs that would clearly be observed in the tree-rings of Bristlecone pines which show these stresses when in "fact" the real authorities, the dendrochronologists, say that Velikovskian type climate stresses will never be found because these trees will stop producing rings. Perhaps these tree-ring scientists should have a good talk with Morrison and Chapman who might learn that one cannot reject Velikovsky's hypothesis with an uncalibrated record or with tree-rings which simply do not and should not exist based on Velikovsky's hypothesis. Though Morrison and Chapman set out to use this evidence to shut down Velikovsky's theoretical shop, I'm afraid the only thing that they have shut down is their own credibility regarding this evidence as with the preceding evidence.

But let us look a little deeper into this tree-ring business. One of the arguments raised against Velikovsky's history is based on tree-ring analysis whom William E. Stiebing, Jr. the historian calls,

"one of the most accurate methods yet developed. A tree-ring sequence 7,000 years long has been developed from the long-lived bristlecone pine trees of the southwestern United States More recently a 6,000-year tree-ring sequence developed from Irish Oak Trees has been radiocarbon tested, and its calibration curve generally agrees with that established from the bristlecone

¹¹⁹⁸John Gribbin, *Future Weather and the Greenhouse Effect*, (New York, NY 1982), p. 124.

¹¹⁹⁹E. Schulman, *National Geographic Magazine*, (March, 1958), p. 356.

¹²⁰⁰*Ibid.*, p. 368.

sequence. Both sets of calibrated dates generally support conventional chronology"¹²⁰¹

However, the reader is reminded that archeological digs carried out in the Near East showed that the conventional chronology is at least 750 to 800 years shorter than conventional chronology allows. But the tree-rings we are told support the conventional chronology. Now Morrison and Chapman have claimed Velikovsky's history placed him in direct confrontation with the evidence of archeology. However, the archaeologists now say that 750 to 800 years of tree-ring dating that supports the conventional chronology simply does not exist! Therefore, the tree-rings must be wrong on this matter. Are Morrison and Chapman now going to argue that the tree-ring historical-chronology is correct but the expert archaeologists who put down four digs over a two year period are not really experts anymore and that their careful research should be thrown out? Gribbin tells us the lack of calibration for the Bristlecone pine tree chronology of the American Southwest demands that it be thrown out. Something has got to be thrown out to save not only the convention chronology of the ancient Near East but also the reputations of certain researchers. If we throw out the tree-rings then how can they disprove Velikovsky? According to Morrison and Chapman, archeology confronts Velikovsky. Then they must also throw it out. In other words, tree-rings or archeology are now contradicting one another. But, of course, it seems rather clear that in the end it is Velikovsky to whom they will decide to give the boot to "save the appearances" that they consider acceptable.

Morrison and Chapman then argue that no climatic changes at the time of Velikovsky's supposed catastrophes are "preserved in the Greenland ice cores."¹²⁰² What these scientists have omitted is the actual measurements of ablation or melting away of the Greenland ice cap during the period between about 8,500 and 3,000 years ago. This warm period is known as the hipsithermal and the average temperature of the Earth was 4 to 5 degrees Fahrenheit or 2-2.5 Celsius warmer than the present.¹²⁰³ According to Clyde Orr, "A 1-degree shift in mean annual temperature is equivalent to roughly a hundred miles of latitude"¹²⁰⁴ He goes on to say, "A 5-degree rise if maintained a few thousand years would surely melt some of the six million square miles of ice and snow now collected at the poles."¹²⁰⁵ According to H. Lister, careful measurements carried out between 1952 and 1954 on the Greenland ice cape showed Greenland would lose about 1 meter of ice per year with a rise of 1 to 2 degrees Fahrenheit.¹²⁰⁶ Now what would happen to the ice caps during the hipsithermal period when the temperature was 4 to 5 degrees F. higher instead of 1 to 2 degrees F? If we use an ablation or melting rate of 1.5 meters per year, which is quite conservative, being 50 percent greater melting when the temperature was actually 300 percent higher, we get the following: For 5,000 years we melt away 7,500 meters of ice or over 22,500 feet of ice. For 4,000 years we melt away 6,000 meters of ice or over 19,500 feet of ice. For 3,000 years we melt away 4,500 meters of ice or over 14,500 feet of ice.

To all intents, we melt away the entire ice cap or nearly all of it. In fact, during an expedition to northeast Greenland, one of the coldest regions of the island, plant and vegetative matter were discovered being exuded through a cleft in the ice which gave off a powerful vegetative odor that could be sensed 820 feet from the source.

"The silt was examined for fossils by Dr. Esa Hyypa of the Geological Survey of Finland who reported the following:

"*Macropedia Fossils*. The silt examined contained two whole leaves, several leaf fragments, and two fruits of *Dryas octopetala*; [also] a small, partly decayed leaf of a

¹²⁰¹William E. Stiebing, Jr., *AEON*, II:5 (1991), p. 49.

¹²⁰²Morrison and Chapman, *loc. cit.*

¹²⁰³P. Borisov, *Can Man Change the Climate*, (Moscow, 1973), p. 36.

¹²⁰⁴Clyde Orr, Jr., *Between Earth and Space*, (New York, 1961) pp. 160-161.

¹²⁰⁵*Ibid.*, p.161.

¹²⁰⁶H. Lister, "Glaciology (1): The Balance Sheet OR The Mass Balance," *Venture to the Arctic*, ed., R. A. Hamilton, (Baltimore, 1958), p. 175.

shrub species not definitely determinable . . . and an abundance of much decayed small fragments of plant tissues, mostly leaf veins and root hair"1207

The scientists claim that there could be,

"little doubt that the silt is being squeezed up from the base of the ice. As the local bedrock is gneiss, it seems probable that the source is a superficial deposit on the valley floor. The modern aspect of the flora precludes a preglacial time of origin for it."1208

What is observed is a rich flora growing on one of the coldest regions of Greenland at its northeastern end. The scientists claim this vegetation grew recently, meaning during the hypsithermal period. Now, if one of the coldest regions completely melted away to permit this growth, what would happen further south where it was even warmer? Greenland is 1400 miles in length from north to south. It is rather clear that the warmer regions of Greenland's ice sheet also melted to the surface. The measured ablation and calculation indicate complete melting, and the vegetative growth in one of the coldest regions of Greenland confirm this analysis. This can only mean that as Velikovsky claimed, the ice caps were formed suddenly during a catastrophe 3,500 years ago.

As proof that this is the case, planetary dust from the catastrophe had to have been laid down with the ice as the ice cap formed, based on Velikovsky's scenario. According to the established glacial theory, the ice was formed gradually year by year. According to C. E. P. Brooks, "during the Quaternary Ice-Age: . . . the rainfall over the non-glaciated regions was heavier than present rainfall."1209 Rainfall, it need not be stressed too strongly, cleanses the atmosphere of dust. Therefore, during the ice age there was much less dust in the atmosphere than the present era and, accordingly, the deeper ice as opposed to the upper part should have much less dust than this upper region. If Velikovsky's theory is correct, the exact opposite should be the case and the deeper ice should have much more dust than the upper regions. And that is precisely what is found. According to U. Hammer, *et al.*, dust particles in the ice of the Greenland glacier "was up to 100 times as great in the last ice age as at present [and Antarctica contained] an order of magnitude higher."1210

In no way does the present concept of ice cap formation correlate with this fundamental finding. While it is quite clear the evidence from the ice cap is in full agreement with Velikovsky's hypothesis, this evidence clearly denies any validity to Morrison and Chapman's assertion regarding the Greenland ice cap contradicting Velikovsky's theory. The "facts" prove just the opposite. What Morrison and Chapman have done once again is make completely unsupported statements about the ice cap of Greenland which, upon examination, turn out to be contradicted by the actual measurements and other evidence.

Morrison and Chapman ultimately argue that Velikovsky has had a negative influence on science.

"What about Velikovsky's influence on mainstream science? It is difficult to be sure, for the human brain works in mysterious ways, but it seems to us that Velikovsky's work inhibited an open-minded appraisal of catastrophism

¹²⁰⁷Louise A. Boyd, *The Coast of Northeast Greenland*, American Geological Society Special Publication, No. 30, (New York, 1948), p. 132.

¹²⁰⁸*Ibid.*, p. 133.

¹²⁰⁹C. E. P. Brooks, *Climate Through the Ages*, 2 ed., (New York, 1970), p. 166.

¹²¹⁰C. U. Hammer, *et al.*, "Continuous Impurity Analysis along the Dye 3 Deep Core," *American Geophysical Union Monograph*, Vol. 33, 90, (1985), p. 1364.

rather than assisted it. The astronomers who attacked *Worlds in Collision*, and others who debated with Velikovsky's followers during the 1960's and 1970's, were repelled by the obvious illogic and absurd conclusions of purported recent cosmic catastrophes. If anything, those scientists who felt it necessary to defend the status quo may have been dissuaded from considering concepts of natural calamities with open minds."¹²¹¹

Harold Urey had been defending the status quo against Velikovsky most vehemently for several years at that time. He was well embroiled in the affair since the 1950's when *Worlds in Collision* was published. Was Urey so "repelled" by Velikovsky's hypothesis, as Morrison and Chapman claim that he abandoned the catastrophist concept? Strangely, this is not at all the case. Instead of fleeing from the concept of cosmic catastrophes, Urey plunged in with both feet.

In 1965, in the journal *Science*, Urey wrote that a possible collision of a comet with the earth or some other body produced such a violent explosive impact that rocks and terrestrial water was expelled with such force that it was captured by the Moon. Does this sound as though Urey was so repelled by Velikovsky's thesis that he abandoned such cosmic catastrophes? Of course not; he did just the opposite: In 1973 Urey returned to the fray with the full fledged suggestion in *Nature*, that cometary collisions closed and/or opened the major geological periods,"¹²¹² Thus instead of shunning extraterrestrial catastrophism in the 1960's and 1970's, as Morrison and Chapman present as the response to Velikovsky by those who dealt with him and debated him, Urey did just the opposite. Instead of Velikovsky's influence on mainstream science "repelling" Urey, it actually attracted him to suggest cosmic catastrophism. And, I do agree with Morrison and Chapman's remark of how "the human brain works in mysterious ways."

But there were other proposals of cosmic catastrophism in the 1960's and 1970's contrary to everything Morrison and Chapman say on this matter. In March 1965 Stephen Jay Gould wrote an article, "Is uniformity necessary?" in the *American Journal of Science*, Vol. 263, pp. 223-228. That is how Gould evaded the concept of catastrophism. In 1970, Digby J. McLaren in the *Journal of Paleontology*, Vol. 44, pp. 801-815, suggested the possibility that a major extinction in the past could be considered to be caused by an extraterrestrial agent. This was how McLaren expressed his repulsion of Velikovsky's hypothesis. Stephen Jay Gould and Niles Eldredge, both of whom were clearly cognizant of Velikovsky's theories, published their theory of "Punctuated equilibria: An alternative to phyletic gradualism, in *Models in Paleobiology*," edited by T. J. Schopf, in 1972 in San Francisco, which suggest cosmic catastrophes have created new niches into which new organisms evolved. This, then is how Gould and Eldredge chose to express how they were repelled by Velikovsky's theory. In 1971, D. A. Russell and W. Tucker were so repulsed by Velikovsky's cosmic catastrophic scenario that they wrote in *Nature*, Vol. 229, pp. 553-554, of "Supernovae and the extinction of the dinosaurs." And there are several other papers as well which may be found.¹²¹³ Yes, the human brain works in mysterious ways.

Morrison and Chapman further suggest that "the catastrophism of . . . Velikovsky has little to offer."¹²¹⁴ Therefore, one would naturally expect astronomers to stay away from ancient myths and history as evidence of recent extraterrestrial catastrophism of which Morrison and Chapman accuse Velikovsky.

"Velikovsky did not base his theory of planetary collisions on new evidence from geology or astronomy. He made no observations, did no experiments, and carried out no calculations. He was motivated to find a natural

¹²¹¹Morrison, Chapman, *op. cit.*, p. 151.

¹²¹²*Nature*, Vol. 242, (March 12, 1973), pp. 32-33.

¹²¹³*The Mass-Extinction Debates: How Science Works in a Crisis*, ed. William Glen, (Sanford, Calif., 1994) in the "References Cited," pp. 301-348.

¹²¹⁴Morrison, Chapman, *loc. cit.*

explanation for a variety of myths and ancient traditions, cutting across many cultures, that recounted natural and supernatural catastrophes experienced millennia ago. He suggested that these events had been global and that they happened as a result of near-collisions of other planets with the Earth."¹²¹⁵

This statement contains a fundamental error. Velikovsky is accused of finding "natural explanations for . . . natural and supernatural catastrophes." As Morrison and Chapman should know, a "natural explanation" of a "natural" catastrophe cannot at the same time be an explanation of a "supernatural" catastrophe. A "supernatural" catastrophe by definition cannot have a "natural explanation." But let us examine how repelled some scientists were by Velikovsky's theory and the method by which *they* arrived at another theory of recent extraterrestrial catastrophism.

In 1982 British astronomers S. V. M. Clube and Bill Napier wrote a book, *The Cosmic Serpent* which is advertised on the inside cover blurb with the following,

"The cosmic serpent was a giant comet that terrorized mankind in prehistoric times. As a fiery dragon and hurler of thunderbolts it wrought destruction and disaster upon the Earth. Comets have been the object of superstition and fascination since cavemen looked up at the night sky and saw 'shooting stars.' In the intervening millennia, much information about comets has been gathered, but even today we do not properly understand these cosmic phenomena.

"In the *Cosmic Serpent*, two internationally respected astronomers propose radical new theories about the origin and future of the Earth, focusing on worlds shaped by catastrophe. They claim that impacts from comets, asteroids, and meteoric bombardments caused devastation on a global scale and brought on massive evolutionary changes, including the demise of the dinosaurs. *And they say these cosmic events gave rise to ancient myths, inspired prophets and philosophers and affected the creation of astrological sciences and ancient calendars.*

"Warning that 'there is something here to outrage everyone,' Clube and Napier present impressive and persuasive arguments that challenge established scientific thought, mythological and biblical interpretation, and historical, archeological and anthropological evidence. Hardly an area of modern knowledge remains unaffected directly or indirectly by the ideas put forth in this book

". . . *Perhaps most provocative of all are assertions concerning both the Old and New Testaments. Here Clube and Napier reveal that cometary collisions with the Earth were responsible for the Great Flood of Noah's time as well as parting of the Red Sea during the Israelite Exodus from Egypt.*"¹²¹⁶ (Emphasis added)

¹²¹⁵*Ibid.*, pp. 148-149.

¹²¹⁶S. V. M. Clube, Bill Napier, *The Cosmic Serpent*, (New York, 1982), blurb, inside cover.

Patrick Moore tells us that the support for this remarkable theory is not based, as Morrison and Chapman state, "on new evidence from geology or astronomy," but from "records left by old civilizations—the Romans, Greek, Mayas, Incas, Chinese, Babylonian's and so on, even the New Zealand Maoris and the aborigines of Australia."¹²¹⁷ Doesn't this all sound suspiciously like Velikovsky's theory based on these very myths? In fact, their catastrophic theory is very similar to Velikovsky's theory.

Let us get the flavor of this theory from Clube himself who states,

"Thus, before 3000 B.C., our ancestors would have regularly observed at least one large comet in the sky. It was probably a brilliant though essentially harmless spectacle, but also frequently an awesome one when orbital coincidences brought the earth particularly close At the epoch in question, however, around 3000 B.C., a major fragmentation of the primary body would produce an additional battery of comets; it would not be surprising if onlookers subsequently thought they were witnessing a battle for mastery over the sky, and that this was in some way associated with the assaults on the earth that inevitably followed. These assaults due to encounters with the core of the [comet] stream would in effect be global bombardments by Tunguska and super-Tunguska type bodies that would leave an indelible memory for the surviving humans and a lasting fear of the god in the sky.

"Such memories would be of local floods and widespread destruction by fire over areas the size of a nation. More or less contemporary migrations would ensue all over the world, with some lands lying sterile for centuries. There would be a trend toward defensive building both against assaults from the sky and against opportunist attacks from human marauders. In time, with all but a few vestiges of the violent destruction erased, there would be an extended period of extreme social disorder—the end of a golden age, or a dark age—to mark the course of history and the impact from above."¹²¹⁸

Each one of the concepts presented by Clube and Napier are almost exactly the same ones presented by Velikovsky. Clube and Napier speak of "at least one large comet" seen in the sky before 3000 B.C. Velikovsky speaks of one giant comet which he identifies as the planet Venus on a highly elliptical orbit observed by early man. Clube and Napier speak of battle observed in the sky while Velikovsky speaks of ancient man depicting battles observed in the heaven by celestial bodies. Clube and Napier speak of global bombardments and mass migrations while so too does Velikovsky. Clube continues to expand these concepts.

"We can well understand how a frenzied response might arise in the form of religious temples to propitiate the violent gods and astronomical observatories to anticipate future returns, though we have until now always assumed it was mere calendric or navigational requirements that arbitrarily inspired the growth of astronomy at these times, and mere technology that inspired a new

¹²¹⁷Patrick Moore, *Fireside Astronomy*, (New York, 1992), p. 193.

¹²¹⁸S. V. M. Clube, "Giant Comets and Their Role in History," *The Universe and its Origins*, ed. S. Fred Singer, (New York, 1990), pp. 158-159.

generation to produce the pyramids and Stonehenge. With the passage of time, of course, the encounters would weaken and there would be only fireballs and declining comets to remind one of the former events. But at some stage there might at any time be a further fragmentation and a revival of earlier terror. The scribe would scan the ancient records and attempt to prophesy the course of events, but eventually there would be no avoiding further multiple bombardment by Tunguska and super-Tunguskas, with massive destruction of cities and widespread incineration of crops and land. Massive migrations would again take place as survivors seek to escape, and doubts would be raised concerning the efficacy of prayer; a dark age would follow and a renaissance in due course with new questions about religion and cosmology. And then with the passage of time, the whole process can be expected to repeat itself, continuing until the core of the giant comet has been completely whittled away. We might even envisage destruction so great, and dark ages so effective, that only the dimmest memories will later exist of giants that once walked the earth, of heavenly clouds that a creator once built in the sky, of prophets and messiahs who warned of doom and salvation, and of floods and cataracts of fire that were used to cleanse the earth. We might anticipate that the intellectual confusion would reach new heights when the comet—asteroid deities known to be responsible for all the mayhem finally disappear from sight—which they apparently did during the first millennium B.C. We might anticipate the worship of new invisible gods or the diversion to new planetary gods. We might even learn to agree with an Aristotle, a Ptolemy, or a Newton as they seek to dismiss the thunderbolts of a previous generation and restore a sense of order in heaven and on Earth."¹²¹⁹

Thus, after all is said and done many years after Velikovsky wrote, these other astronomers were so repelled by Velikovsky's theory that they created a theory—sans interplanetary instability—which contains nearly every concept of cosmic catastrophism to the Earth that Velikovsky proposed. For example, the destruction of cities in the ancient world by cataclysms beyond the destructiveness of mankind found by Schaeffer and Finley are described by Clube as "massive destruction of cities." Or the worship of sky gods, "violent gods" described by Clube, are also described by Velikovsky. So as one can clearly see, the astronomers were so repulsed by Velikovsky's cosmic catastrophic theory that they created a theory which incorporates nearly all of Velikovsky's concepts of recent cosmic catastrophism.

Sir Fred Hoyle, who has become an ardent supporter of Clube and Napier's catastrophic evidence, states that

"Whole heads of mammoths perished all in a moment. They did so by sudden melting of the permafrost on which they spent their lives, causing them to become immersed in icy water, which then refroze within a matter of hours. Only a blast from the sky could have had such an effect

"We . . . see what it was that rained down fire on the cities of Sodom and Gomorrah, and it had nothing to do with what later preachers claimed it had. We can also have a pretty good idea what it was that Joshua saw in the sky [at

¹²¹⁹*Ibid.*, p. 159.

Beth Heron], not the sun standing still but the distant glow of an immense fireball. The city of Jericho lies on a major earthquake line, so that the fall of the walls of Jericho can readily be attributed to a major earthquake. It is also true that lights in the sky are often associated with large earthquakes Viewed in this way the writer of the biblical story can be seen to have done very well. He got the two essential features, the bright light in the sky [at Beth Heron] and the shattering of the city [at Jericho]."¹²²⁰

Here a world respected astronomer claims that there were catastrophes in historical times and that interpretation of the *Bible* and of myths helps to explain them. He supports, in part, the thesis of Clube and Napier which in many respects is the same as that of Immanuel Velikovsky except that their means of destruction is a gigantic comet and not the planet Venus seen as a comet.

That is how disgusted these astronomers were! The only real difference between the theories of Velikovsky and that of Clube and Napier is the agent of the earth's destruction; in most other aspects the theories are quite alike. Because Clube and Napier suggest a giant comet as their agent, which eventually broke apart and became a group of comets or a meteoric stream which is more acceptable to establishment astronomers than Velikovsky's planetary orbital instability, is not definitive evidence to reject Velikovsky's hypothesis for which it is admitted there is a "prejudice" against this concept.

Recently Eugene H. Levy pointed to the concept that there were recent catastrophes in *historical times* and that myths and legends are the record of these events. In a collection of papers on catastrophes edited by Tom Gehrels, Levy stated:

"It is conceivable that other impact events have occurred within or near historical times. I have occasionally mused that one or two such events may be recorded vaguely in our cultural memory, perhaps meandering and evolving through generations of myth and legend possibly ending up as religious stories of miraculous events. We have no evidence one way or the other; but it is at least conceivable."¹²²¹

But Morrison and Chapman will not even allow that such catastrophic events in historical times are "conceivable," but rather it is "unthinkable." It becomes unthinkable only to the dogmatic and prejudiced mind. As Duncan Steel states:

"The real problem for science is astronomers, in America in particular, became so entrenched and vehement in their criticisms of Velikovsky's astronomical nonsense that their mind also became instilled with not only a rejection of, but also a nonconsideration of, the possibility that the myths and records of past civilizations might contain important information about what was happening in the sky in pre-modern times. In fact, the similarity between the legends of disparate human cultures are startlingly similar. In scientific publications I have pointed out that Australian Aborigines and New Zealand Maoris have

¹²²⁰Sir Fred Hoyle, *The Origin of the Universe and the Origin of Religion*, (London, 1993), pp. 40-41.

¹²²¹Eugene H. Levy, "Early impacts: Earth emergent from its cosmic environment," *Hazards Due To Comets and Asteroids*, T. Gehrels, ed., (Tucson, 1994), p. 6.

oral traditions of strange rocks falling from the sky, causing awful fires and many deaths, and this scenario is common in the myths of other peoples. On one hand astronomers have prided themselves in instructing geologists that impact catastrophes were responsible in part for shaping of the planet, but on the other hand, they have been blind to the fact that they have made a uniformitarian assumption when it comes to their own science. That the sky as it is now, is as it ever was, at least while humans have walked the Earth. There is ample evidence not only from historical records of various forms, but also from the analysis of data from this century . . . that around 5,000 years ago the sky did not appear as quiescent as it does now, and that since that time there have been other disruptions of the heavens producing conflagrations here below."¹²²²

It is, in reality, only prejudice that stands in the way of the astronomers interpreting these catastrophic events from a different perspective, using myths and legends just as Velikovsky did.

With regard to concepts of recent historical catastrophism on the Earth of cometic origin, Hoyle states that:

"Science was made to support a continuing amnesia over the earth's past history, an amnesia which science has worked hard to maintain almost to the point of religious intensity. Indeed one might see the modern decline of the Christian Church as a consequence of science now doing its job more effectively than the Church is currently able to do

"[There] are . . . abiding consequences of shutting out the sky as a causative agent, [recent catastrophes] as it was done a millennium and a half ago. Science has itself locked the door of its own cell, throwing away the keys through its own barred window, not a situation that calls for much celebration

It could be seen as curious that society would seek to investigate distant galaxies while at the same time ignoring all possibility of serious impacts with the earth, surely a clear example of amnesia in action . . . only blind amnesia can explain it."¹²²³

And, might I add, it is only callowness, anger, and blindness on the part of Velikovsky's critics that can explain such unscholarly criticism as that found in *The Skeptical Inquirer*.

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¹²²²Duncan Steel, *Rogue Asteroids and Doomsday Comets*, (New York, 1995), pp. 155-156.

¹²²³*Ibid.*, pp. 60-62.

CONCLUSION

"The main weakness of the totalitarian (as compared with the democratic) state is the difficulty, if not impossibility of obtaining independent sincere criticism."¹²²⁴

Finally, what can one conclude about *The Skeptical Inquirer* with respect to Velikovsky and his theories? For one thing, its record is not rational but irrational, not ethical but fraudulent, not open and democratic but totalitarian and filled to the brim with vituperation, misrepresentation, authoritarianism and vilification. The ceaseless misrepresentations in that journal bespeak unending malice. The willingness to employ such tactics bespeak yellow journalism in the service of anti-Velikovsky propaganda. The miserable character of the behavior of *The Skeptical Inquirer* was nicely summed up by Joseph Goodavage especially with regard to *The Skeptical Inquirer* and its treatment of Velikovsky:

"The scientists who formed the Committee for the Scientific Investigation of Claims of the Paranormal, have declared war on 'all irrational, superstitious and obscurant' beliefs which block the path of true scientific progress. One can understand and truly appreciate the stated purposes of these sincere men (eight of the original group are Nobel laureates). Moreover, I admire and fully endorse their alleged objectives—to help eradicate ignorance, to expose charlatanry and fraud, to encourage clear thinking, to rid society of superstition and irrationality, and to encourage the use of the scientific method in our affairs and beliefs.

"I believe in and subscribe to that—*except when in pursuing these desirable goals totalitarian methods are employed*. By this, I mean the refusal to discuss frankly and rationally that which they publicly condemn. I mean personal vilification. I mean character assassination. I mean conspiracy, elitism, or *any* collusion formed to smother ideas the Committee does not like or agree with—ideas against which the Committee previously sat in judgment. And I mean the use of intimidation to enforce the idiotic dogma that theirs is the only true and approved method by which anyone may pursue the truth about Nature's secrets.

"When an elite group protected by a self-proclaimed cloak of infallibility vilifies, personally attacks, and intimidates a man championing a new theory or idea, it is no better than a gang. It is not invincible or infallible and should

¹²²⁴George Sarton, *Science Through the Golden Age of Greece*, (New York, 1980), Dover Books, p. 414.

be exposed. In 1950 such a gang forced Immanuel Velikovsky's publisher to stop printing his book, *Worlds in Collision*. That gang succeeded with its pressure tactics because they were academics who wrote textbooks for that publisher, and therefore they had a lot of clout. Their cover story was that Velikovsky's theories were grievously in error and that he was a crackpot. The publisher caved in.

"Nearly three decades later, the inheritors of the squeeze play continue the attack on the old man

"The truth is they hate his *influence*."¹²²⁵ (Goodavage's emphasis)

But can the frauds and misrepresentations perpetrated against Velikovsky be stopped by these same individuals? Well, perhaps it can. On the other hand they can turn around and denounce this condemnation of their actions as "all lies" similarly as did Morrison with regard to the Doubleday ad.

About two thousand years ago another group of scholars and scientists declared war on all irrational, superstitious and obscurant beliefs. They were known as stoics, whose most famous spokesman caught the temper of rationalism of that time with the following statement: "Life is short, art is long, experience painful, judgment difficult." The stoics knew life was difficult, sometimes painful and short and judgment difficult to come by. Yet they said that in spite of these difficulties people should act in a humane and rational manner. Its greatest exponent politically was Marcus Aurelius who, during his emperorship, allowed propaganda to be published against the Christians by a Roman zealot named Celsus in a book *True Word*.

"Celsus was alarmed by the spread of Christianity, by its scornful hostility to paganism, military service, and the state; . . . A good citizen, he thought should conform to the religion of his country and his time without public criticism of its absurdities; these did not much matter: what counted was a unifying faith."¹²²⁶

Celsus during the time of Aurelius mocked, insulted and attacked the Christians with every propaganda weapon at hand. Needless to say, these attacks bore fruit and led to the ritual murder of later Christians for believing in a merciful deity. According to Broad and Wade,

"In his book *The Decline of the West*, the philosopher Oswald Spengler cited fraud by scholars as one of the signs of a decadent civilization. It is not necessary to believe Spengler's thesis to be alarmed."¹²²⁷

When editors, journalists, science writers and scientists themselves forget the simple Mosaic law, "Thou shalt not bear false witness," when dealing with the work of a sincere scholar, then something is deeply wrong and deeply disturbed. As George Sarton, the science historian said,

¹²²⁵Joseph Goodavage, "Scientists in Collusion," *Storm on the Sun*, (New York, 1979), pp. 124-125.

¹²²⁶Will Durant, *Caesar and Christ*, (New York, 1944), p. 607.

¹²²⁷William Broad, Nicholas Wade, *Betrayers of the Truth*, (New York, 1982), p. 223.

". . . philosophers may gloss over aberrations, but for men of science that is an unpardonable sin. A *paideia* based on the lies is bad and the better it looks on the surface the more seductive it is, the more pernicious."¹²²⁸

In Rome, rationalism failed because anyone could see through the sham of a rationalist society based on military enslavement of other peoples and ritual assassination of non-believers, that is, Christians.

Misrepresentation of Velikovsky, as of Christians even by exalted members of the scientific community, is still misrepresentation, and in "fact," irrational. The treatment of Velikovsky in the pages of *The Skeptical Inquirer* is not open, balanced or even honest. When one observes scientists, editors and science writers acting with such a callous disregard for truth, and slandering a man with reprehensible falsehoods, it is no wonder people are running away from them, from their form of rationalism, from their lack of decency and from their science. In the face of the intellectual and scientific, journalistic violence exhibited by *The Skeptical Inquirer* from the outset against Velikovsky's ideas and the man himself, rationalism failed before it ever began. Nevertheless, I point out with certain reservations such as the destruction in ancient times of the heliocentric theory of Aristarchus:

"In all of history, it has never been possible to reverse the progression of scientific ideas. An intellectual or political tyranny may succeed in suppressing an idea for a generation, or even several generations, but it does so at the peril of losing the respect and confidence of the generations that follow."¹²²⁹

The aim of *The Skeptical Inquirer* has always been to degrade and humiliate Velikovsky and his concepts. The humiliation of another or of his ideas and followers should never be the goal of a group of rational, "honorable men." Rather, their behavior is a form of intellectual and scientific fascism. And I do not now hesitate to say that *The Skeptical Inquirer* operates without the principles of decency.

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¹²²⁸George Sarton, *Ancient Science Through the Golden Age of Greece*, (New York, 1993), Dover reprint, p. 427.

¹²²⁹Jeremy Bernstein, *Experiencing Science*, (New York, 1978), p. 31.

COMETARY VENUS, By Dwardu Cardona

Introduction

As I pointed out in 1980,¹²³⁰ Immanuel Velikovsky's *Worlds in Collision* is not a faultless work. What work ever is? Even so, regardless of how right or wrong Velikovsky might have been in this or that—and this writer has criticized him enough in the past—the work in question still contains, to this day, many a valid disclosure. Perhaps prime among these is his claim that, up until the fifteenth century B.C., and maybe even for a time thereafter, the planet Venus appeared in Earth's sky as a comet.¹²³¹ Personally, I'm not quite sure whether one can hold to Velikovsky's postulated dates in this matter, but, on the other hand, the mytho-historical record seems to leave no doubt that the Cytherean planet did, indeed, appear in cometary form some time in man's past.

The past cometary nature of Venus is a topic that continues to be discussed to this day, with many an argument having been offered for and against the contention, but in the forty-five years since the publication of *Worlds in Collision*, the documentation, both *pro* and *con*, has been scattered over a wide range of publications and separated by time. Additionally, certain proponents of cosmic catastrophism, including myself, have utilized the data in question to bolster cosmic scenarios other than Velikovsky's original one.¹²³² And, to make matters worse, as far as these alternative scenarios are concerned, the data concerning cometary Venus has been buried within a larger array of evidence which, while it might strengthen the general views presented, somewhat beclouds the issue. The strength of the evidence pertaining to this topic has therefore been weakened and it is the attempt to reinforce it that has prompted me to write this chapter. For that reason little, if any, that I have to offer below can be considered new, but at least the available

evidence—that collated by Velikovsky *and others*—including the refutation of the criticisms that have been brought against it, will here be presented in one place. And, in order not to obfuscate the issue any further, the data will be presented in isolation, that is, without any reference to Velikovsky's particular, or any other, scenario, so that the evidence will be allowed to stand or fall on its own strength.

Even so, at the very outset I have to submit a stipulation which can best be stated in the following words: The issue at hand has nothing to do with whether Venus is, or ever was, a comet, *but with whether the collective record of ancient man presents the planet in question as having once had the form of a comet*. If by comet we understand a dirty cosmic snowball, or a snowy dirtball, as is presently assumed by mainstream astronomers, obviously Venus is not, and could never have been, a comet.

But are *all* comets really dirty snowballs?

¹²³⁰D. Cardona, "Other Worlds, Other Collisions," read at the San Jose Seminar sponsored by KRONOS, August 30, 1980.

¹²³¹I. Velikovsky, *Worlds in Collision* (New York, 1950), pp. 163–167.

¹²³²See, for instance, D. Talbott and E. Cochrane, "The Origin of Velikovsky's Comet," KRONOS X:1 (Fall 1984), pp. 26 ff.; D. Cardona, "The River of Ocean," *Chronology and Catastrophism Review* (1989) XI, pp. 40–42; D. Talbott, "The Great Comet Venus," *AEON* III:5 (May 1994), pp. 5 ff., among various others.

The Lesson of Shoemaker–Levy 9

The impact of Comet Shoemaker–Levy 9 on the surface of Jupiter in July of 1993 is now history. This comet, we have been told, had been ripped into twenty-one separate pieces by Jupiter's powerful gravity when it had passed within 10,000 miles of the gaseous giant earlier in 1992. But because no evidence of any released water molecules were detected during and after the July 1993 impact, astronomers are now stating that they cannot be sure that Shoemaker–Levy 9 had actually been a comet and that it might have been a shattered asteroid instead.¹²³³ After all, a dusty asteroid might grow both a coma and a tail, commented Space Telescope astronomer Hal Weaver.¹²³⁴ But in view of the ascendant belief among astronomers that asteroids are really the remains of comets, does it really matter that Shoemaker–Levy 9 may now be considered to have been a fragmented asteroid rather than a fractured comet?

Although there can be compelling differences between comets and asteroids, it can also be difficult at times to distinguish between the two. In fact, there seems to exist strong circumstantial evidence to support the hypothesis that comets can evolve into asteroids and that, in a way, comets and asteroids may not, after all, be separate entities. Thus, for instance, astronomers are loath to regard Phaethon, an Apollo asteroid and the parent body of the Geminid meteor stream, as a comet despite its cometary characteristics simply because the object is rocky and thus at odds with the belief that comets are primarily composed of cosmic ice. On the other hand, the idea that Phaethon may be the degassed nucleus of a comet has been suggested.¹²³⁵ Richard Kerr has even gone as far as submitting that the Apollo asteroids, which move on a highly elliptical orbit, are actually the remains of defunct comets.¹²³⁶ And then, old photographs of an Earth–grazing asteroid revealed that this particular fragment had previously boasted a small tail which technically renders the object as another degassed comet.¹²³⁷ So, also, with Shoemaker–Levy 9, the twenty-one fragments of which all exhibited comas and a tail, now described as steady streams of dust.¹²³⁸

There are other large bodies in the Solar System which may represent transitional stages between comets and asteroids. Thus, for instance, asteroid 944 Hidalgo follows a distinctly comet–like orbit and displays color fluctuations, which fluctuations could well depict the final stages of outgassing or, as one observer put it, "the last gasp of a dying comet."¹²³⁹ Other asteroids with comet–like orbits include 1983SA and 1983XF. Then there are comets which display asteroid–like orbits, such as comet Arend–Rigaux and comet Neujmin–1 which, when first discovered, were seen to possess cometary comas. These comas now seems to have disappeared, suggesting that these comets are on the verge of death. Some astronomers have even tendered the suggestion that these comets now appear to be asteroids.¹²⁴⁰ What is becoming clear, therefore, is that if asteroids are the degassed nuclei of comets, comets cannot all be dirty snowballs.

Comet–like objects which are not comets are not, however, restricted solely to asteroids. The recently discovered Chiron, considered something of a mini–planet, in an unstable orbit between Saturn and Uranus, is between 300 and 400 kilometers in diameter. Yet it, also, has been considered by some as a

¹²³³S. Flamsteed, "The Great Comet Crack-Up," *Discover* (January 1995), p. 32.

¹²³⁴*Ibid.*

¹²³⁵*New Scientist* (December 10, 1988), pp. 34–38.

¹²³⁶R. A. Kerr, "Could an Asteroid Be a Comet in Disguise?" *Science* (1985) 277, p. 930.

¹²³⁷*New Scientist* (September 12, 1992), p. 17.

¹²³⁸S. Flamsteed, *op. cit.*, p. 32, and see also the color photograph on p. 28.

¹²³⁹*New Scientist* (December 20/27, 1984), pp. 46–48.

¹²⁴⁰*Ibid.*

gigantic comet nucleus.¹²⁴¹ Observations at Kitt Peak National Observatory in Arizona have indicated that not only does Chiron travel in a comet-like orbit, it actually possesses what was first described as a faint coma,¹²⁴² and later as a *huge* coma of gas and dust which brightened the object considerably.¹²⁴³ So that if the nuclei of comets can be at least as large as Chiron, the difference between comets and the idea that Venus *could* at one time have been a comet becomes merely one of size.¹²⁴⁴

Has any comet with a nucleus the size of Venus ever been observed in recent years? The answer, of course, is a resounding NO. But, of itself, this does not necessarily mean that nothing of the sort could have been observed in the past.

So what, then, constitutes a comet? As far as our ancient forefathers were concerned, a comet was a celestial object that exhibited a hair-like coma and/or tail. The very word "comet" derives from the Latin *cometa*, through the Greek *kometes*, which simply means "long haired." In Maltese, the native language of the present writer, a comet is still referred to as *kewkba bixxuxa* or *kewkba bix-xaghar*, both designations meaning a "star with hair" or "hairy star." The question with which we are concerned, therefore, boils down to this: was the planet Venus, according to the collective record of ancient man, ever described as having once boasted a hairy appearance and/or tail? And here the answer is a resounding YES.

The Extra-Biblical Sources—A Bad Choice

Not all of the evidence enumerated by Velikovsky on the subject is valid. Having, for instance, come across references to the past cometary nature of Venus in the literature of the Sumerians, the Assyro-Babylonians, the ancient Chinese, the Greeks and the Romans, he must have felt that his own ancestors, the ancient Hebrews, would not have been silent about the subject. One can almost see him poring over Biblical and extra-Biblical sources looking for what he sought and, in his zeal, he really did believe to have found it. He thus wrote that the Hebrews had it stated that fire hangs down from the planet Venus,¹²⁴⁵ which description must have seemed cometary enough to him, and references the Tractate Shabbat (156a) of the Babylonian Talmud as his source.

But here I have to agree with Bob Forrest,¹²⁴⁶ one of Velikovsky's most vehement critics, in that Velikovsky obviously stretched the truth since all that is said in this astrological section of the work in question about Nogah, *i.e.*, Venus, is the following:

"He who is born under Venus will be wealthy and unchaste [immoral]. What is the reason? Because fire was created therein."

Which is hardly the same as "fire is hanging down from the planet Venus."

¹²⁴¹*Ibid.*

¹²⁴²*Ibid.* (May 13, 1989), p. 35.

¹²⁴³*Ibid.*, (August 25, 1990), pp. 48–51.

¹²⁴⁴In this respect, see also C. J. Ransom, *The Age of Velikovsky* (New Jersey, 1976), p. 75.

¹²⁴⁵I. Velikovsky, *op. cit.*, p. 165.

¹²⁴⁶B. Forrest, *Velikovsky's Sources*, Vol. 4, (Manchester, 1982), pp. 299–300; *idem*, *A Guide To Velikovsky's Sources* (Santa Barbara, 1987), p. 27; *idem*, "Venus and Velikovsky: The Original Sources," *The Skeptical Inquirer*, Vol. 8 (Winter 1983–84), pp. 155–156.

Velikovsky also stated that the Hebrews referred to "the brilliant light of Venus [which] blazes from one end of the cosmos to the other end."¹²⁴⁷ But as Forrest also pointed out,¹²⁴⁸ this, too, is a garbled interpretation of a passage in the *Midrash Rabba* which reads:

"The Lord will create over the whole habitation of mount Zion, and over her assemblies, a cloud and smoke by day, and the shining of a flaming fire by night . . . The expression *shining* indicates that its brilliance will shine from one end of the world to the other."¹²⁴⁹

Here, once again, it is obvious that Velikovsky was carried away by his zeal. The word "shining" in the above quote is rendered *nogah* in Hebrew, which word also happens to be the Hebrew name for the planet Venus. Thus, to Velikovsky, the "shining" whose "brilliance will shine from one end of the cosmos (or world) to the other" must have referred to Venus—hardly the best of evidence, even if the argument could be considered valid, for the past cometary nature of Venus.

What, then, constitutes *valid* evidence?

Inanna—Queen of Heaven

Inanna, which name translates as "Queen of Heaven," was the Sumerian name for the planet Venus as also its goddess. This identification has recently been challenged by Moe Mandelkehr who wrote that:

"Clube and Napier explained that Inanna/Ishtar became associated with the planet Venus at a late date in history . . . Clube and Napier did not invent the idea, which is in the literature and appears to be well established . . . I agree that goddesses Inanna and Ishtar were well identified with the Morning Star and Evening Star in the third millennium BC, but they weren't identified with the planet Venus until much later, when people came to the conclusion that the early references to the Morning Star and Evening Star must have meant Venus."¹²⁵⁰

But, although Clube and Napier *do* offer an explanation of this transference of names, and although they *do* relate this to Ishtar—Inanna is not really named by them—the explanation they offered cannot be accepted for two important reasons: (a) the explanation is based on middle Persian texts which relate to Anahit and not Ishtar, and (b) the explanation is based on the opinion of Nyberg who interpreted

¹²⁴⁷I. Velikovsky, *op. cit.*, pp. 164–165.

¹²⁴⁸B. Forrest, *A Guide to Velikovsky's Sources* (Santa Barbara, 1987), pp. 27–28; *idem*, "Venus and Velikovsky: The Original Sources," *The Skeptical Inquirer*, Vol. 8 (Winter 1983–84), pp. 159–160.

¹²⁴⁹*Midrash Rabba* XXI:245a.

¹²⁵⁰M. Mandelkehr, "The Comet Wasn't Venus!" *Chronology and Catastrophism Review* XIV (1992), p. 37.

Anahit as the Milky Way, an interpretation that Clube and Napier themselves did not accept.¹²⁵¹ Besides, as Ev Cochrane was later to ask,¹²⁵² *what* literature is Mandelkehr referring to when he states that Inanna became associated with Venus "at a late date in history"? *Where* is this "well established"? On the contrary, as Wolfgang Heimpel indicated, the identification of Inanna as Venus is apparent "*in all historical periods*" and traces back to *prehistoric times*.¹²⁵³

What is borne out by the literature is that Inanna/Venus did not present the appearance the planet presents at present. Thus, in a Sumerian hymn, Inanna is twice referred to as "the *strange star*"¹²⁵⁴ which leads one to ask: what is so strange about the planet Venus today? Inanna is also described as the queen of the zenith,¹²⁵⁵ a celestial position which, at present, Venus cannot achieve. Moreover, Inanna's well-known symbol is curious in that it resembles neither a star, nor planet, nor a female figure (see Figure 1). One of the usual interpretation of this symbol is that it represents an ornate reed bundle, the likes of which are found depicted on the sides of Mesopotamian sacred buildings or as free-standing pillars at other holy enclosures. Such bundles, or baroque pillars, however, are only themselves three dimensional replicas of Inanna's symbol. Although these symbols are predominantly encountered at Uruk, they have recently been found flanking a four-tiered cult stand from Taanach, in Israel,¹²⁵⁶ concerning which Glen Taylor has remarked that they "resemble the pillars that frequently marked temple entrances in Syria-Palestine."¹²⁵⁷

Despite his severe criticism of Velikovsky at the 1974 Symposium held by the American Association for the Advancement of Science, Peter Huber was forced to state that: "Admittedly, Velikovsky's conclusions may contain some truth, even if his arguments are incorrect."¹²⁵⁸ And it was he who noticed that "the Inanna symbol sometimes looks like a comet," although he immediately added that "the similarity is not borne out by the more elaborate representations."¹²⁵⁹ But, as Lynn Rose was later to point out, the Inanna symbol looks like a comet not only *sometimes* but *all of the time*, and that this resemblance is "borne out by the more elaborate representations" of it.¹²⁶⁰ "Each of the Inanna symbols," wrote Rose, "has a roundish head, and a gradually spreading tail that reaches a length several times the diameter of the head. What more could one want?"¹²⁶¹

The cometary nature of Venus is also borne out by ancient texts which laud Inanna as "the pure torch that flares in the sky, the heavenly light, shining bright like the day, the queen of heaven."¹²⁶² "Holy Torch!" it is written of her, "You fill the sky with light" and "Radiant Star, the Great Light that fills the sky"¹²⁶³ – all of which are hardly apt descriptions of the present appearance of the planet.

¹²⁵¹V. Clube and B. Napier, *The Cosmic Serpent* (London, 1982), pp. 163-164.

¹²⁵²E. Cochrane, "Inanna, the Venus-Star," *Chronology and Catastrophism Review* XV (1993), p. 42.

¹²⁵³W. Heimpel, "A Catalog of Near Eastern Venus Deities," *Syro-Mesopotamian Studies* 4:3 (1982), p. 12.

¹²⁵⁴A. Falkenstein, *Sumerische und Akkadische Hymnen und Gebete* (Zurich, 1953), pp. 86, 90-99, 133.

¹²⁵⁵W. W. Hallo and A. J. A. van Dijk, *The Exaltation of Inanna* (New Haven, 1968), p. 29.

¹²⁵⁶J. G. Taylor, "Was Yahweh Worshipped as the Sun?" *Biblical Archaeology Review* (May/June 1994), pp. 52, 59.

¹²⁵⁷*Ibid.*, p. 58.

¹²⁵⁸P. J. Huber, "Early Cuneiform Evidence for the Existence of the Planet Venus," in D. Goldsmith (ed.), *Scientists Confront Velikovsky* (Cornell, 1977), p. 117.

¹²⁵⁹L. E. Rose, "'Just Plainly Wrong': A Critique of Peter Huber," *KRONOS* III:2 (Winter 1977), p. 108.

¹²⁶⁰*Ibid.*, p. 109.

¹²⁶¹*Ibid.*

¹²⁶²T. Jacobson, *The Treasures of Darkness* (New Haven, 1976), p. 139.

¹²⁶³D. Wolkstein and S. N. Kramer, *Inanna: Queen of Heaven and Earth* (London, 1984), pp. 93, 101.

The Cometary Dragon

Inanna is also lauded as having had the form of a great serpent,¹²⁶⁴ described as a great fire-breathing dragon.¹²⁶⁵ In Egypt, also, the word for "serpent," rendered *ara*, also meant "goddess." As Robert Temple noted, "we encounter *ara* frequently having the common general meaning of 'goddess',"¹²⁶⁶ all of which is significant in view of the fact that "early conceptions of the comet saw in it a dragon or serpent."¹²⁶⁷

Granted, meteors were also represented as dragons. Thus, according to the Chinese, when dragons fight, fire-balls fall to the ground.¹²⁶⁸ As recently as 1895, one

definition of the word "dragon" was given as "a fiery shooting meteor."¹²⁶⁹ The fall of a fireball in 1600 was represented as a winged serpent, with a luminous head and a glowing train.¹²⁷⁰ The great bolide seen in 1783 was considered to be "of that Species of Meteor which the great Physiologist, Dr. Woodward, and others, call the *Draco volans* or Flying Dragons."¹²⁷¹ As Victor Clube and Bill Napier noted, "the trail of dust left by an impacting fireball, after it has become windblown, takes on just such an appearance [that is, of a serpentine dragon] and is often so described, especially in medieval literature."¹²⁷²

"Dall'Olmo has investigated Latin terminology relating to astronomical phenomena . . . and finds that terms like *serpens* and *draco* are frequently used to describe meteor falls. He remarks that 'the smokey remnants of a big sporadic meteor may take the twisted shape of a snake or of a dragon, due to the currents in the upper atmosphere.'¹²⁷³

In fact, the Anglo-Saxon dragon standard owes its very origin to such a phenomenon as described by Matthew of Westminster:

"The brother of the British king Aurelius beheld a vision—a fiery meteor in the form of a great dragon illumined the heavens with a portentous glare. The astrologers unanimously expounded the omen to signify that the seer would one day sit upon the throne of Britain. Aurelius died, and his brother became

¹²⁶⁴J. V. K. Wilson, *The Rebel Lands* (Cambridge, 1979), p. 17

¹²⁶⁵W. W. Hallo and A. J. A. van Dijk, *op. cit.*, p. 15.

¹²⁶⁶R. K. G. Temple, *The Sirius Mystery* (New York, 1976), p. 175.

¹²⁶⁷M. Leach (ed.), *Funk and Wagnall's Dictionary of Folklore, Mythology, and Legend* (New York, 1949), p. 243.

¹²⁶⁸M. Oldfield Howie, *The Encircled Serpent* (New York, 1955), p. 259.

¹²⁶⁹N. P. Warren, *Journal of the British Astronomical Association* (April 1978), as cited in *SIS Workshop*, #2 (July 1978), p. 18.

¹²⁷⁰*Ibid.*

¹²⁷¹*Ibid.*

¹²⁷²V. Clube and B. Napier, *The Cosmic Serpent* (London, 1982), p. 195.

¹²⁷³*Ibid.*, pp. 195-196.

king. His first royal act was to cause the fabrication of two dragons in gold, like the figure which the meteor assumed. One of these he placed in Winchester Cathedral; the other he reserved to be carried before him in his military expeditions. And hence the custom which the kings of England have ever since observed – that of having the dragon standard borne before them in battle."¹²⁷⁴

The dragon, however, is more often associated with comets, such as the one of 449 A.D., described by Geoffrey of Monmouth:

"There appeared a star of marvelous bigness and brightness, stretching forth one ray whereon was a ball of fire, spreading forth in the likeness of a dragon, and from the mouth of the dragon issued forth two rays, whereof the one was of such length as that it did seem to reach beyond the regions of Gaul, and the other, verging toward the Irish sea, did end in seven lesser rays."¹²⁷⁵

The comet of A.D. 999 is similarly described in an entry from the monastic chronicles of Helinandus, referred to by dall'Olmo:

"A comet appeared on 13 December at about 3 p.m. splitting the sky *as if it were a fiery torch* . . . While the split in the sky was gradually vanishing, its shape became the figure of the head of a snake with deep-blue feet."¹²⁷⁶

We notice here that not only was this comet described as a serpent but also as "a fiery torch," the very same term employed of Inanna/Venus.

Other comets which took on the appearance of a dragon are described by Comyns Beaumont.¹²⁷⁷ Some, assuming the shape of a fiery serpent, breathing fire, were seen lashing their tail wildly as if in agony.

Winecke's Comet and Donati's Comet, both of which appeared in 1858, were also said to look like fiery serpents. Swift's Comet and Holmes' Comet, both of which appeared in 1892, were likewise said to look like flying serpents. Even Biela's famous comet, the one that eventually split in two and later disintegrated altogether, was described as looking like a great red serpent.¹²⁷⁸

¹²⁷⁴M. Oldfield Howie, *op. cit.*, p. 101.

¹²⁷⁵Geoffrey of Monmouth, *The History of the Kings of Britain* (New York, 1958), p. 169.

¹²⁷⁶V. Clube and B. Napier, *op. cit.*, p. 196. (Emphasis added) (NOTE: Because this "comet" was seen to fall to the ground, it may have been a bolide rather than a comet. The falling to the ground, on the other hand, could refer to the visual disappearance of the comet beyond the horizon.)

¹²⁷⁷C. Beaumont, *The Mysterious Comet* (London, 1925), pp. 82-83.

¹²⁷⁸G. Campbell, "The Great Flying Serpent," *Vancouver Sun* (November 20, 1973).

Thus, when Inanna/Venus is described as a dragon, the implication is that it once appeared in the form of a comet, as her very symbol more than aptly indicates. That this is not a wild conjecture on our part will be made evident below when we come to investigate similar beliefs among the natives of Mesoamerica.

The Bearded Ishtar

That the goddess Ishtar was one of the Assyro-Babylonian personifications of the planet Venus is well-known.¹²⁷⁹ Thus, in the Babylonian psalms, Ishtar is not only described as Journeying "on high," but also as "goddess of the morning," in reference to Venus as Morning Star.¹²⁸⁰

In the Omen Texts and in hymnal inscriptions, however, Ishtar is lauded as having had a beard,¹²⁸¹ concerning which Schaumberger had so much to say.¹²⁸² That this refers to the actual planet is evidenced by an astrological text which describes Ishtar as rising, or ascending, while "decorated with a beard."¹²⁸³ Also: "[Ishtar] is bearded with a beard, in a sheen of light she is clothed."¹²⁸⁴ Besides which, the beard is not only said to have belonged to the goddess Ishtar but, in other sources, directly to have belonged to the planet Dilbat, which was Venus, and identified with Ishtar, concerning which Jastrow specifically states that this beard clearly refers to "some phenomenon connected with the appearance of the planet" and not to any masculine character of the goddess Ishtar.¹²⁸⁵

It was this that led Velikovsky to state that, among the Assyro-Babylonians, or Chaldeans, as he termed them, the planet Venus "was said to have a beard."¹²⁸⁶ Morris Jastrow believed that this "beard" referred to the image of a glittering Venus,¹²⁸⁷ concerning which, and similar explanations, Bob Forrest made so much in his criticism of Velikovsky.¹²⁸⁸ But if one needed a simile for the brightness of a planet, would one think of a beard, especially when the planet in question was visualized as a *female* deity?

That the "beard" of Ishtar denoted something more than just scintillating brightness is exemplified by her additional descriptions as a "bright torch of heaven,"¹²⁸⁹ a "magnificent spectacle in the middle of the sky," the "Dilbat-Star standing in bright daylight in the brilliant sky," and a "diamond that shines like the sun."¹²⁹⁰ That Schaumberger, like other mythologists, explained these descriptions as having been purely poetic¹²⁹¹ is, of course, understandable since there is nothing about the present

¹²⁷⁹W. Heimpel, *loc. cit.*

¹²⁸⁰S. Langdon, *Sumerian and Babylonian Psalms* (1909), pp. 188, 194.

¹²⁸¹M. Jastrow Jr., *Die Religion Babyloniens und Assyriens* II/2 (1912), pp. 633-637.

¹²⁸²J. Schaumberger, "Der Bart der Venus," in F. X. Kugler, *Sternkunde und Sterndienst in Babel* (3rd supp., 1935), p. 303.

¹²⁸³M. Jastrow, Jr., *loc. cit.*

¹²⁸⁴S. Langdon, *Tammuz and Ishtar* (1914), p. 181.

¹²⁸⁵M. Jastrow, Jr., "The Bearded Venus," *Revue Archéologique* XVII (1911), pp. 271-298.

¹²⁸⁶I. Velikovsky, *op. cit.*, p. 165.

¹²⁸⁷M. Jastrow Jr., *Die Religion Babyloniens und Assyriens* II/2 (1912), pp. 633-637.

¹²⁸⁸B. Forrest, *op. cit.*, Vol. 3 (Manchester, 1982), pp. 230-231, 235; Vol. 4 (same date), p. 263; Vol. 7 (Manchester, 1983), pp. 528-529; *idem*, *A Guide to Velikovsky's Sources* (Santa Barbara, 1987), p. 23; *idem*, "Venus and Velikovsky: The Original Sources," *The Skeptical Inquirer*, Vol. 8 (Winter 1983-84), p. 156.

¹²⁸⁹"A Prayer of the Raising of the Hand to Ishtar," in L. W. King, *Seven Tablets of Creation* (1902).

¹²⁹⁰J. Schaumberger, *op. cit.*, p. 291.

¹²⁹¹*Ibid.*; see, similarly, B. Forrest, *A Guide to Velikovsky's Sources* (Santa Barbara, 1987), pp. 23-24.

appearance of Venus that could even remotely be said to justify them. But as Ilse Fuhr was later to point out: "It would not have been difficult to apply this curious epithet to a comet [since] '*decorated with a beard*' is a characterization still commonly applied to tail-stars."¹²⁹² Thus, even among the Greeks, a frequent name for "comet" was *pogonias*, "bearded star."¹²⁹³ What is of additional interest here is that, in Egypt, the hieroglyph for "beard" is an inverted representation of the Inanna symbol (see Figure 1), the cometary nature of which we have already discussed above.¹²⁹⁴

The Biblical Sources—A Better Choice

Velikovsky's Biblical evidence for the past cometary nature of Venus, although not decisive on its own, fares better under scrutiny than does his use of *extra*-Biblical sources, as seen above. Here Velikovsky stated that: "The (Greek) translation of the Seventy (Septuagint) reads: 'Canst thou bring forth Mazzaroth in his season and guide the Evening Star by his long hair?'"¹²⁹⁵ The quote is from the Book of Job 38:32. To this Forrest objected because the King James version of the Old Testament has Arcturus instead of Evening Star. "Clearly," he wrote, "someone has seen fit to remove Arcturus from the proceedings, and to replace him by the Evening Star . . ." ¹²⁹⁶ But the King James version, first published in A.D. 1611, cannot be given precedence over the much older Septuagint which dates from the 2nd century B.C. If anything, it is the *older* Evening Star that was supplanted by Arcturus and not, as Forrest would have it, the other way around.

In Hebrew, the celestial object in question is rendered *Ayish*, sometimes as *Ash*, which object, together with others of similar nature, has been *contradictorily* identified, by Jews and gentiles alike, as this or that constellation,¹²⁹⁷ thus clearly indicating that the identifications are nothing but wild guesses. What should be kept in mind, however, is that the Septuagint is the oldest known version of the Old Testament since the Hebrew text from which it was translated is no longer extant. The oldest *Hebrew* version that we now possess, the Masoretic text, dates from the 6th to 8th century A.D. The Septuagint rendering of *Ayish* as Hesperos, that is, Evening Star, should therefore be given preference over all later identifications. To this may be added the fact that, in Maltese, *ghaxija*, pronounced *ashiya* and derived from the same Semitic root as *Ash* and *Ayish*, means "evening."¹²⁹⁸

Forrest also objected because in an *English* translation of the Septuagint that he consulted, published in London in 1879, the last part of the quote supplied by Velikovsky reads "the Evening Star *with his rays*" rather than "the Evening Star *by his long hair*."¹²⁹⁹ But *other* English versions of the Septuagint *do* translate the line in question as "drag out Hesperos (the Evening Star) by his hair."¹³⁰⁰ Besides, it is the original *Greek* version that Forrest should have consulted had he wished to challenge the

¹²⁹²I. Fuhr, "On Comets, Comet-Like Luminous Apparitions and Meteors," *KRONOS* VII:4 (Summer 1982), p. 43. (Emphasis added)

¹²⁹³Aristotle, *Meteorologica* I:7.

¹²⁹⁴See, for instance, E. A. W. Budge, *An Egyptian Hieroglyphic Dictionary*, Vol. I (New York, 1920/1978), p. cv.

¹²⁹⁵I. Velikovsky, *op. cit.*, p. 202.

¹²⁹⁶B. Forrest, *Velikovsky's Sources*, Vol. 7 (Manchester, 1983), p. 533.

¹²⁹⁷See, for instance, D. Cardona, "The Mystery of the Pleiades," *KRONOS* III:4 (Summer 1978), pp. 24 ff.

¹²⁹⁸E. D. Busuttill, *Kalepin Tliet IIsna* (Valletta, 1978), p. 33.

¹²⁹⁹B. Forrest, *loc. cit.*

¹³⁰⁰See, for instance, L. A. Burses (ed.), *The Septuagint Bible*, as translated by Charles Thomson (Indian Hills, Colo., 1954), p. 867.

validity of Velikovsky's claim and not an English version in which the mention of hair in relation to the Evening Star was obviously found to be ambiguous by the translator.

As for *Mazzaroth*, like *Ayish*, this word has also been variously rendered in different translation of the Old Testament and, as Velikovsky himself confined, the conclusion that scholars reached was that the meaning of the name remains uncertain.¹³⁰¹ But in the Vulgate, which dates from the 4th century A.D., considered the second oldest version of the Old Testament of any authority, *Mazzaroth* is rendered *Lucifer*, which is one of the Latin names for the planet Venus.

On the other hand, according to some authorities, *Mazzaroth* means "comet" and, therefore, couldn't have been Lucifer/Venus.¹³⁰² But, in view of the context in which *Mazzaroth* is found together with the hairy *Ayish*, as well as the Vulgate's translation of it as *Lucifer*, I am inclined to agree with Velikovsky in that *Mazzaroth* stands for both Venus and a hairy star,¹³⁰³ in other words a hairy, or cometary, Venus.

Sekhmet and the Circling Star

When it came to the Egyptian material, Velikovsky unfortunately fell prey to his own carelessness. "The Egyptians under Seti," he wrote, "Thus described Venus (Sekhmet): 'A circling star which scatters its flame in fire . . . a flame of fire in her tempest.'¹³⁰⁴ This quote comes from James Breasted,¹³⁰⁵ concerning which Bob Forrest was later to write that "the Venus-Sekhmet equation seems to be pure assumption on V[elikovsky]'s part" and that as far as he could see "from Breasted and a few other sources, no text specifically links the goddess Sekhmet with the planet Venus."¹³⁰⁶ Forrest stresses the claims of Max Müller¹³⁰⁷ that Sekhmet is generally shown wearing the solar disc upon her head and must, therefore, be identified as the "warlike manifestation of the sun," that is "the destructive solar force."¹³⁰⁸ But that Sekhmet was solar in origin, and/or that the disc she is shown wearing represented the Sun, which is the opinion of most mythologists,¹³⁰⁹ is not borne out by Egyptian texts.

Forrest, of course, was correct in that the identification of Sekhmet as Venus *was* an assumption on Velikovsky's part, but it was one that is lent support by comparative mythology. Thus in a temple inscription we find it stated that "Hathor overcometh the enemy of her sire by this her name of Sekhet [the same as Sekhmet],"¹³¹⁰ thus identifying the goddess as Hathor,¹³¹¹ among whose many other names

¹³⁰¹I. Velikovsky, *loc. cit.*, where other authorities are cited.

¹³⁰²J. S. Suschken, as cited by I. Velikovsky, *loc. cit.*

¹³⁰³I. Velikovsky, *loc. cit.*

¹³⁰⁴*Ibid.*, p. 165.

¹³⁰⁵J. H. Breasted, *Ancient Records of Egypt*, Vol. III (Chicago, 1906), p. 264.

¹³⁰⁶B. Forrest, *op. cit.* Vol. I (Manchester, 1981), p. 17.

¹³⁰⁷*Ibid.*, Vol. 3 (Manchester, 1982), p. 189.

¹³⁰⁸W. Max Müller, "Egyptian Mythology," *The Mythology of All Races*, Vol. 12 (1964), pp. 29, 87, 146-147.

¹³⁰⁹See, for instance, D. A. Mackenzie, *Egyptian Myth and Legend* (New York, 1907/1978), p. xxxviii, where he also refers to Wiedemann's similar opinion; E. A. W. Budge, *The Gods of the Egyptians*, Vol. 1 (New York, 1904/1969), p. 515.

¹³¹⁰R. Van Over, *Sun Songs: Creation Myths From Around the World* (New York, 1980), p. 261; see also D. A. Mackenzie, *loc. cit.*

¹³¹¹E. A. W. Budge, *op. cit.*, p. 514.

was Isis,¹³¹² whose identity as Venus was well-known to Pliny.¹³¹³ The Egyptians also identified Hathor, and therefore Sekhmet, with the Near Eastern goddess Anat¹³¹⁴ who, as Astour informs us, was herself identified as the planet Venus.¹³¹⁵ The Greeks, on the other hand, identified Hathor with their own Aphrodite—and Budge stresses the fact that "they were justified in doing so"¹³¹⁶ —whose identity as Venus is well-known.

Velikovsky's error in using the quote from Breasted as evidence of Venus' former cometary nature was not in his identification of Sekhmet as Venus but, as Forrest rightly pointed out,¹³¹⁷ in attributing the "circling star" in question to Sekhmet. The inscription of Seti I at Karnak, which is what we are really dealing with here, is a self-flattering victory hymn dedicated to Amon. Velikovsky was led astray because, toward the end of the hymn, Amon's majesty is compared to "the very being of Sekhmet, in her tempest." *But it is Amon's majesty, and not Sekhmet's, who is initially likened to "a circling star, which scatters its flame in fire,"*¹³¹⁸ and no one, not even Velikovsky, has ever identified Amon as a personification of the planet Venus.

The Cometary Eye

Even so, had Velikovsky dug a little deeper into Egyptian mythology he might have come across a different line of comparative reasoning to show the association, if not the actual identification, of the Venerian deity as a comet. Central to this issue, of course, is the fact that both Hathor and Sekhmet were considered to be the eye of Ra, as amply demonstrated in the myth known as "The Destruction of Mankind."¹³¹⁹ Now, if I may be permitted to cross traditional boundaries, it is interesting to note that, in Sanskrit, the word *netram*, which bears a striking similarity to the Egyptian word *neter*, which means both "god" and "star,"¹³²⁰ likewise means "star," but also "eye."¹³²¹ In like manner, *tara* again means "star" or "planet," but, again, also "pupil of the eye" or eye-ball.¹³²²

That the Great Mother of archaic religion was venerated as an Eye Goddess throughout the ancient Near East and northern Europe is well-known.¹³²³ And, as Crawford has shown, it was the Sumerian Inanna, as well as the Akkadian Ishtar, both identifiable as Venus, who served as the prototype of this goddess.¹³²⁴

In Mesoamerica, the celestial eye was directly associated with the planet Venus. Thus, in Maya codices, the planet was often associated with the "death eye."¹³²⁵ In fact, one of the most common Mayan

¹³¹²*Ibid.*, p. 431.

¹³¹³Pliny, *Historiae Naturalis* II:37.

¹³¹⁴A. Kapelrud, *The Violent Goddess Anat in the Ras Shamra Texts* (Oslo, 1969), p. 15.

¹³¹⁵M. Astour, *Hellenosemitica* (Leiden, 1967), p. 261.

¹³¹⁶E. A. W. Budge, *op. cit.*, p. 435.

¹³¹⁷B. Forrest, *op. cit.*, Vol. 1 (Manchester, 1981), p. 17.

¹³¹⁸In this respect, see also, I. Fuhr, *op. cit.*, p. 55.

¹³¹⁹E. A. W. Budge, *op. cit.*, pp. 392 ff.

¹³²⁰*Idem*, *An Egyptian Hieroglyphic Dictionary*, Vol. I (New York, 1920/1978), p. 409.

¹³²¹V. S. Apte, *The Practical Sanskrit-English Dictionary* (Delhi, 1965), pp. 570-571.

¹³²²*Ibid.*, p. 473.

¹³²³O. G. S. Crawford, *The Eye Goddess* (New York, 1956), in toto.

¹³²⁴*Ibid.*

¹³²⁵J. E. S. Thompson, *Maya Hieroglyphic Writing* (Norman, 1971), p. 172.

names for Venus was *Nohoch ich*, which means "Great eye."¹³²⁶ Not only in their texts, but also in their symbolic murals did the Maya associate Venus with the eye.¹³²⁷ Thus, as John Carlson informed his readers, "the five pointed star with an 'eye' was the central Mexican symbol for the death-dealing planet"¹³²⁸ — (see Fig. 4).

In the Hervey islands of the Pacific, Venus was known as Tamatanui, which translates as the "eye of Tane."¹³²⁹ Even in Australia, among the Ringa-Ringaroo, the planet Venus is called Mimungoona, that is "Big Eye."¹³³⁰

The association of the celestial eye with comets is implied in a passage from the Egyptian *Coffin Texts* which reads: "I raised up the hair from the Sacred Eye at the time of its wrath,"¹³³¹ concerning which Eye it was said that "it's flame is to the sky."¹³³² As Ev Cochrane and David Talbott explained: "The time of the Eye's wrath refers to the time when, as Hathor, the Eye burned across the heavens . . . intent on destroying mankind [as narrated in the "The Destruction of Mankind," mentioned above]."¹³³³

Other passages from Egyptian texts describe how the Eye Goddess turns into the Great Flame, the tip of which Flame "crosses the land from the sky." It is written that: "No one at all can approach her, the streams behind her are flames of fire."¹³³⁴ So, also, elsewhere, where the celestial eye is described as a "flame (moving) before the wind of the sky."¹³³⁵

That this is not idle speculation is more than implied from the fact that the Egyptian word *uauti*, which means "comet,"¹³³⁶ is derived from the same root as *uat*, which was one of the names of the eye of Ra.¹³³⁷ And, to clinch the matter, we find that *Uauti*, that is "Comet," was one of the epithets of Hathor¹³³⁸ whom we have already seen identified as Venus.

Here it is interesting to note what Victor Clube and Bill Napier wrote concerning the "hairy one(s)" of Egypt:

"Although the word comet is derived from the Greek *Kometes* ('hairy one'), it was almost certainly the Egyptians who first used the description 'a hairy star' The Egyptians are reported alluding to the analogy of long female tresses in connection with the appearance of a spectacular comet."¹³³⁹

¹³²⁶*Ibid.*, p. 218.

¹³²⁷J. B. Carlson, "America's Ancient Skywatchers," *National Geographic* (March 1990), p. 101.

¹³²⁸*Ibid.*, p. 98.

¹³²⁹R. W. Williamson, *Religious and Cosmic Beliefs of Central Polynesia*, Vol. I (New York, 1977), p. 132.

¹³³⁰E. M. Curr, *The Australian Race*, Vol. II (1886).

¹³³¹R. Faulkner, *The Ancient Egyptian Coffin Texts* (Warminster, 1973), p. 260.

¹³³²Coffin Texts, spell 249.

¹³³³E. Cochrane and D. Talbott, "When Venus was a Comet," *KRONOS* XII:1 (Winter 1987), p. 15.

¹³³⁴Coffin Texts, spell 336.

¹³³⁵Pyramid Texts, utterance 324; see also, E. A. W. Budge, *On the Hieratic Papyrus of Nes-Amsu* (London, 1891), pp. 17, 27.

¹³³⁶E. A. W. Budge, *An Egyptian Hieroglyphic Dictionary*, Vol. 1 (New York, 1920/1978), p. 155.

¹³³⁷*Ibid.*, p. 154.

¹³³⁸*Ibid.*

¹³³⁹V. Clube and B. Napier, *op. cit.*, (London, 1982), p. 162.

It therefore comes as no surprise that one of the epithets for Isis, long recognized as a personification of Venus, was "*hensekti*," which means "hairy one."¹³⁴⁰

The Indic Venus—A Minor Datum

Velikovsky also stated that the *Vedas* describe Venus as a "fire with smoke."¹³⁴¹ To this, once again, Forrest objected¹³⁴² because the source that Velikovsky cited¹³⁴³ refers to the *Mahabharata* rather than the *Vedas* and because the context in which the reference to Venus is found "is not a story of Venusian catastrophes," but a list of prodigies and portents associated with the approaching battle between the Kauravas and the Pandavas. But this does not detract from the fact that Venus is there described to have looked "like a fire accompanied by smoke."

Granted that, on its own, this entry would not be enough to convince anyone that Venus once appeared in cometary form, the datum, minor as it is, should be evaluated in conjunction with the evidence so far presented together with that still to be presented below.

The Ching Hsing and Tai Po

The Chinese knew of a celestial phenomenon which seems to have appeared at rare intervals and which they referred to as the *Ching Hsing*, the "Orb [or Star] of Splendor," and *Te-Hsing*, the "Orb [or Star] of Virtue." This *Ching Hsing* is sometimes explained as earthshine, the sometime illumination of the darkened lunar orb at new moon by reflected earthlight. But the sinologist Edward Schaeffer doubted whether the word *ching*, which means "bright," would have been used by the Chinese to define this rarely visible phenomenon.¹³⁴⁴

The Chinese themselves seem to have had no doubts as to what the *Ching Hsing* represented. Thus *Ssu Ma Chien* writes that, in 110 B.C., a comet appeared and emphasizes that astronomers identified this comet as a *Ching Hsing*.¹³⁴⁵

Much later, on a night in the year 1340 A.D., a senior astronomer reported having seen this *Ching Hsing*. A watch was set up to verify the observation. "And indeed," reads a memorandum of the Astronomical Bureau, "only nine days later the planet Venus 'crossed the meridian.'"¹³⁴⁶

Although, as Charles Raspil noted,¹³⁴⁷ the *Ching Hsing* is not specifically identified with Venus, the implication is clear, since no other celestial object is mentioned in the memorandum in question. And

¹³⁴⁰E. A. W. Budge, *op. cit.*, p. 491.

¹³⁴¹I. Velikovsky, *op. cit.*, p. 164.

¹³⁴²B. Forrest, *A Guide to Velikovsky's Sources* (Santa Barbara, 1987), pp. 22-23; *idem*, "Venus and Velikovsky: The Original Sources," *The Skeptical Inquirer*, Vol. 8 (Winter 1983-84), p. 155.

¹³⁴³J. Scheftelowitz, *Die Zeit als Schicksalsgottheit in der Iranischen Religion* (1929), p. 4.

¹³⁴⁴E. Schaeffer, *Pacing the Void*, as cited by C. Raspil, "The Ching Hsing," *HORUS* 1:2 (Summer 1985), p. 25.

¹³⁴⁵*Ibid.*

¹³⁴⁶J. Needham, *History of the Civilization of China*, as cited in *ibid.*, p. 24.

¹³⁴⁷*Ibid.*

if the *Ching Hsing* was both a comet as well as being associated with Venus, the reference could very well allude to the ancient belief that Venus had once appeared in cometary form.

The above is lent additional support from the fact that *chhang-keng*, translatable as "long path," is another Chinese word meaning "comet," which name was, however, also applied to the planet Venus.¹³⁴⁸ Likewise, comets were also known as candle stars which were said to consist of a star above which brooms could be seen pointing upward, strangely said to resemble Venus. And, in fact, the term "Broom Star," in Chinese rendered *sao-hsing*, was the most common Chinese name for "comet."¹³⁴⁹ A bundle of straw, which is what primitive brooms consisted of, was also an old European cometary symbol.¹³⁵⁰ And, to clinch the matter even further, *po*, another common term for comet among the Chinese, was likewise applied to the planet Venus which, among other things, was called *Tai-po* that is the "Great Comet."¹³⁵¹

The Soochow astronomical text of the Chinese refers to a time when "Venus was visible in full daylight and, while moving across the sky, rivaled the sun in brightness."¹³⁵²

The Bearded Aphrodite

One of the Greek names for the planet Venus is Hesperus. Here, as Jean Paul had it stated, it is interesting to note that "as often happens in the sky to Hesperus, he can be taken for a hair, beard and tail star,"¹³⁵³ all of which appellations are usually reserved for the description of comets.

This also explains why the Cypriot Venus and/or Aphrodite was sometimes portrayed as sprouting a beard, as shown, for instance, on a coin from Mallus Cilicia,¹³⁵⁴ and as also portrayed in Rome,¹³⁵⁵ which fact gave rise to the appellation *Venus Barbata*, that is the bearded Venus.¹³⁵⁶ Is it not otherwise strange that this goddess, which was for the Greeks the epitome of beauty, should have been depicted as bearded like a man? As we have seen, however, Aphrodite was not the only Venerian deity to have been so depicted by the ancients and, in this respect, as in many others, she was merely the Greek version of the Assyro-Babylonian Ishtar.

Aphrodite also bore the epithet *Comaetho*, which translates as "fiery haired,"¹³⁵⁷ an appropriate designation for a cometary body. As has been noticed by Walker: "A comet was supposed to be a tendril of the Great Mother's hair appearing in the sky . . ." ¹³⁵⁸

¹³⁴⁸H. Yoke, "Ancient and Medieval Observations of Comets and Novae in Chinese Sources," *Vistas in Astronomy* 5 (1962), pp. 137, 164.

¹³⁴⁹*Ibid.*

¹³⁵⁰J. Grimm, *Teutonic Mythology*, Vol. II (Gloucester, 1976), p. 722.

¹³⁵¹A. Schuessler, as cited by E. Cochrane, "On Comets and Kings," *AEON* II:1 (June 1989), p. 60.

¹³⁵²W. C. Rufus and Hsing-chih Tien, *The Soochow Astronomical Chart* (1945), as cited by I. Velikovsky, *op. cit.*, p. 165.

¹³⁵³J. Paul, *Hesperus*, as quoted by Z. Rix, "Notes on the Androgynous Comet," *SIS Review* I:5 (Summer 1977), p. 17.

¹³⁵⁴F. Lajard, *Recherches sur Vénus*, as cited in *ibid.*, p. 18.

¹³⁵⁵Aurelius Augustinus, *Civitate Dei* IV:11.

¹³⁵⁶Macrobius, *Saturnalia* III:8:10-11; see also, W. A. Smith, *Dictionary of Greek and Roman Biography and Mythology* (London, 1890), p. 1240.

¹³⁵⁷H. Lidell and R. Scott, *A Greek-English Lexicon* (New York, 1872), p. 782; see also, Apollodorus, *Bibliotheka* II:iv:7.

Iubar

Pliny wrote that: "Sometimes there are hairs attached to the planets,"¹³⁵⁹ to which Velikovsky appended the words: "an old description of Venus must have served as a basis for his assertion."¹³⁶⁰ According to Forrest, "clearly this is nonsense."¹³⁶¹ Pliny, Forrest reminds us, referred to the planets in the plural as also "the other stars," which Velikovsky chose to omit when discussing these hairs. Thus Forrest is of the opinion that "Pliny is almost certainly here referring to atmospheric distortions, and not to any former comet[ary] tail of Venus."¹³⁶² In this respect, it might well be that Forrest is correct although, to be sure, the mytho-historical record alludes to the cometary "hairs" of more than just the planet Venus – but that is a topic outside the present study.

It is not that easy, however, to discard the strange cometary lore of the Italian Pythagoreans, respecting which Aristotle wrote: "Some of the Italians called Pythagoreans say that the comet is one of the planets, but that it appears at great intervals of time and only rises a little above the horizon."¹³⁶³ Aristotle himself found fault with this lore but, as Velikovsky pointed out,¹³⁶⁴ only because, not knowing what the Pythagoreans were alluding to, he misunderstood them to have meant that all comets were one and the same planet.¹³⁶⁵ Whether the Pythagoreans were really referring to Venus or not we shall never know for certain. But, in view of what we have already disclosed, and will continue to disclose below, a comet that is described as a planet that "only rises a little above the horizon" does seem to fit the bill.

Long before the Romans bestowed the name Lucifer on Venus, the planet, especially in its "morning" aspect, was known as Iubar.¹³⁶⁶ As Jan Sammer pointed out, "Latin writers derived *Iubar* from the word *iuba*, meaning 'hair.'"¹³⁶⁷ Thus Varro could write that "this star is called *iubar* because it is hairy,"¹³⁶⁸ comparing the star's hair to a lion's mane,¹³⁶⁹ as also did Festus.¹³⁷⁰ And, in fact, *stellaiuba*, that is "maned, or hairy, star," was one of the Latin names for "comet."¹³⁷¹ More than that, Seneca actually employed the word *iubar* itself to describe a comet,¹³⁷² as so, also, did Pliny.¹³⁷³ It can thus be said that, in early Rome, the word and/or name *iubar* stood for both "comet" and the planet Venus.

¹³⁵⁸B. Walker, *The Woman's Encyclopedia of Myths and Secrets* (New York, 1983), pp. 368.

¹³⁵⁹Pliny, *op. cit.*, II:23.

¹³⁶⁰I. Velikovsky, *op. cit.*, p. 165.

¹³⁶¹B. Forrest, *Velikovsky's Sources*, Vol. I (Manchester, 1981), p. 65.

¹³⁶²*Ibid.*

¹³⁶³Aristotle, *Meteorologica*, I:6.

¹³⁶⁴I. Velikovsky, *op. cit.*, pp. 162-163.

¹³⁶⁵Aristotle, *loc. cit.*

¹³⁶⁶Varro, *De Lingua Latina* VI:6.

¹³⁶⁷J. N. Sammer, "An Ancient Name for Venus," *KRONOS* VI:2 (Winter 1981), p. 61.

¹³⁶⁸Varro, *loc. cit.*

¹³⁶⁹*Ibid.*, VII:76.

¹³⁷⁰Sextus Festus, *On the Meaning of Words* 92:13.

¹³⁷¹R. Onians, *The Origins of European Thought* (Cambridge, 1954), pp. 164-166.

¹³⁷²Seneca, *Octavia* 231.

¹³⁷³Pliny, *op. cit.*, II:91.

I should also point out in passing that the comparison of this star's hair to a lion's mane brings to mind the various representations of the Egyptian Sekhmet (see Fig. 2) who is usually portrayed as having had a lion's head. Max Müller, of course, was of the opinion that the feline-headed goddesses themselves "contributed toward associating them with the luminary of day, because the sun-god often had a leaning toward a lion's form."¹³⁷⁴ This, however, is belied by the well-known association of the lion with the planet Venus which Franz Kugler traced through the literary and artistic traditions of the ancient Near East.¹³⁷⁵

Colbon

The Yakuts of Siberia were of the belief that *Colbon*, whom they identified as Venus, was "the daughter of the Devil and to have had a tail in the early days."¹³⁷⁶

Granted, as Bob Kobres pointed out, Ksenofontov was of the opinion that, originally, *colbon* referred "to stars that change their place in the sky, sometimes appearing and disappearing," and that it is only "nowadays" that the name is applied to Venus.¹³⁷⁷ Stars, however, are not known to "change their place in the sky." A star with a tail, on the other hand, can only have been a comet, as Kobres himself accepts.

What is also telling here is that this comet was described "to have had a tail" *only* "in the early days," the implication being that the comet no longer possesses such a tail.¹⁴⁸

The identification of *Colbon* as Venus, whether arrived at early or late, meanwhile, is in keeping with the ancient belief that Venus, also, once possessed a cometary tail.

The Smoking Star

The Maya described Venus as having been "very ugly" and adorned "with a heavy beard,"¹³⁷⁸ which immediately brings to mind the bearded Ishtar and Aphrodite of Babylonian and Greek lore. More than that, the indigenes of pre-Columbian Mexico were of the opinion that the planet Venus smoked. "The star that smoked, *la estrella humeava*," wrote Alexander von Humboldt, "was Sitlae choloha, which the Spaniards call Venus."¹³⁷⁹

This led the same Humboldt to inquire: "Now, I ask, what optical illusion could give Venus the appearance of a star throwing out smoke?"¹³⁸⁰ His explanation was that Venus might have been confused

¹³⁷⁴W. Max Müller, *op. cit.*, p. 29.

¹³⁷⁵F. X. Kugler, *Sibyllinischer Sternkampf und Phaethon in Naturgeschichtlicher Beleuchtung* (1927).

¹³⁷⁶V. L. Serosevsky, as cited by B. Kobres, "Comets and the Bronze Age Collapse," *Chronology & Catastrophism Workshop* (1992) 1, p. 6.

¹³⁷⁷G. V. Ksenofontov, in V. Dioszegi, *Popular Beliefs and Folklore Tradition in Siberia* (Bloomington, 1968), as cited by B. Kobres in *ibid.*

¹³⁷⁸J. E. S. Thompson, *Maya Hieroglyphic Writing* (Norman, 1971), p. 218.

¹³⁷⁹A. von Humboldt, *Researches Concerning the Institutions and Monuments of the Ancient Inhabitants of America*, Vol. II (London, 1814), p. 174.

¹³⁸⁰*Ibid.*

with the fiery crater of the volcano Orizava which, during the night, resembles a rising star,¹³⁸¹ and concerning which, as usual, Forrest made so much.¹³⁸²

But, as Bernardino de Sahagun informed his readers—and Forrest, who has often accused Velikovsky of selectivity, should not have omitted this material in his various criticisms—"a star that smoked" was what the Mexicans called a comet.¹³⁸³ More recently, David Kelley reinforced this identification of a "smoking star" as "a known Yucatec term for comets."¹³⁸⁴ As Anthony Aveni noted: "Comets *citlalimpopoca*, or the stars that smoked) are represented frequently by the surviving historical documents, usually by a stellar image on a blue background with emanating streams of smoke . . ." ¹³⁸⁵ And, in fact, the Mesoamericans actually described comets as celestial cigars.¹³⁸⁶

Additionally, as Weldon Lamb noted, the Yucatec Maya term "smoke star" can be defined as "maned comet."¹³⁸⁷ And this has additional meaning because, as Brasseur de Bourbourg informed his readers, Venus was also referred to as Tzontemocque, which means "the mane,"¹³⁸⁸ this being equivalent to the Greek *kometes*, or "long haired" star.

Puzzled by the reports pertaining to Venus as a smoking star, Aveni was forced to speculate that, just possibly, the belief could have arisen because a comet could have once appeared in close proximity to the planet Venus.¹³⁸⁹ But would the Maya, whose accuracy in astronomical observation has been lauded by all those who have studied their lore, have mistaken such a comet for the planet Venus? Or, if such a comet had once appeared in close visual proximity to that planet, would they not have recorded the event as such? Why would they have transferred the characteristics of such a comet to the planet Venus? Is it not more probable that Venus was referred to as a star that smoked simply because the ancestors of the Mexicans remembered Venus as having formerly appeared in cometary form? And are these illogical questions to ask in view of the fact that such a belief, as we have seen, is not restricted to Mesoamerica? Thus, a passage in the *Codex Telleriano-Remensis* includes an illustration of a comet-like object the caption to which reads: "the star Venus is smoking."¹³⁹⁰

The cometary nature of Venus as believed in by the Mesoamericans is also illustrated by a common Maya hieroglyph known as the *Caban*-curl (see Fig. 4), which depicts a tassel or lock of hair, and which is repeatedly found attached to Venus symbols, including the very name of Venus itself.¹³⁹¹

¹³⁸¹*Ibid.*

¹³⁸²B. Forrest, *op. cit.*, Vol. 6 (Manchester, 1983), pp. 494-496; *idem*, *A Guide to Velikovsky's Sources* (Santa Barbara, 1987), p. 22; *idem*, "Venus and Velikovsky: The Original Sources," *The Skeptical Inquirer*, Vol. 8 (Winter 1983-84), p. 155.

¹³⁸³B. de Sahagun, *Historia General de las Cosas de Nueva España*, Bk. VII, Chap. 4, as cited by I. Velikovsky *op. cit.*, p. 164.

¹³⁸⁴D. Kelley, *Deciphering the Maya Script* (Austin, 1976), p. 133.

¹³⁸⁵A. F. Aveni, *Skywatchers of Ancient Mexico* (Austin, 1981), p. 27.

¹³⁸⁶W. W. Lamb, "Star Lore in the Yucatec Maya Dictionaries," in A. F. Aveni (ed.), *Archaeoastronomy in Pre-Columbian America* (Austin, 1975), p. 245.

¹³⁸⁷*Ibid.*, p. 237.

¹³⁸⁸B. de Bourbourg, *Sources de l'Histoire Primitif du Mexique*, p. 48.

¹³⁸⁹A. F. Avenue, *Skywatchers in Ancient Mexico* (see ref. #156), *loc. cit.*

¹³⁹⁰*Ibid.*

¹³⁹¹M. Closs, "Venus in the Maya World," in M. Robertson (ed.), *Tercera Mesa Redonda de Palenque* (Monterey, 1978), p. 152 ff.

Xiuhcoatl and Chaska

In the astronomical treatise of the Dresden Codex, a picture of a fiery dragon bears the Lamat hieroglyph on its head, which is rather telling since the Lamat symbol is the well-known glyph signifying the planet Venus.¹³⁹² Similarly, Xiuhcoatl (see Fig. 4), whose name translates as Turquoise Dragon, was, like Inanna/Ishtar, described as a "heavenly torch."¹³⁹³ Like the Egyptian Hathor, Xiuhcoatl was hurled as a fiery weapon by the primeval sun-god against his enemies.¹³⁹⁴ He also appears as the "fire stick" wielded by Huitzilopochtli against the demons of darkness.¹³⁹⁵ Should it therefore surprise us that, as Brundage noted, Xiuhcoatl "can be identified, from the quincunx (the five points that together form the emblem of the morning star) that adorns him, as the planet Venus"?¹³⁹⁶ And, to be sure, among the Tzotzil, Venus is still called *Mukta Ch'on*, that is "the Big Serpent"¹³⁹⁷ and, among the Chichimec, as the "Serpent Cloud."¹³⁹⁸

All of which becomes doubly telling in view of Inanna/Venus, as we have seen above, being also described as a fire-breathing dragon, and in view of the fact that fire-breathing dragons, as we have also seen, were ancient images of cometary phenomena. Thus, for instance, a fiery serpent or dragon-like creature, shown descending from a starry heaven, is what stands for a comet in Aztec manuscripts (see Fig. 4),¹³⁹⁹ and the priest-astronomers referred to the same as the "star serpent."¹⁴⁰⁰ The plumed serpent, "depicted in various forms," represented comets to the natives of Mexico.¹⁴⁰¹ It is therefore evident that when Venus was represented as a dragon, it was being represented in cometary form. More than that, Xiuhcoatl was not only a dragon-comet, but himself spewed forth comets¹⁴⁰² which, as the planet Venus with which he was identified, would find a ready explanation if sizable debris from the cometary tail was seen to detach itself and fly off independently.

In Peru, Venus is still known as Chaska, which means the "wavy-haired,"¹⁴⁰³ or "she with the curly hair,"¹⁴⁰⁴ and the "youth with the long curling locks";¹⁴⁰⁵ also as "the Radiant Star with the Flowing Hair,"¹⁴⁰⁶ and the "morning star, Chaska (The Disheveled One)."¹⁴⁰⁷

¹³⁹²J. E. S. Thompson, *op. cit.*, p. 233.

¹³⁹³B. C. Brundage, *The Fifth Sun* (Austin, 1979), p. 189.

¹³⁹⁴*Ibid.*

¹³⁹⁵M. L. Portilla, *Native Mesoamerican Spirituality* (Mahwah, N.J., 1980), p. 220 ff.

¹³⁹⁶B. C. Brundage, *op. cit.*, p. 33.

¹³⁹⁷I. Sprajc, "The Venus-Rain-Maize Complex," *Journal for the History of Astronomy*, 24 (1993), p. 22.

¹³⁹⁸H. B. Alexander, *Latin American Mythology* (New York, 1964), p. 87.

¹³⁹⁹R. D. Chapman and J. C. Brandt, *The Comet Book* (Boston, 1984), p. 21.

¹⁴⁰⁰C. Burland, *The Aztecs* (London, 1980), p. 102.

¹⁴⁰¹P. L. Brown, *Comets, Meteorites and Men* (New York, 1973), p. 18.

¹⁴⁰²B. C. Brundage, *op. cit.*, p. 189.

¹⁴⁰³E. Nordenskjöld, *The Secret of the Peruvian Quipus* (1925), p. 533 ff.; see also, Anonymous, *De las Costumbres Antiguas do los Naturales del Piru* (1879), as translated and cited by J. Sammer, "The Cosmology of Tawantinsuyu," *KRONOS IX:2* (Winter 1984), p. 25.

¹⁴⁰⁴A. Vollemaere, "Venus—Geboorte en Leven," *Oud-America* 08 (August-October 1981), p. 123.

¹⁴⁰⁵W. H. Prescott, *The World of the Incas* (New York, 1970), p. 78.

¹⁴⁰⁶B. C. Brundage, *Empire of the Inca* (Norman, 1963), p. 162.

¹⁴⁰⁷*Ibid.*, p. 50.

The Most Telling Problem

It has, of course, been pointed out, even by Velikovsky's own adherents, that while Venus' atmosphere might have been distended enough to form a coma, even taking on a spiral and other exotic shapes,¹⁴⁰⁸ the planet is too massive to have been able to "grow" a cometary tail. Thus, for instance, Chris Marx, who wrote:

"A comet will develop a tail because its mass is too small to retain gases and fine dust which, being repelled by solar radiation pressure, form the tail. As the heavy mass of Venus precludes this, it cannot have been a 'comet'; it cannot have wagged a tail."¹⁴⁰⁹

Ragnar Forshufvud is of a similar opinion:

"The mass of an ordinary comet is small. No one knows exactly how small, but even very bright comets have been shown to have masses smaller than 10^{-6} Earth masses. Consequently, the escape velocity at the surface of the nucleus is also small. As the comet approaches the Sun, material on its surface is vaporized, and the vaporized material immediately leaves the nucleus and is swept away by radiation pressure and the solar wind . . .

"With a large body like Venus, things are different. The escape velocity at its surface is about 10 km/s. Enormous amounts of energy are required to accelerate material to this velocity."¹⁴¹⁰

Needless to say, Velikovsky himself was not unaware of this problem, as is evident from an interview he had with Joseph Goodavage in 1970. Goodavage had asked: "wouldn't [Venus] have been too massive to be a true comet?"¹⁴¹¹ And Velikovsky replied:

"But Professor Bobrov[n]ikov, Director of [the] Perkins Observatory in Ohio wrote in the book *Astrophysics*, edited by Hynek, that several comets seen during the last century were originally a single comet, extraordinarily large—equal to a mass of the Moon. The opposition to the view that no comet could be as large as a planet is consequently undermined. It couldn't be too farfetched because the Moon is only one order of magnitude smaller than Mercury. Now along comes Whipple, a Harvard astronomer, who claims that Pluto was originally a comet. Pluto is not large, as planets go, but nevertheless it *is* a planet!

¹⁴⁰⁸C. Marx, "Did Venus as a Protoplanet Ever Look Like a 'Comet'?" *SIS Workshop* 3:4 (April 1981), pp. 4-5.

¹⁴⁰⁹*Ibid.*

¹⁴¹⁰R. Forshufvud, "On the Circularization of the Orbit of Venus," *KRONOS* VII:2 (Winter 1982), p. 10.

¹⁴¹¹J. F. Goodavage, "Immanuel Velikovsky: Genius vs. the Scientific Mafia," *SAGA* (September 1970), p. 93.

Whipple claims that Pluto was originally a comet because it moves along an elliptical orbit—because it does not move in the [plane of the] ecliptic, but on an angle of 17 degrees from the ecliptic—and because he needs this for his explanation of that *sphere* of comets that swing, so to say, all around the periphery of the solar system . . . [He thus] claims that one of those comets settled in the solar system as Pluto. Now, if you permit this for Pluto, you must also permit it for Venus. You cannot make a selection between male and female."¹⁴¹²

As things were to turn out, it was eventually found that the mass of Venus does not preclude the planet from exhibiting, if not a cometary tail, at least a comet-like one to this very day.

The Mariner Probes

The magnetic field and magneto tail of Venus had originally been assumed to be similar to that, if not five times stronger than that, of Earth.¹⁴¹³ The Mariner 2 probe to Venus, however, discovered that, at 34,751 km, the planet had no detectable magnetic field.¹⁴¹⁴ Later literature described the Venerian magnetic field as "very weak"¹⁴¹⁵ and unmeasurable.¹⁴¹⁶ The plasma data taken during the encounters of Mariners 5 and 10 later disclosed "a fluid interaction of the solar wind with the ionosphere of Venus" and that this is "consistent with the presence of a standing bow wave which can pass through the high-density regions observed by both spacecraft,"¹⁴¹⁷ itself explained as being due to the lack of the planet's magnetic field.

"The interaction between the solar wind and the Venusian atmosphere appears to resemble in some ways that thought to occur with a comet . . . unusual intermittent features unlike those observed in the terrestrial magnetosheath or in the free-streaming solar wind were observed thousands of scale lengths downstream of Venus . . . "¹⁴¹⁸

The conclusion reached by Bridge and his co-writers was that "Venus probably has a 'tail' hundreds of scale lengths long, suggestive of that of a comet."¹⁴¹⁹ So, also, with the similar report of Ness and his colleagues who wrote that "it has . . . been suggested that the interaction is more analogous to that

¹⁴¹²*Ibid.* (Emphasis as given.)

¹⁴¹³*Sky and Telescope* 15:8 (1955), p. 419.

¹⁴¹⁴D. Lunan, *et al.*, *New Worlds for Old* (New York, 1979), p. 141.

¹⁴¹⁵V. A. Firsoff, "On Some Problems of Venus," *KRONOS* V:2 (Winter 1980), p. 60.

¹⁴¹⁶*New Scientist* (June 17, 1982), pp. 786-788.

¹⁴¹⁷H. S. Bridge, *et al.*, "Observations at Venus by the Plasma Science Experiment on Mariner 10," *Science* (29 March 1974), p. 1296.

¹⁴¹⁸*Ibid.*

¹⁴¹⁹*Ibid.*

of the solar wind with the rarefied atmosphere of a comet in which an extended interaction region leads to a nearly shockless deceleration and deflection of the solar wind flow."¹⁴²⁰

"The magnetic field measurements are suggestive of the sporadic detection of a taillike phenomenon in which the magnetic field at times is quieter (fewer fluctuations), and slightly stronger . . . than usual and closely aligned with the extended sun-Venus line. It seems plausible that these intervals are associated with the solar wind wake of Venus and, as such, they would represent the downstream detection of disturbances in the interplanetary medium trailing behind the planet at scale distances greater than any other planetary wake thus far studied . . .

". . . these magnetic field data may be consistent with the primitive models of the solar wind interaction with a cometary atmosphere in which the solar wind is decelerated in a broad region and a weak shock subsequently develops."¹⁴²¹

In conclusion, Ness and company stated that: "The situation regarding the nature of the obstacle to solar wind flow, *if a cometary-type interaction is not considered*, is . . . uncertain."¹⁴²²

Later still, Fred Scarf voiced the entire matter in this manner:

"The Venus/solar wind interaction is different. It's much more like what we would expect to find at a comet which would also have no magnetic field."¹⁴²³

To be fair, as Robert Driscoll pointed out to me, the cometary aspect of Venus' wake is purely a plasma flow consisting of electrons and ions and, therefore, not optically visible.¹⁴²⁴

"There were no optically visible features except for refraction effects. These were too small . . . to cause twinkling or shimmering of stars seen through the wake . . . except for highly magnified images in telescopes.

"The downstream wake was (and is) a mix of solar wind plasma—nearly all proton ions, a small percentage of helium and heavier ions, with electrons which keep it electrically neutral—and stripped-off atmosphere, ionized and mixed with the solar wind plasma. There seems little likelihood of any

¹⁴²⁰N. F. Ness, *et al.*, "Magnetic Field Observations near Venus: Preliminary Results from Mariner 10," in *ibid.*, p. 1302.

¹⁴²¹*Ibid.*, pp. 1303, 1306.

¹⁴²²*Ibid.*, p. 1306 (emphasis added).

¹⁴²³*Astronomy* (April 1982), pp. 28-30.

¹⁴²⁴R. B. Driscoll to D. Cardona, January 4, 1995, private communiqué.

microparticles of the sizes literally seen in cometary comas such as Halley's . .
. having been in the plasma wake."¹⁴²⁵

The optical invisibility of the Venerian tail is not, however, unique. According to Pioneer Venus data, the Apollo asteroid 2201 Oljato seems to possess a tail of material that may be millions of kilometers long, which tail has never been detected by optical telescopes. In fact, Oljato has been compared to comet Tempel-2 and found to be surprisingly similar. Thus infrared observations have indicated that comet Tempel-2 also possesses an enormous tail which, like that of asteroid Oljato, is optically invisible.¹⁴²⁶

Back in 1980 Wal Thornhill noted that: "The observations of Mariner 10 appear to offer striking confirmation of the possibility that Venus may [still] possess the remnants of a past cometary tail . . ." ¹⁴²⁷ But, although I cannot here go into any details, the conditions under which Venus seems to have acquired its *original* cometary nature were entirely different from those that obtain at present. This seems to have transpired in an environment of closer planetary proximity during which other forces than the solar wind may have participated. Even so, the present Venerian wake does indicate that the planet *can* and *does*, have its atmosphere stripped off to form a comet-like appendage despite its sizable mass.

At bottom, of course, lies the fact that, historically speaking, as we have seen, Venus was spoken of and described by the people of antiquity as having had a cometary form. And, as Isaac Newton himself was shrewd enough to recognize, the "contention that the solar system has no history stands or falls on the *historical* record."¹⁴²⁸ Bob Forrest, like others of his ilk, can of course explain away this or that particular datum on the basis of this, that, or the other thing—as with the explanation of the Mesoamerican smoking Venus having been confused with a smoking volcano, or the beard of Inanna/Ishtar having risen due to the planet's scintillating brightness, neither of which are as convincing as Forrest makes them out to be. But even if better explanations can be found to account for some of the evidence reported above—such as that Venus may have been mistaken for a comet because a comet may have once appeared in visual close proximity to it—these remain but rationalizations. In the end, it is not merely this or that datum that needs to be accounted for, but the entire body of evidence in its totality.

We have seen Inanna/Venus represented by a symbol that best resembles a comet; we have seen the same Inanna being described as a dragon when dragons have been one of the symbolic representations of comets since time immemorial; we have seen Ishtar portrayed as having been adorned with a beard, which beard was specifically said to have belonged to the planet Venus as Dilbat, when the very term "bearded star" was one of the ancient designations for a comet; we have seen Hesperos, as the Evening Star, being dragged out by his hair, when "hairy star" was the most common ancient designations for a comet, and which, in fact, is Lucifer, one of the Latin names for Venus, but also as a comet; we have seen Hathor identified as Venus while being called *Uauti*, which was one of the Egyptian words for "comet"; we have seen Isis identified as Venus and referred to as the "hairy one"; we have seen the Chinese refer to Venus as *Tai-po*, which term translates as "Great Comet"; we have seen the Roman Venus being called *Iubar*, which term, once again, was also used to designate a comet; we have seen the Siberian *Colbon* identified as Venus, said "to have had a tail in the early days"; we have seen the Mesoamerican Venus called a star that smoked, when "smoking stars" was one of the Mesoamerican terms for comets; we have seen the same Mesoamerican Venus, like other Venerian deities, being represented as having been bearded, and, like Inanna/Venus, portrayed as a dragon, when Mesoamerican dragons were specifically identified as comets.

¹⁴²⁵*Ibid.*

¹⁴²⁶*New Scientist* (December 20/27, 1984), pp. 46-48.

¹⁴²⁷W. Thornhill, "Venus—An Interim Report," *SIS Review* IV:4 (Spring 1980), pp. 93-94.

¹⁴²⁸L. C. Stecchini, "The Inconstant Heavens," in A. de Grazia (ed.), *The Velikovsky Affair* (New York, 1966), pp. 122-123.

Are all these coincidences? Is it probable that all these diverse nations mistook Venus for a comet because a comet may once have appeared in its vicinity? And how long would such a comet have remained in the vicinity of Venus? Would none of these nations have realized that the comet and Venus were two separate entities?

As David Talbott asked: "At what point, then, does a 'coincidence' or seemingly irrational use of language (comet-words of glyphs attached to Venus) become an anomaly worth pursuing?"¹⁴²⁹

The belief that Venus had once appeared in cometary form was so prevalent that it continued well into the modern era as is evidenced by a public disputation with Galileo at the Collegio Romano of the Jesuit Society conducted in 1619, during which Horatio Grassi had reason to note that "the ignorant mass of the people had considered Venus as a comet . . ." ¹⁴³⁰

What this really says, as Charles Raspil indicated,¹⁴³¹ is that even at this late date, the average individual was still referring to the planet Venus as a comet.

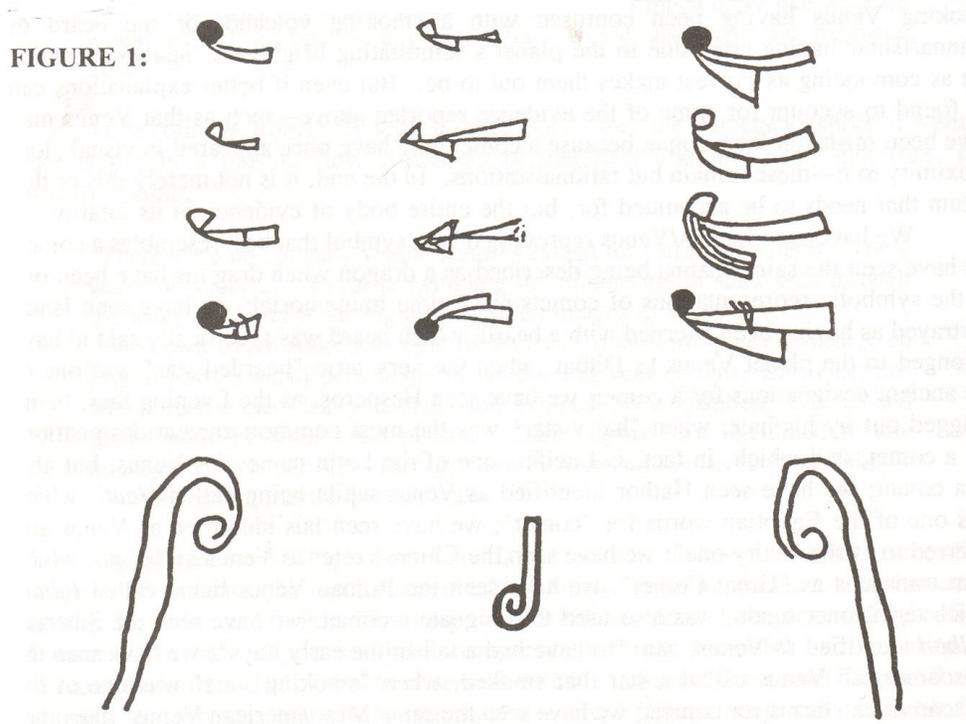


Figure 1
Upper three rows: Variants of Inanna's symbol from Uruk
Bottom left and right: Variant of same from Taanach, Israel
Bottom center: Egyptian hieroglyphic beard

¹⁴²⁹D. Talbott, "The Great Comet Venus," *AEON* III:5 (May 1994), p. 14.

¹⁴³⁰H. Grassi, "An Astronomical Disputation on the Three Comets of the Year 1618," in S. Drake and C. O'Malley, *The Controversy of the Comets of 1618* (Philadelphia, 1960), p. 7.

¹⁴³¹C. Raspil, *loc. cit.*



Figure 2
Sekhmet—a form of Hathor—identified as Isis/Venus

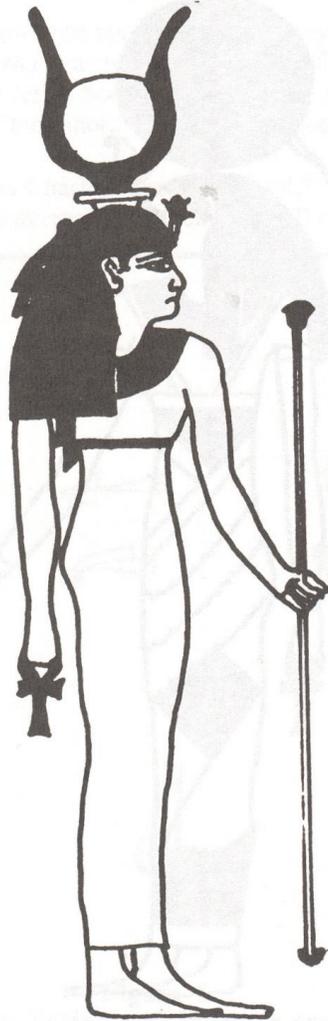


Figure 3
Hathor—identified as Isis/Venus—lauded as *Uauti*, i.e. “Comet”

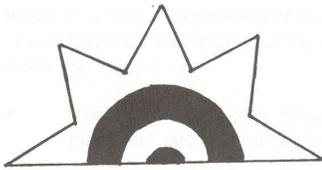
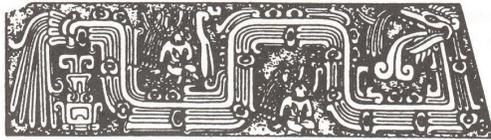
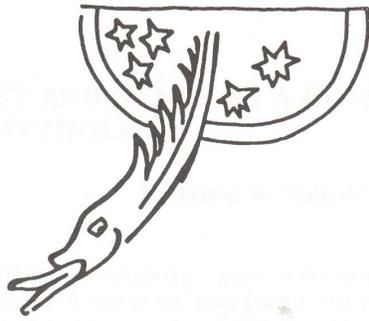


Figure 4

Top: Aztec dragon as comet

Middle: Xiuhtecuhtli—the Turquoise Dragon, identified as the planet Venus

Bottom left: Five pointed eyed star, Mayan hieroglyph for Venus

Bottom right: Mayan Caban curi, symbol of planet Venus

BOB FORREST AND VENUS AS A COMET IN WORLD MYTHOLOGY, By David N. Talbott and Ev Cochrane

To evaluate Immanuel Velikovsky's place in the history of science one must consider fundamentals, for it was at the most fundamental level that his conclusions challenged modern theoretical frameworks. Velikovsky claimed that –

Planets now moving on stable orbits many millions of miles from Earth have not always moved on these paths.

The solar system has been unstable within *human memory*. That means not millions or billions of years ago, but within the past few thousand years.

More than once in recent history, errant planets menaced the Earth.

In myths and legends the world over, ancient man preserved a record of spectacular encounters between planets.

Hence, we cannot understand ancient myth by any reference to the way things are today.

Based on a new interpretation of ancient records, Velikovsky offered a dramatic profile of our planetary neighbors, raising issues that had never been broached before. He proposed that –

The planet Venus once possessed a comet-like tail, and its orbit brought the planet into confrontation with the Earth.

The planet Mars, the war-god of the ancient world, participated directly in Earth-threatening catastrophes and appeared to battle other celestial bodies in the sky.

During an epoch remembered as the Golden Age, the planet Saturn once shone as the dominant light in the sky – when the Earth was apparently a satellite of Saturn.

These principles are not just novel, but central to an entirely new way of looking at man, the Earth, and its celestial habitat. But will further investigation substantiate Velikovsky's revolutionary conclusions?

VELIKOVSKY AND VENUS

It is hardly surprising to find that in virtually every attempt to discredit Velikovsky, the "impossibility" of his Venus scenario is the primary complaint. Both critics and supporters seem to understand very well what a *cometary Venus* would mean for the underpinnings of modern science.

Was Venus a "comet" in the early historical period? If so, a hundred secondary issues debated by Velikovsky and his critics are virtually irrelevant to Velikovsky's place in the history of science. Are there hydrocarbons in the atmosphere of Venus, as Velikovsky had suggested? Did vermin actually descend on the Earth as a result of a Venus encounter? Did a destructive encounter with the Earth really occur in c. 1500 B.C., followed 52 years later by another? Could a planet-sized body actually have been ejected from the gas giant Jupiter? It is certainly conceivable that questions such as these, which have tended to draw much of the attention since the publication of *Worlds in Collision* in 1950, could all be answered in the negative with Velikovsky still gaining the lion's share of vindication for having brought to light the cometary Venus, the previously unrecognized roles of *planets* in myth, and the more general catastrophic motifs. If early races did indeed experience a world-threatening Venus comet, then neither supporter nor critic could dispute Velikovsky's pioneering role in one of the great intellectual revolutions of modern times.

In *Worlds in Collision*, Velikovsky noted numerous tales of disaster and upheaval in which the agent of destruction possessed *cometary* attributes—even as it was identified with the *planet Venus*. The anomalous cometary traits of Venus in world mythology thus represent key pieces of argument; the strength of the argument, in turn, deriving from the breadth of sources. Velikovsky did not rely on traditions of one region only, but drew on traditions from every ancient civilization. He noted, for example, that in Mexican records Venus was the "the smoking star," the very phrase natives employed for a "comet." He documented in both the Americas and the ancient Near East a recurring association of Venus with celestial "hair" and with a celestial "beard," two of the most common terms for "comet" in the ancient world. Yet another popular term for "comet" was "serpent-star," a form taken by the planet Venus in several lands. And the same planet, among the Babylonians and other peoples, was called the "flame," or "torch of heaven," a widespread symbol of comets among ancient peoples.

According to Velikovsky, the history of the Venus-comet inspired some of the most powerful themes of ancient myth and speaks for a collective memory of global upheaval, featuring earthshaking battles in the sky, decimation of nations on Earth, an extended period of darkness, the end of one world age and the birth of another. A study of the ancient records, therefore, becomes paramount to test Velikovsky's thesis—a thesis considered wildly irrational by established science.

THE CRITIC BOB FORREST

When it comes to debunking Velikovsky's historical argument, no critic has applied himself more energetically than Bob Forrest of England. In a six volume work, *Velikovsky's Sources*, Forrest undertook to analyze virtually every historical reference employed by Velikovsky, concluding that in their original contexts the cited references do not support the thesis of *Worlds in Collision*.¹⁴³²

Since its publication, Forrest's work has been frequently cited by scientific skeptics as delivering a definitive blow to Velikovsky. And whatever one's opinion on the merits of *Velikovsky's Sources*, it is to

¹⁴³²Forrest's research was originally published between 1981 and 1983 as *Velikovsky's Sources*, a six-volume work. It was subsequently updated and summarized as *A guide to Velikovsky's Sources* (Santa Barbara, 1987). The quotes in this article come from the latter work unless otherwise noted. Forrest reiterated many of the same criticisms in the article "Venus and Velikovsky: The Original Sources," in *The Skeptical Inquirer* 8 (Winter 1983/84), pp. 154-163.

Forrest's credit that in the forty years since publication of *Worlds in Collision*, his is the only substantial critique of Velikovsky's use of myth.

"Despite the scholarly appearance of Velikovsky's work," Forrest writes, "I think the theories put forward in *Worlds in Collision* are wrong at an elementary and common sense level."¹⁴³³

And what, at an "elementary level," does Forrest object to? "The gist of the objection to it is that one will nowhere find anything like a direct historical reference to catastrophic bombardments by the planets Venus and Mars."

Having devoted, between us, almost forty years to the exploration of myth, we find the objection particularly interesting because our conclusion is quite the opposite. The planetary subjects of *Worlds in Collision* are Venus and Mars, and the catastrophic roles of these planets in ancient times are not only evident but provable through normal rules of logic and demonstration. For the sake of focus, we will here consider only Venus. It is not only possible to answer the question—was Venus formerly a "comet"?—but to answer the question in overwhelming detail, with incontrovertible data and an inescapable conclusion: Velikovsky's comet Venus lies very close to the center of ancient religious, artistic and literary traditions.

VELIKOVSKIAN RESEARCH AND CATASTROPHISM

How can it be that different researchers, approaching the same field of data, can draw such incompatible conclusions? The heart of the issue, we suggest, has to do with one's approach to the subject matter. The Velikovskian researcher has discovered that *few if any of the primary themes of myth answer to our familiar sky*. Hence, to focus on recurring themes is to focus on the *recurring anomalies* of myth.

But rather than confront the issue of recurring anomalies, Forrest descends into a swamp of marginal details, picking at virtually every paragraph of *Worlds in Collision*

while always avoiding cross-referencing. As a result, Forrest consistently fails to see past the veil in which modern perception has wrapped ancient myth. It is as if general patterns and connections are of no interest. In every case of an *anomaly* noted by Velikovsky, Forrest's "answer" is simply to cite someone else's guess at an explanation (and we do mean *guess*)—though many of the authorities cited offered their guesses prior to Velikovsky's novel interpretation, and few if any of them seem aware of the larger patterns detected by the author of *Worlds in Collision*. In this way, Forrest reverses Velikovsky's approach, for Velikovsky connected anomalous Venus images of one land with corresponding anomalies from other parts of the world. *Recurring anomalies, as correctly perceived by Velikovsky, are a key to discovery.*

A useful example of the methodological issue is the recurring "world catastrophe" myth. For the Velikovskian researcher, the question is whether *globally-experienced* events will account for the universal theme of the world-destroying cataclysm. Or must all such themes be explained by wholly separate, localized disasters? If one resorts to the latter explanation, then no underlying integrity of catastrophe myths is possible in significant detail. But the inescapable counterpart of this observation is that, if the myths of widespread cultures present the same improbable story in significant detail, then it is the localized explanation that becomes impossible.

A reasonable methodology cannot ignore the convergence of recurring themes on an underlying idea, even if that idea stands outside modern theoretical frameworks.

In recognition of this principle, we have chosen in this overview to draw periodically upon Bob Forrest's critique as a means of clarifying the methodological issues. We trust the reader will take this use of

¹⁴³³B. Forrest, *A Guide to Velikovsky's Sources* (Santa Barbara, 1987), p. 5.

Forrest's work in a constructive spirit and find in our approach no attempt to discredit the researcher himself, since Forrest is, after all the best of Velikovsky's critics and deserves much credit for having raised questions no Velikovskian researcher can afford to ignore.

SMOKING STAR

In arguing for the cometary character of Venus, Velikovsky cited Aztec records wherein the planet Venus shared the same name given a comet.

"The early traditions of the peoples of Mexico, written down in pre-Columbian days, relate that Venus smoked. 'The star that smoked, *la estrella que humeava*, was Sitlal Choloha, which the Spaniards called Venus.'

"Now, I ask,' says Alexander Humboldt, 'what optical illusion could give Venus the appearance of a star throwing out smoke?'"¹⁴³⁴

"Sahagun, the sixteenth century Spanish authority on Mexico, wrote that the Mexicans called a comet 'a star that smoked.'¹⁴³⁵ It may thus be concluded that since the Mexicans called Venus 'a star that smoked,' they considered it a comet."¹⁴³⁶

In Bob Forrest's mind, the Aztec references could have nothing to do with "what may or may not have happened back in the mid second millennium BC" —because the references to Venus "smoking" come from the sixteenth century A.D. As to the source of the tradition, Forrest was apparently satisfied with the first guess he uncovered. "All we have are some sixteenth century records which say, every so often, that the star smoked, but since the smoking seems frequently to be intertwined with earthquake activity . . . Humboldt's assumption seems reasonable."¹⁴³⁷ With that stated, Forrest moved on, never returning to the issue of the Aztec "smoking star."

An entirely different approach would have been to explore the possibility of a broader venus-comet association to see where the available evidence leads. Pursuing this course, Forrest would have quickly found that the association of "earthquake activity" with "smoking stars" belonged to the general mythology of the *comet* among the Aztecs—a fact immediately challenging the volcanic explanation of the smoking star. Thus, with respect to the comets portrayed in the *Codex Vaticanus* and *Codex Telleriano-Remensis*, an authority on Mexican astronomy writes:

"Comets (*citlalimpopoca*, or the stars that smoke) are represented frequently by the surviving historical documents, usually by a stellar image on a blue background with emanating streams of smoke . . . These usually signify that a person of nobility will die; for example [one picture] tells of the death of the ruler of Tenochtitlan following the apparition of a

¹⁴³⁴WIC, *op. cit.*, p. 173, citing Humboldt, *Researches*, II, p. 174., and E. T. Hamy, *Codex Telleriano-Remensis* (1899).

¹⁴³⁵Citing B. Sahagun, *Historia generale de las cosas de Nueva Espana*, Bk. VII, Chapter 4.

¹⁴³⁶WIC, *op. cit.*, p. 173. Among the Yucatec Maya names for comet are *Budz ek*, *budzal ek*, and *budzil ek*. The literal meaning is "smoke stars." W. Lamb, "Star Lore in the Yucatec Maya Dictionaries," in Anthony Aveni, ed., *Archaeoastronomy in Pre-Columbian America* (Austin, 1975), p. 237.

¹⁴³⁷B. Forrest, *op. cit.*, p. 22.

comet [smoking star]; later another comet occurs, then an earthquake, all of nature's events being connected in the Aztec cosmic view."¹⁴³⁸

Why, then, do the astronomer-priests summon *comet-like* images of Venus "smoking" in a symbolic relationship to fear-inspiring natural disturbances? Forrest seems unaware that astrological texts and omens draw upon very ancient traditions, not just contemporary observations. Thus one scholar has noted of Mesoamerican religion: "A dominant characteristic of Toltec and Aztec sacred histories was a prophetic emphasis, that is, the view that linked past, present, and future together in some detailed meaningful pattern."¹⁴³⁹

Following Forrest's methodological assumption, no records of "portents" in the sky recorded in the last three millennia would be of any relevance to Velikovsky's argument, even when repeatedly attaching explicit cometary symbols to Venus!

With respect to the description of Venus as a "smoking star" in the *Codex Telleriano-Remensis*, Aveni offers his own attempt at an explanation: "Perhaps a cometary object appeared near the planet."¹⁴⁴⁰ Of course, Forrest could just as easily have cited this guess, then dropped the whole issue. but is there something more worth investigating here?

Throughout the Americas, including Mesoamerica, natives called comet the "star with hair," or a "long-haired star," or a "maned star," an appellation that accords completely with the global language of the comet. In fact, the "long-haired star" is the single most common phrase for "comet" around the world, and the word comet itself comes from the Greek *Kometes*, the "long-haired" [star]. Yucatec Maya dictionaries, for example, give as a gloss for "smoke star" the "maned comet."¹⁴⁴¹ But curiously, Mesoamerican peoples used this very language to describe Venus. Thus, the Cuicatec Indians still describe Venus as "the star like a hairy beast."¹⁴⁴²

To encounter the long-flowing locks of Venus, one need only consult available sources. Turn to the Incan word for Venus, for example. According to William Prescott, Venus was "known to the Peruvians by the name of Chasca, or the 'youth with the long and curling locks.'"¹⁴⁴³ Burr Cartwright Brundage adds that the Incan Venus was "the Radiant Star with the Flowing Hair."¹⁴⁴⁴

"The morning star, Chasca (The Disheveled One), dispensed stores of freshness and loveliness upon flowers, princesses, and virgins below. She was the deity of the rosy cloud rack of morning, and when she shook out her long hair she scattered the dew upon the earth."¹⁴⁴⁵

¹⁴³⁸A. Aveni, *Skywatchers of Ancient Mexico* (Austin, 1980), p. 27. Earthquakes were associated with comets in the Old World as well. See the discussion in W. Gundel, "Kometen," *RE* (Stuttgart, 1894-1980), columns 1143-1149.

¹⁴³⁹D. Carrasco, *Quetzalcoatl and the Irony of Empire* (Chicago, 1982), p. 77.

¹⁴⁴⁰A. Aveni, *op. cit.*, p. 27.

¹⁴⁴¹W. Lamb, *op. cit.*, p. 237.

¹⁴⁴²E. Hunt, *The Transformation of the Hummingbird* (Ithaca, N.Y., 1977).

¹⁴⁴³W. Prescott, *The World of the Incas* (New York, 1970), p. 78.

¹⁴⁴⁴B. C. Brundage, *Empire of the Inca* (Norman, 1963), p. 162.

¹⁴⁴⁵*Ibid.*, p. 50. This "charming" and "lovely" image of Venus goes hand in hand with the portrait of the world-destroying hag. As acknowledged by the best students of the great mother archetype—Erich Neumann, for example— the goddess of feminine charm and the goddess of Terrible Aspect are one and

The point to be underscored is that Forrest's "explanation" of the Aztec Venus/ "smoking star" identification fails to acknowledge *converging lines of evidence*. Aztec comet as "smoking star," Aztec Venus as "smoking star," Aztec and Mayan "long-haired star" as comet; Cuicatec Venus as "hairy star," Incan Venus as "long-haired star." Hence, the methodological issue is placed in sharp relief.

There is another way of looking at the issue logically. Around the world there are only a small number of pre-astronomical terms or glyphs for "comet." You could, in fact, count the primary glyphs on the fingers of one hand:

long-haired star (star with flowing locks, mane, tresses, disheveled hair, beard, hairy tail);

torch-star (flame, smoke, smoking star, train of fire, ember, spark, or train of sparks);

celestial feather (winged star, soul-bird, feathered headdress, shining bird's tail);

cosmic serpent, dragon or similar monster.

heart-soul of a deceased god-king or great leader rising in the sky.¹⁴⁴⁶

At what point, then, does "coincidence" or a seemingly irrational use of language—comet-terms or glyphs attached to Venus—become an anomaly worth pursuing? Forrest not only sidesteps the implications of parallel, cometary images of Venus in other lands, he ignores the convergence of such images *in Mexico*. As a methodology, the approach is disastrous because there is much, much more.

QUETZALCOATL

In the popular Aztec myth of Quetzalcoatl, the Venus-comet anomaly grows by leaps and bounds. And in this case, the *completeness* of the cometary motifs leaves no room for *ad hoc* explanations.

Whether remembered by the Aztecs as a former great king and founder of a golden age, or a former sun god ruling a primordial epoch, Quetzalcoatl was a cultural hero without equal in the Aztec pantheon, his countenance adorning countless temple walls, frescoes, codices, sarcophagi and monoliths strewn across Mexico.

the same mythical figure. The "comely" beauty of the long-haired Venus gives way to the raging, all-devouring goddess with disheveled hair.

¹⁴⁴⁶For a thorough review of these various symbols in ancient myth and religion, see D. Talbott and E. Cochrane, "On the Nature of Cometary Symbolism," *KRONOS* 11:1 (Fall 1985), pp. 23-34; E. Cochrane, "On Comets and Kings," *AEON* 2:1 (1989), pp. 53-75; D. Talbott, "The Great Comet Venus," *AEON* 3:5 (1994); pp. 5-51. The remaining general hieroglyphs for the comet could be counted on the fingers of your second hand. They include: a sword, a bundle of grass or straw (whisk, broom), or a spiraling rope (cord, tie, or knot).

The climactic event in the Quetzalcoatl myth is the god's catastrophic death and transformation in an overwhelming disaster—an event endlessly repeated in sacrificial rites and supplying the cornerstone of Aztec calendar rituals and astronomical symbolism. In a pervasive version of the myth, at the death of Quetzalcoatl the god's *heart* or *soul* rose in the sky as a great spark or ember, trailing smoke and fire—a "star" whose fiery train the Aztecs portrayed as the streaming tail of a quetzal-bird.

Was this flaming star a "comet"? One notes that Aztec artists often drew comets as stars with quetzal tails, the bright and luminous plumes of the quetzal signifying streams of fire and thereby providing a particularly well-suited hieroglyph for a comet.¹⁴⁴⁷ Indeed, the symbolism accords well with that of other peoples. Thus the Pawnee gave to the comet the name *u: pirikis kuhka*, "feathered headdress."¹⁴⁴⁸ In Africa, a comet's streaming tail was identified as the feathers of the nightjar, and the natives say of a comet, "it is wearing streaming feathers."¹⁴⁴⁹ Carl Sagan, in his review of worldwide comet motifs, notes that comets are called "tail stars" and "stars with long feathers."¹⁴⁵⁰

But there is a problem here. In each of the several variations on the story of Quetzalcoatl's death which have been preserved, one of the central elements is the identification of the departing heart-soul as the *planet Venus*. Burr Cartwright Brundage gives this summary: "The god's heart, like a great spark, flies up . . . to become a new and splendid divinity, the Morning Star."¹⁴⁵¹ A native source declares: "Then the heart of Quetzalcoatl rose into heaven and according to the elders, was transformed into the Morning Star."¹⁴⁵²

If the story has roots in an actual celestial occurrence (as explicitly claimed in the myths), the "deaths" of Quetzalcoatl must have involved a cosmic disaster of unprecedented scale, for no mythical-historical event left a deeper impression on Aztec thought and culture. From this traumatic episode, the Aztecs evolved their collective sense of *cyclical time*, including a calendar of world ages. The death of Quetzalcoatl, the onset of celestial confusion, and the transformation of his heart-soul in the Morning Star signified nothing less than the end of one world age and the beginning of another.¹⁴⁵³

COSMIC SERPENT

The planet Venus, the rising heart-soul of Quetzalcoatl, in addition to being portrayed as an ember-like star (= comet), and as a star with quetzal-tail (= comet), is also said to have taken the form of a great *cosmic serpent* (= comet both in Mexico and in the universal language of comets).

The name Quetzalcoatl itself is simply a combination of two Nahuatl terms—that for the *quetzal*-bird, known for its long, brilliant turquoise tail, and that of the serpent or *coatl*.¹⁴⁵⁴ Thus two of the most common comet glyphs are brought together in the name of the god. And the combined hieroglyphs clearly have a long history. The earliest known version of the plumed serpent pre-dates the Aztecs by many centuries,

¹⁴⁴⁷P. Brown, *Comets, Meteorites and Men* (New York, 1973), p. 18 n.

¹⁴⁴⁸Von Del Chamberlain, *When Stars Come Down to Earth* (College Park, 1982), p. 256.

¹⁴⁴⁹A. Roberts, "Comets importing change of Times and States': ephemerae and process among the Tabwa of Zaire," *American Ethnologist* (1982), p. 718.

¹⁴⁵⁰C. Sagan & A. Druyan, *Comet* (New York, 1985), p. 14.

¹⁴⁵¹B. Brundage, *The Phoenix of the Western World* (Norman, 1982), p. 263. See also the discussion in H. Nicholson, "Religion in Pre-Hispanic Mexico," in *Origins of Religious Art and Iconography in Preclassic Mesoamerica* (1976), p. 429.

¹⁴⁵²*Anales de Cuauhtitlan*.

¹⁴⁵³As we have documented elsewhere, the symbolism of the ancient sun-god's heart-soul involves both Venus and Mars in clearly defined roles.

¹⁴⁵⁴M. Miller and K. Taube, *The Gods and Symbols of Ancient Mexico and the Maya* (London, 1993), p. 141.

appearing on monuments of the Formative Olmecs.¹⁴⁵⁵ Conceptually, the avian serpent reached significantly beyond Aztec culture. The Maya god *Kukulkan* carries an equivalent meaning, as does the Quiché figure, Gucumatz.¹⁴⁵⁶ The same figure appears to have entered Zuni ritual as the plumed serpent Kolowisi, while Hopi tradition celebrates the plumed serpent Palulukong.¹⁴⁵⁷

Though the figure of Quetzalcoatl is complex and appears to combine originally distinct traditions, the identification of the spiraling serpent itself (the transformed heart-soul) with Venus has survived even into modern times. Some of the Tzotzil groups, for example, still describe Venus as "the Big Serpent" (Mukta Ch'on).¹⁴⁵⁸

Is it significant, then, that Aztec manuscripts depict a *comet* as a fiery serpent or dragon-like creature descending from the stars? In this exploration of comet symbolism, Peter Lancaster Brown observed that the natives of Mexico represented comets "by the plumed serpent depicted in various forms."¹⁴⁵⁹ The priest-astronomers similarly knew the comet as "the star serpent."¹⁴⁶⁰ But what does this say about the acknowledged identification of the plumed serpent with the *planet Venus*, the ascending heart-soul of Quetzalcoatl?

"It seems very likely that the white and bearded god who appeared in the east associated with the Quetzalcoatl (Serpent God) legends of pre-columbian Middle America relates to the apparitions of spectacular comets in the morning sky and *not* to the planet Venus," Brown writes.¹⁴⁶¹ Here again we see an author attempting to rationalize a clearly stated Venus-comet connection, offering his own explanation. But in this instance the "explanation" involves nothing less than a rewriting of the Aztec religion: for the identify of the transformed heart-soul of Quetzalcoatl as the Morning Star was an unshakable tenet of the myths and rites.

With respect to the Mesoamerican celestial serpents and dragons, there is also the issue of attached streamers that often look more like long-flowing, spiraling locks of hair than feathers. (See Figure 1 at end) This unique feature is particularly significant, considering that the disheveled "mane" of the celestial serpent-dragon is a worldwide motif.¹⁴⁶²

A symbolic counterpart of this streaming hair is the enigmatic, but frequently depicted, *beard* of the Mesoamerican serpent-dragon. The Aztec Plumed Serpent, the Mayan Great Bearded Dragon, and numerous counterparts of these celestial monsters are distinguished by flowing beards that are every bit as preposterous, on the face of it, as their streaming manes. (See Figure 2 at end) The reader will recall the celestial beard or bearded star in our short list of comet symbols as a logical extension of the "long-haired star." (Thus the Greek *pogonias*, the "bearded star," signifies a "comet.") While a bearded serpent is a biological absurdity, the anomalous beard is immediately explained if the objective reference for these traditions was a long-haired star or comet. If the incongruous beard did not mirror a comet-like form in the

¹⁴⁵⁵See Monument 19 from La Venta showing a rattlesnake with avian beak. Mary Miller and Karl Taube, *op. cit.*, p. 141.

¹⁴⁵⁶P. Brune, editor, *Companion to Literary Myths, Heroes and Archetypes* (New York, 1992), p. 443.

¹⁴⁵⁷Mary Miller and Karl Taube, *op. cit.*, p. 141.

¹⁴⁵⁸I. Sprajc, "The Venus-Rain-Maize Complex," *JHA* 24 (1993), p. 22.

¹⁴⁵⁹P. L. Brown, *op. cit.*, p. 18 n.

¹⁴⁶⁰C. Burland, *The Aztecs* (London, 1980), p. 102.

¹⁴⁶¹*Ibid.*, (Emphasis added)

¹⁴⁶²The "hairy" serpent can be found around the globe. In ancient Egypt, for example, not only do the well-established serpent and dragon figures possess streaming fiery "hair," the root words for hair and for serpent continually overlap, demonstrating the archetypal unity of what is a *profoundly incongruous idea* apart from the cometary connection.

sky, then the bearded serpent is one more anomaly left unanswered, despite a consistent pattern that seems to cry out for recognition.

THE GREAT COMET

In seeking out the general patterns of the Mesoamerican Venus-myth, we cannot fail to observe that our listed cometary symbols are not just present, but *prominent*. Venus is described as a great serpent; as "hairy" or "long-haired"; as a fiery ember or smoking star; and as the "heart-soul" of the ancient sun-god. Each of the symbols, moreover, is inextricably connected with each other, yet they do not direct us to any recognizable forms either in the sky or in the natural world today. Rather, they contradict natural experience at every level and remain *unexplained* despite decades of discussion by the best experts.

Witness, for example, the surprising conclusion reached by the Mayan scholar Peter Joralemon upon confronting the incongruous symbolism of the celestial dragon:

"The primary concern of Olmec art is the representation of creatures that are biologically impossible. Such mythological beings exist in the mind of man, not in the world of nature."¹⁴⁶³

It is easy to see how one might draw this conclusion. But if the symbolism lacks any roots in "the world of nature" and is simply the result of chaotic imagination, then an even greater anomaly arises: Why do the same symbols continually occur in juxtaposition? The critic is left with nothing but coincidence to account for the convergence. And yet, when it comes to the convergence of *all five of the world's most common cometary symbols on one celestial body*—Venus—is it reasonable to ask sheer imagination and "coincidence" to account for the situation?

In point of fact, most authorities continue to look for natural references for the Venus myth because it is difficult to believe that such dramatic images as the plumed serpent could dominate an entire civilization without a link to natural experience. Only the rarest of specialists would suggest that the primitive mind conjured up its primary mythical forms out of a wholesale denial of the natural world. Indeed, if they can find even the most remote natural explanation, the experts *will* use it. Miguel Leon-Portilla, for example, offers a picturesque explanation of the Venus-Quetzalcoatl relationship—

"The association of Venus and Quetzalcoatl can probably be attributed to the fact that when this planet sets upon the moving waters of the Pacific, its reflection seems not unlike a serpent with brilliant scales and plumes."¹⁴⁶⁴

Here is a guess at a "natural explanation" that would fit easily into Bob Forrest's analysis, as is there nothing in the plumed serpent crying out for a comparison with the highly improbable yet *similar* images of other peoples—and as if the combined *cometary* associations deserve no attention.

¹⁴⁶³P. Joralemon, "The Olmec Dragon," in H. Nicholson, ed., *Origins of Religious Art and Iconography in Preclassic Mesoamerica* (Los Angeles, 1976), p. 33.

¹⁴⁶⁴M. León-Portilla, *Aztec Thought and Culture* (Norman, 1963), p. 51.

How, then, does one break through the vicious circle? And how does one properly weigh the lines of evidence when confronted with a repeated convergence of comet words and symbols on Venus? It seems that until one asks the question—did Venus formerly present itself as a spectacular "comet"?—even the most obvious evidences will be seen as something else, as confirmation of the recklessness and confusion of myth, yet another reason not to take myth seriously.

It needs to be understood that we are not dealing with a "multiple choice" situation with respect to plausible explanations. If one includes in the lexicon of comets the "shooting star," the mythical image of which is drawn from the same reservoir of natural experience, then the only known celestial phenomenon called a "long-haired star" is a comet; the only celestial phenomenon known to have been represented as a star with streaming "tail feathers" is a comet. The only celestial phenomenon known to have been represented as a star with a serpentine tail is a comet. That these very images are repeatedly jointed with each other and attached to Venus cannot be explained away by *ad hoc* reasoning.

Now add the mythical role of the comet as the ascending soul of a former great king together with the explicit role of *Venus* as the ascending soul of the *prototypical king* Quetzalcoatl, and you will begin to see what is at issue here. If nothing else, the stunning convergence of cometary images should make clear that Humboldt's guess about the "smoking star" Venus and a local volcano is hardly a sufficient answer! The repeated juxtaposition of cometary motifs with Venus—a planet whose appearance today could not begin to explain these associations—forces us to confront the logical alternative: If Venus *did* formerly possess a comet-like "tail," all of the aforementioned anomalies disappear.

Hence the imperative of cross-referencing when taking up such issues. No approach that isolates each evidential fragment, explaining away that fragment without explaining parallels and converging cometary images, can resolve the Venus-comet issue. And in this sense, Forrest's analysis breaks down completely with the very first instance cited for the comet Venus is a *global* theme, and the one credible explanation of the myth is that Venus *did* look like a comet and that it *did* participate in literally earthshaking events not all that long ago. One only has to follow the evidence to determine that this is so.

VENUS IN MESOPOTAMIAN TRADITION

It is remarkable enough that aboriginal peoples of Mesoamerica affixed to Venus the words and symbols for "comet," but what would be our conclusion if we find the same anomaly in the Old World? The ancient cultures of Mesopotamia offer a good source of comparison with those of Mesoamerica, it being well-known that the inhabitants of the Fertile Crescent worshipped the planet Venus with Obsessive zeal.

In ancient Sumeria, the planet Venus was worshipped under the name of Inanna—"Queen of Heaven"; in later Akkadian and Babylonian times, as Ishtar. As scholars have noted, the celestial identification of the goddess is already present at the dawn of history. On this issue, Heimpel has written:

"It is, of course, a well-known fact that Inanna was identified with the planet Venus. Astronomical and astrological texts provide clear identifications . . . When and how the link between the planet was made cannot now be ascertained. It is prehistorical . . . [It] was already complete when Inanna met Ishtar."¹⁴⁶⁵

¹⁴⁶⁵W. Heimpel, "A Catalog of Near Eastern Venus Deities," *Syro-Mesopotamian Studies* (Malibu, 1982), pp. 10-11.

If we are to reconstruct the ancient conceptions surrounding the planet Venus, the cult of the great goddess will prove to be an invaluable source. But ancient Mesopotamia was also the birthplace of the earliest astronomy, an outgrowth of planetary worship and concomitant, systematic observations of the respective celestial bodies. Here, too, traditions can be found surrounding Venus which are most difficult to reconcile with modern astronomical theory.

For example, in light of the widespread designation of comets as "bearded-stars," it is striking to find that the Babylonian astronomical records described the planet Venus as "bearded."¹⁴⁶⁶ Nothing about the present appearance of Venus would appear to warrant such terminology, and consequently such reports have caused much speculation among archaeoastronomers exploring these early texts.

Vestiges of this anomalous "beard" can be found in the ancient cults of the Venus-goddesses, as well. In an ancient Babylonian cult, for example, Ishtar was represented as bearded.¹⁴⁶⁷ The Cypriote Aphrodite was likewise depicted as bearded, as was the Latin Venus.¹⁴⁶⁸ Inasmuch as each of these goddesses represented the very ideal of beauty and femininity to their respective cultures, the presence of the beard is difficult to explain apart from their identification with the Cytherean planet. Yet no explanation of the goddess' beard is necessary if Venus formerly possessed *any* comet-like features.

Bob Forrest's response to this anomaly follows the pattern established in the first pages of *A Guide to Velikovsky's Sources*. Once again, he refers us to the surmises of various authorities –

"Velikovsky takes Chaldean references to 'the beard of Venus' to refer to the tail of his Venus Comet. But the beard may have a much more mundane explanation . . . ; when the planet Venus is at its brightest, it seems, when viewed with the naked eye, to have a spiky or jagged appearance, this being produced by the effects of turbulence in the earth's atmosphere. The expression 'with a beard' may refer simply to this phenomenon. It is also known, however, that the phrase 'Venus has a beard' could refer to stars in the vicinity of the planet. The presence or absence of a beard was supposed to have astrological significance."¹⁴⁶⁹

Forrest here cites two different and unrelated "explanations" of the bearded Venus, the first in terms of atmospheric distortion, the second in terms of companion stars. Of course, almost any guess *would* be a sufficient answer were it not for independent lines of evidence which point to cometary attributes of Venus. And it is precisely because of the global, *overarching* cometary symbolism of Venus that such guesses lose all credibility.

In addition to the anomalous beard, it is common to find the Venus-goddesses described as a "torch," raining fire and destruction from heaven. Inanna, for example, is "the pure torch that flares in the sky, the heavenly light, shining bright like the day, the queen of heaven,"¹⁴⁷⁰

¹⁴⁶⁶P. Gossman, *Planetarium Babylonicum* (Rome, 1950), p. 41.

¹⁴⁶⁷W. Heimpel, *op. cit.*, p. 15.

¹⁴⁶⁸L. Farnell, *The Cults of the Greek States* (New Rochelle, N.Y., 1977), Vol. 2, p. 628. See also Macrobius, *Saturnalia* 3:8:2.

¹⁴⁶⁹B. Forrest, *op. cit.*, p. 23.

¹⁴⁷⁰T. Jacobsen, *The Treasures of Darkness* (New Haven, 1976), p. 139.

The Akkadian Ishtar is described in similar terms: "O brilliant one, torch of heaven and earth, light of all peoples . . . , O firebrand which is kindled against the enemy, which brings about the destruction of the furious, O gleaming one, Ishtar, assembler of the host."¹⁴⁷¹

A similar prayer is the following:

"Valiant Ishtar, your valor is great, shining torch of heaven and earth, . . . fierce in the battle that cannot be withstood, brave in the melee, fire that blazes against the enemy, who causes the destruction of the fiercest soldiers."¹⁴⁷²

Conventional scholars have typically interpreted Inanna/Ishtar's epithet "torch" by reference to Venus' brilliant appearance in the evening sky. Forrest follows suit in his response to Velikovsky's discussion of Ishtar/Venus as the "torch-star":

"Velikovsky claims that Chaldean descriptions of the planet Venus are not consistent with the Venus we see today – for example, 'bright torch of heaven' and 'diamond that shines like the sun' . . . But when Venus is at its brightest in the early evening sky it is a beautiful sight, and it requires very little imagination to see it as a 'bright torch of heaven'."¹⁴⁷³

Such an interpretation, although perfectly logical as a first attempt to account for the imagery, fails to account for the catastrophic context of the "torch-star" and ignores entirely the significant fact that Ishtar/Venus' description as "bearded" and "torch-like" brings together two of the most common terms for "comet." Indeed, it is the catastrophic overtones of the imagery surrounding the epiphany of the Venus-goddess as "torch" which rule out Forrest's interpretation. Repeatedly the planet-goddess is compared to fire and/or said to rain fire from heaven:

"Celestial luminary, you're like the fire! Verily you [shake?] the earth. Hierodule Inanna, celestial luminary, you are like the fire!"¹⁴⁷⁴

"You are the celestial luminary blazing like fire upon the earth."¹⁴⁷⁵

"I (Inanna) am the flaming fire, raining down upon the enemy country."¹⁴⁷⁶

¹⁴⁷¹From "Prayer of Lamentation to Ishtar" in J. Pritchard, *Ancient Near Eastern Texts Relating to the Old Testament* (Princeton, N.J., 1969), p. 384.

¹⁴⁷²E. Reiner & H. Guterbock, "The Great Prayer to Ishtar and its Two Versions from Bogazkoy," *Journal of Cuneiform Studies* 21 (1967), p. 261.

¹⁴⁷³B. Forrest, *op. cit.*, p. 23.

¹⁴⁷⁴M. Cohen, *Sumerian Hymnology: The Ersemma* (Cincinnati, 1981), p. 130.

¹⁴⁷⁵*Ibid.*, p. 134.

The Akkadian Ishtar is described in similar terms, typically in the general context of great tumult in heaven and earth: "By causing the heavens to tremble and the earth to quake, By the gleam which lightens in the sky, By the blazing fire which rains upon the hostile land, I am Ishtar."¹⁴⁷⁷

Now we ask: Does the current appearance and/or behavior of Venus inspire such spectacular imagery? In what sense can the planet be said to shake heaven and earth? Yet these very same descriptions of the goddess' rampage can be found throughout the ancient world.¹⁴⁷⁸

The goddess as "torch-star," moreover, is inseparable from the goddess as warrior, whose rampage threatens to blot out the sun and destroy the world. The following description of Ishtar/Venus is typical in this regard:

"Planet for the warcry . . . Gushea [an epithet of Ishtar], whose mail is combat, clothed in chilling fear . . . At the thought of your name, heaven and the netherworld quake . . . Shining torch of heaven . . . Fiery glow that blazes against the enemy, who wreaks destruction on the fierce, Dancing one, Ishtar . . ."¹⁴⁷⁹

Yet another prayer addresses the planet-goddess as follows:

"I rain battle down like flames in the fighting, I make heaven and earth shake with my cries, . . . I, Ishtar, am queen of heaven and earth. I am the queen . . . I constantly traverse heaven, then (?) I trample the earth, I destroy what remains of the inhabited world."¹⁴⁸⁰

Descriptions of Inanna's celestial rampage are equally dramatic:

"You make the heavens tremble and the earth quake. Great Priestess, who can soothe your troubled heard? You flash like lightning over the highlands; you throw your firebrands across the earth. Your deafening command . . . splits apart great mountains."¹⁴⁸¹

¹⁴⁷⁶A. Sjöberg and E. Bergmann, *The Collection of the Sumerian Temple Hymns* (Locust Valley, 1969), p. 86.

¹⁴⁷⁷Quoted in I. Velikovsky, *Worlds in Collision* (New York, 1973), p. 186.

¹⁴⁷⁸See here the discussion in E. Cochrane, "The Birth of Athena," *Aeon* 2:3 (1990), pp. 5-28.

¹⁴⁷⁹B. Foster, *Before the Muses: An Anthology of Akkadian Literature* (Bethesda, Md., 1993), pp. 510-512.

¹⁴⁸⁰*Ibid.*, p. 74.

¹⁴⁸¹D. Wolkstein and S. Kramer, *Inanna* (New York, 1983), p. 95.

"When you are angrily staring that which is bright gets dark, you turn the midday light into darkness."¹⁴⁸²

That the earliest literature surrounding Inanna/Ishtar depicts her as a warrior—but also as a planet—is well-known.¹⁴⁸³ With the exception of Velikovsky, however, scholars have yet to consider the possibility that this curious juxtaposition of imagery has its original reference in a spectacular disaster associated with the planet Venus.

Indeed, it is safe to say that so long as the leading scholars are content to rely on the current appearance of Venus to account for the ancient imagery surrounding Inanna/Ishtar, the original nature of the goddess must remain elusive and more than a little incongruous. Thus, Jacobsen writes that the "offices attributed to her show little unity or coherent pattern." Kramer, likewise, speaks of the "contrasting strands in Inanna's multi-faceted character." Oppenheim, finally, held that Ishtar represented "divine qualities which are extremely hard to characterize."¹⁴⁸⁴

If the leading scholars have had trouble explaining the original significance of the planet-goddess as "bearded," "torch," and warrior, they are thoroughly at a loss when encountering the goddess as a great dragon raining fire from the sky! In the *Exaltation to Inanna*, for example, the goddess is invoked as follows:

"Like a dragon you have deposited venom on the land. When you roar at the earth like Thunder, no vegetation can stand up to you. A flood descending from its mountain, Oh foremost one, you are the Inanna of heaven and earth! Raining the fanned fire down upon the nation . . ."¹⁴⁸⁵

Once again we are confronted with a glaring anomaly: Despite the fact that the planet Venus never presents the appearance of a dragon-like body spanning the skies, the very same image has now been found in earliest Mesopotamia as well as Mesoamerica! If the objective reference behind the goddess' epiphany was a comet-like body, however, the imagery appears perfectly coherent, comets being known as "dragon-stars" since time immemorial.

Some scholars, with Forrest, have sought to distinguish between the goddess as dragon and the goddess as planet when attempting to account for the bizarre references to the fire-spewing monster. Thus, while admitting that a serpent-dragon would be a fine manifestation of comet¹⁴⁸⁶ and that Inanna/Ishtar was described as a dragon, Forrest concludes that the goddess as dragon was a personification of the earth!¹⁴⁸⁷ Such an interpretation is wholly unwarranted, however, for the Sumerian texts leave little room for doubt

¹⁴⁸²A. Sjöberg, "in-nin sa-sur-ra A Hymn to the Goddess Inanna by the en-Priestess Enheduanna," *Zeitschrift für Assyriologie* 65 (May 1976), p. 197.

¹⁴⁸³Thus, Edzard notes that the astral aspect of Inanna/Ishtar is frequently expressed together with the warlike aspect of the goddess. See D. O. Edzard, "Mesopotamien: Die Mythologie der Sumerer und Akkader," in *Wörterbuch der Mythologie*, ed. by H. Haussig (Stuttgart, 1962), p. 85. See also the discussion in H. Balz-Cochois, *Inanna* (Gutersloh, 1992), p. 46.

¹⁴⁸⁴See the discussion in R. Harris, "Inanna-Ishtar as Paradox and a Coincidence of Opposites," *History of Religions* 30:3 (Feb. 1991), pp. 261-262, where these opinions and others are collected.

¹⁴⁸⁵W. Hallo & J. van Dijk, *The Exaltation of Inanna* (New Haven, 1968), p. 15.

¹⁴⁸⁶B. Forrest, *Velikovsky's Sources* (1983), p. 564.

¹⁴⁸⁷*Ibid.*, Vol. 3, p. 227.

that it is a celestial body (Venus) that is the subject of the imagery in question. Consider the following passage from a Sumerian temple-hymn collected by Sjöberg and Bergmann: "Inanna . . . the great dragon who speaks inimical words to the evil, . . . Through her the firmament is made beautiful in the evening."¹⁴⁸⁸

In striking contrast to conventional interpretations of the goddess, which must posit an assimilation of different goddesses to explain Inanna/Ishtar's many seemingly unrelated attributes and functions in myth and cult¹⁴⁸⁹ the interpretation offered here – whereby Inanna is understood as a planet Venus) which once presented a comet-like appearance – can account for each and every aspect of her cult. And while it would be impossible to defend this statement here, a few observations are in order to illustrate the explanatory power of the thesis.

Consider, for example, the fact that Inanna/Ishtar is frequently represented as a winged being in ancient iconography,¹⁴⁹⁰ just as in numerous myths she is described as flying about the skies or flying out to do battle.¹⁴⁹¹ Here, too, there is a certain parallel in Mesoamerican sources, where the planet Venus was represented as a winged orb.¹⁴⁹² In ancient Babylonian astronomical texts, however, the phrase "winged star" signifies a "comet."¹⁴⁹³

Also relevant here are the ancient myths which describe Ishtar/Venus as falling from heaven. As several scholars have recognized, Ishtar is indistinguishable from Lamastu, a witch-like goddess renowned for her monstrous form and ogre-like behavior.¹⁴⁹⁴ According to ancient tradition, Lamastu was thrown from heaven by Anu, whereupon she sprouted disheveled hair. An old Assyrian incantation alludes to this theme:

"She is a haunt, she is malicious, Offspring of a god, daughter of Anu. For her malevolent will, her base counsel, Anu her father dashed her down from heaven to earth, For her malevolent will, her inflammatory counsel. Her hair is askew, her loincloth is torn away."¹⁴⁹⁵

The image of Ishtar-Lamastu being hurled from heaven with wildly disheveled hair once again recalls cometary imagery, comets having long been compared to women with long or disheveled hair. And, in lieu of the fact that scholars have cited Lamastu as a prototype of the witch, it is relevant to note that Venus itself was described as the "witch-star" in ancient Babylonian astronomical texts.¹⁴⁹⁶ Nor is this the only land in which Venus was compared to a witch.¹⁴⁹⁷

¹⁴⁸⁸A. Sjöberg & E. Bergmann, *The Collection of the Sumerian Temple Hymns* (Locust Valley, 1969), p. 36.

¹⁴⁸⁹Thus Jacobsen can speak of Inanna as "infinite variety," musing that she has so many different aspects that "one is inclined to wonder whether several, originally different deities have not here coalesced in one, the many faceted goddess Inanna." T. Jacobsen, *Treasures of Darkness* (New Haven, 1976), p. 135.

¹⁴⁹⁰M. Barrelet, "Les deesses armées et ailees," *Syria* 32 (1955), pp. 222-260.

¹⁴⁹¹See the discussion in W. Hallo & J. van Dijk, *The Exaltation of Inanna* (New Haven, 1968), p. 51.

¹⁴⁹²A. Aveni, *op. cit.*, pp. 23-25.

¹⁴⁹³P. Gossman, *op. cit.*, p. 68.

¹⁴⁹⁴W. Fauth, "Ishtar als löwingottin und die löwenkopfige Lamastu," *Die Welt des Orients* 12 (1981), pp. 33-34. For a general survey of the traditions surrounding this figure, see D. Foxvog & W. Heimpel & A. Kilmer, "Lamma/Lamassu," in *Reallexikon der Assyriologie* Vol. 6 (Berlin, 1980-1983), pp. 446-453.

¹⁴⁹⁵B. Foster, *Before the Muses: An Anthology of Akkadian Literature* (Bethesda, Md., 1993), p. 59.

¹⁴⁹⁶P. Gossman, *Planetarium Babylonicum, op. cit.*, p. 62.

¹⁴⁹⁷Venus was also likened to a witch in Norse lore, for example. See J. Grimm, *Teutonic Mythology* (Gloucester, Eng., 1976), Vol. II, p. 723.

Various other elements of the goddess' cult are best interpreted as cometary in origin, as well. Thus, one of Inanna/Ishtar's most common epithets was *Labbatu*, "lioness."¹⁴⁹⁸ Inanna was invoked as the "lion of heaven," and, as such, she was said to brighten the heavens.¹⁴⁹⁹ Here, as in the guise of the warring torch-star and fire-spewing dragon, the destruction wrought by the raging lioness knew no bounds:

"O splendid lioness of the Igigi-gods, who renders furious gods submissive . . . great is your valor, O valiant Ishtar, Shining torch of heaven and earth, brilliance of all inhabited lands. Furious in irresistible onslaught, hero to the fight, Fiery glow that blazes against the enemy, who wreaks destruction on the fierce, Dancing one, Ishtar . . . Irninitum [an epithet of Ishtar], raging lion, may your heart be calmed."¹⁵⁰⁰

Here it is necessary to ask: What is there about the lion that would make it an appropriate symbol for a planet-goddess located in heaven?

According to our thesis, the answer is not far to seek: It is the lion's mane that would appear to offer the explanation. And inasmuch as Venus was described as a "maned" star by other ancient peoples (the Latin *Iubar*, an early name for Venus,¹⁵⁰¹ likewise signifies the "maned" star), it is only reasonable to ask if the planet-goddess as lioness is simply a symbolic counterpart to the "maned-star" or "long-haired" star of other peoples?

That such is indeed the case is supported by yet another symbol associated with Ishtar, one brought into specific relation with the goddess' lion. Thus, a widespread motive in ancient iconography finds lions being marked with a "hair-star" on their

bodies, various authorities noting of the star that "the motive was a token of possession marking . . . animals [with it] as the property of Ishtar."¹⁵⁰² (See Figure 3 at end) The hair-star, needless to say, in addition to being a prehistoric pictograph found throughout the ancient world, is an archetypal symbol of the comet. Here, too, it stands to reason that if Ishtar/Venus once took on a comet-like appearance, the significance of the hair star is readily understandable. By any other interpretation of the planet-goddess, the "hair-star" must stand as yet another anomaly awaiting explanation. [And then also, there are the Babylonian symbols of Inanna or Venus which look like nothing else than a comet. (See Figure 4 at end) Why would the ancient Babylonians draw Venus-Inanna as a comet if it never gave such an appearance? These symbols are so strikingly that of a comet that it again becomes difficult to accept Forrest's dismissal of such evidence. (G)]

¹⁴⁹⁸W. Lambert, "Labbatu" in E. Ebeling & B. Meissner, eds., *Reallexikon der Assyriologie* Vol. 6 (Berlin, 1980-1983), p. 411.

¹⁴⁹⁹W. Fauth, *op. cit.*, p. 24.

¹⁵⁰⁰B. Foster, *op. cit.*, p. 512.

¹⁵⁰¹J. Sammer, "An Ancient Latin Name for Venus," *Kronos* 6:2 (Winter 1981), p. 61. As Sammer points out, Varro wrote that "this star was called Iubar because it is hairy," expressly comparing it to a lion's mane. See *De lingua latina* VII:76. Also significant is the fact that various Greek and Latin authors used the same word to describe a comet.

¹⁵⁰²E. van Buren, "An Additional Note on the Hair-Whirl," *JNES* IX (1950), p. 55.

FIGURE 1: Aztec Serpent-Comet



FIGURE 2: Bearded Dragon

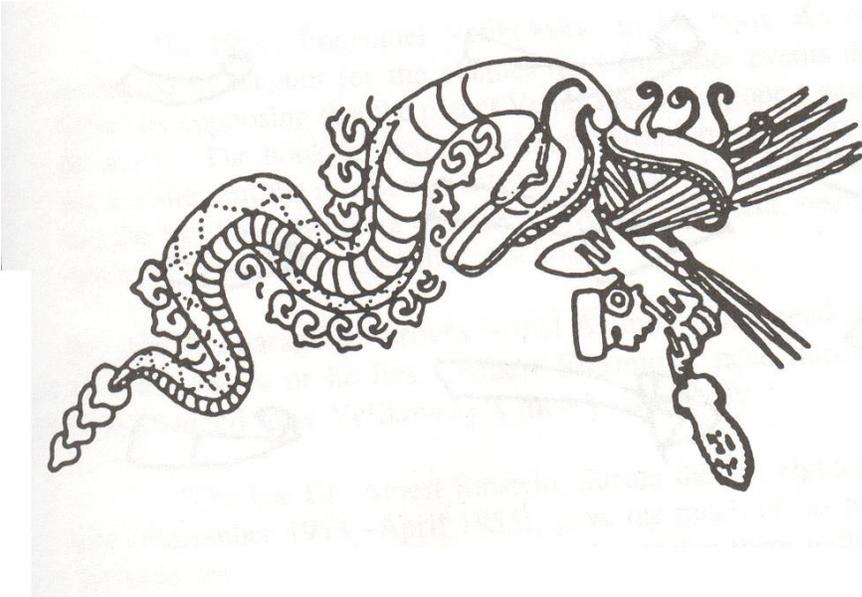


FIGURE 3: Lion With Hair-Star

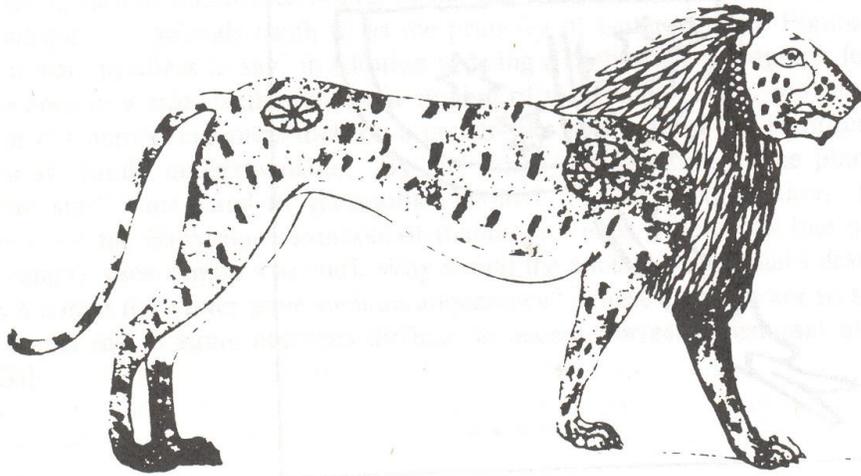
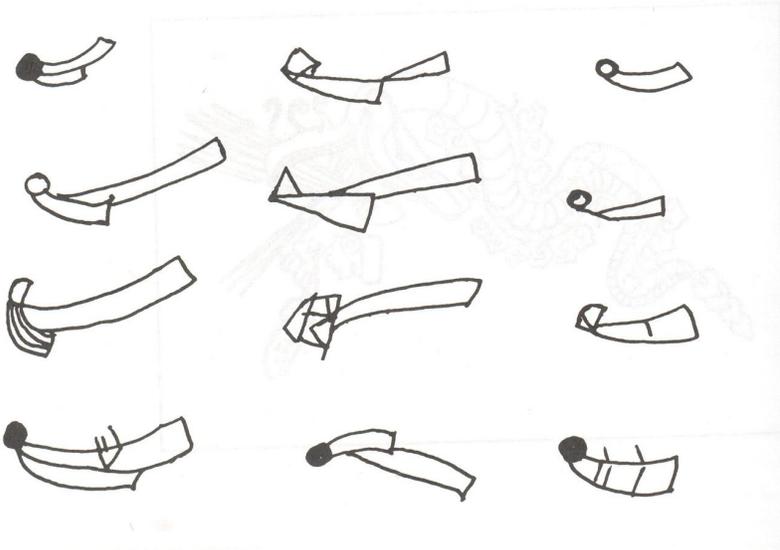


FIGURE 4: Babylonian Symbols of Inanna



ISAAC ASIMOV IN ABSURDITY, By Charles Ginenthal

"Science writers, if they do it well, both inform and entertain, but the task of informing is primary. They must, under no circumstances, misinform. If they do, their work is worthless and harmful—all the more worthless and harmful if it is entertaining and attracts readers."

Isaac Asimov

"Writing of Two Sorts,"

Planets, ed. B. Preiss

(1985), p. 20

Isaac Asimov in 1969, published a diatribe full of rage and invective about Velikovsky's theory titled "Worlds in Confusion" in the October 1969 issue of *Fantasy and Science Fiction*, a pulp magazine. Three years later it was reprinted in his book *Stars in their Courses*. His criticism amounts to a mole hill of barely controlled vituperation. This I discovered to be the nature of his assault which I conceive to be absurdities piled upon absurdities, and hence the title of this essay. Asimov's attacks have little to recommend but his name attached to them. Therefore, on that account, they deserve a reply. He begins by telling the following falsehood.

"In 1950, Immanuel Velikovsky, in his book *Worlds in Collision* attempted to account for the plagues (and for other events described in the Bible) by supposing that the planet Venus had undergone a near collision with the earth. The book created a moderate sensation among the general public for a while, but the reaction of astronomers varied from amusement to anger, and the Velikovskian theory has never, for one moment, been taken seriously either by scientists or by Biblical scholars."¹⁵⁰³

All that this paragraph proves is that Asimov either read nothing of the literature related to these issues or he lies. Albert Einstein, I need hardly remind anyone, *is* a scientist who indeed took Velikovsky's theory "seriously."

"The late Dr. Albert Einstein, during the last eighteen months of his life (November 1953—April 1955), gave me much of his time and thought. He read several of my manuscripts and supplied them with marginal notes. Of *Earth in Upheaval* he read chapters VII through XII; he made handwritten comments on this and other manuscripts and spent not a few long afternoons and evenings, often till midnight, discussing and debating with me the implications of my theories."¹⁵⁰⁴

¹⁵⁰³Isaac Asimov, *The Stars in their Courses*, (New York, 1971) p. 36.

¹⁵⁰⁴Immanuel Velikovsky, "Acknowledgements," *Earth in Upheaval*, (New York, 1955), p. IX.

This information was written 14 years prior to Asimov's attack and which, as we will see, he claims to have read; but in spite of this evidence being available to him he proclaims Velikovsky's theory has never "been taken seriously" by a scientist. What, then, was Einstein if not a scientist? According to Asimov, Einstein was not a scientist. Why? Because he took Velikovskian theory seriously enough to read, to annotate his manuscripts, and discuss and debate these concepts with Velikovsky. This misrepresentation is a common theme that has been expressed by several critics of Velikovsky, including some in this book. Probably none of them, including Asimov, have ever had Einstein pay the slightest attention to their ideas, or read their material, or write commentaries of their concepts, or discussed their theories with them in person late into the night. But Asimov maintains none of this happened! Einstein is considered the crowning glory of scientific achievement of our age, and in spite of Asimov's senseless denial, he took Velikovsky and his theories seriously. Asimov's remarks are, therefore, out and out propaganda. To deny this fact regarding Einstein is sheer absurdity.

Asimov also ignorantly proclaims in that same remark that "the Velikovskian theory has never, for one moment, been taken seriously . . . by Biblical scholars." In 1952 Velikovsky published his historical book which dealt with the *Bible* titled *Ages in Chaos*. One of the Biblical scholars who advised Velikovsky on that manuscript was Dr. Robert H. Pfeiffer. Of this highly respected Biblical scholar Velikovsky wrote:

"I am also indebted to Dr. Robert H. Pfeiffer, outstanding authority on the *Bible*. Director of the Harvard excavation at Nuzi, curator of the Semitic Museum at Harvard University, professor of ancient history at Boston University, editor of the *Journal of Biblical Literature* (1943–1947), and author of a distinguished standard work on the *Old Testament*"1505

Here is what this eminent international scholar of the *Bible* has to say about Velikovsky's thesis. "Dr. Velikovsky discloses immense erudition and extraordinary ingenuity. He writes well and documents all his statements with original sources."1506 Here is what this eminent scholar of the Bible has to say about *Worlds in Collision*.

"Allow me first of all to congratulate you, not of course for the fact that your book has become a 'run-away best seller,' but for magnificent qualities of content and form of your book . . . I was amazed at the depth and vastness of your erudition which I have not seen equaled except possibly in O. Spengler's *Decline of the West*."1507

The information in Velikovsky's book, *Ages in Chaos*, as relates to the *Bible*, was available to Asimov 17 years prior to his attack, and which, as we will see, he claims to have read. But, again, in spite of this information being available, he proclaims Velikovsky's theory has never "been taken seriously" by Biblical scholars. But what was Pfeiffer if not an eminent Biblical scholar? According to Asimov, Pfeiffer was not a Biblical scholar. Why? Because he took Velikovskian theory seriously enough to read and advise Velikovsky on his Biblical-historical manuscript and discussed and analyzed these concepts with Velikovsky, he permitted the above remarks to be clearly displayed on the dust cover of Velikovsky's book without retracting them over the pressure brought to bear on him by Harlow Shapley. Pfeiffer was a world renowned Biblical scholar, slightly more qualified than Asimov to judge such matters, and he took Velikovsky's theory seriously. For Asimov to then deny this fact is not only mendacious but absurd.

To add further to Asimov's problems, he then condemns himself by writing that he had read this material about Einstein and Pfeiffer:

¹⁵⁰⁵Immanuel Velikovsky, "Acknowledgments," *Ages in Chaos*, (New York, 1952), p. XIII.

¹⁵⁰⁶*Ibid.* Jacket blurb, (back cover).

¹⁵⁰⁷Immanuel Velikovsky, *Stargazers & Gravediggers* (New York, 1983), p. 207.

" . . . *some* non-scientist scholars have been lionizing Velikovsky. It's not surprising that the most vehement letter I received was from an English teacher.

"I don't want to beat a dead horse, but some of my correspondents self-righteously demand that I *read* Velikovsky before I denounce him. The implication is that if I only read him the truth of what he has to say will be borne irresistibly down upon me.

"But, as it happens, I *have* read him, and I remain untouched."¹⁵⁰⁸

Apparently Pfeiffer fits Asimov's description of "non-scientist scholars" when he lionized Velikovsky by writing "I was amazed at the depth and vastness of your erudition which I have not seen equaled except possibly in O. Spengler's *Decline of the West*"¹⁵⁰⁹

In essence, Asimov goes out of his way to claim he read Velikovsky's books. Thus he read about Einstein and Pfeiffer's serious considerations of Velikovsky's theory but also claims he is untouched by this evidence which makes his statements absurd. I suggest that no evidence on this matter could have ever seeped through Asimov's rage which he exhibits by denying these facts. One does not have to be a "scholar" of any kind to see through the transparent deceit Asimov has presented. This is simply double-entry bookkeeping. After reading the information in Velikovsky's books, in his own book Asimov acts as if the information does not exist.

But to attack Velikovsky's use of science Asimov states:

"Thus, at the very beginning of the book, Velikovsky describes various theories as to the origin of the solar system and the development of the Earth. He stresses the shortcomings and insufficiencies of these theories, naturally, for he plans to advance a far better one himself. He says on page 11:

"'According to all existing theories, the angular velocity of a satellite must be slower than the velocity of rotation of its parent. But the inner satellite of Mars revolves more rapidly than Mars rotates.'

"That's a very pretty paragraph but its quite wrong, and it shows that as an astronomer, Velikovsky may quite possibly be an excellent psychologist.

"There is absolutely nothing in any astronomic theory I have ever heard that relates the angular velocity of a satellite to the period of rotation of the planet it circles. Nothing requires that a satellite revolve about its planet either faster or slower than the planet's period of rotation.

"The angular velocity of a satellite depends on two things and *only* two things: The masses of its primary and itself, and the distance between the two bodies. If the primary is much larger than the satellite (as is usually the case), the mass of the satellite can be ignored.

"The closer a satellite is to its primary, the more rapidly it moves in its orbit. If it is close enough to its primary, it will revolve about that primary in exactly the same time that the primary rotates, and if it moves still closer, then it will revolve about the primary in 'less time than it takes the primary to rotate.'

"'The inner satellite of Mars revolves more rapidly than Mars rotates'(to quote Velikovsky again) only because it is close enough to Mars to do so. At the distance of that satellite to Mars, it *can't* revolve any more slowly if Newton's law of gravity is to be obeyed. Far from denying 'all existing theories' by revolving so quickly, the inner satellite would defy them if it did *not* revolve so quickly."¹⁵¹⁰

¹⁵⁰⁸Asimov, *op. cit.*, pp. 37-38.

¹⁵⁰⁹*Stargazers, loc. cit.*

¹⁵¹⁰Asimov, *op. cit.*, pp. 38-39.

That statement is a pretty analysis but it is wrong (to quote Asimov) and all it shows is that as an astronomer, Asimov is an excellent writer of science fiction. Now why is that so? The answer has to do with the birth of the satellites of the planets based on the 'existing theories' of solar system formation and based strictly on gravitational theory which Velikovsky was discussing. Here is a general description and analysis of how the theory actually works. We start with the condition

"That the solar system originally consisted of a vast disc-shaped mass of gas at high temperature which was rotating slowly in the direction in which the planets now revolve in their orbits. As this mass of gas lost heat by radiation into space, it cooled. As the mass cooled it also shrank; owing to this shrinking it had to rotate faster. This effect is seen when a skater spinning on one foot with his arms extended wraps them around his body. When he moves his arms in, he spins more rapidly. That is, a spinning body has a certain quantity of rotation (*angular momentum*, as it is called in technical language) As the rotating mass of gas continues to shrink and rotate faster there came a time when the centrifugal force at its edge became equal to the force of gravitation acting inward, and when this happened a ring of matter was left behind at the [sun's] equator. The mass continued to contract until eventually another ring was left off, and another and another, as many rings being sloughed off as there are planets in the solar system. These rings would hardly be the same size all the way around. One part [of the ring] would be larger than the rest and this would gradually attract the remainder of the ring to it. This mass of gas formed from the rings is the material from which a planet condenses. The planetary nebula would itself shrink also, leaving rings which condensed into satellites."¹⁵¹¹

What we have just been told is that as the gases and dust material fall inward toward the center of the condensing mass, during planetary or satellite formation, like a spinning skater moving his arms inward, the mass closer to the center spins or rotates more rapidly than the gas and material farther from the center. Based on gravity the greater the distance inward these materials fall the faster that region spins or rotates. Therefore, the central body, such as the sun, born from the greatest amount of material which fell the greatest distance inward rotates in about 26 days, Mercury made up of the lesser material that fell inward, but not as far as the sun, rotates more slowly around the sun and its center in 88 days. Venus, made up of material that fell inward, but not as far as Mercury, rotates even more slowly around the sun and its center in 225 days. And as we go outward, the planets farther from that center rotate around it ever more slowly. That is what the formation theories based on gravity demand. It is a rule.

There is absolutely nothing in any astronomic theory based on gravity for the formation of planets and for satellites such as the inner satellite of Mars that I have ever heard of that does not relate the angular velocity of a satellite to the distance its materials fall inward to the primary it circles. Everything based on gravitational theory under these conditions requires that a satellite born this way must revolve about its planet slower than the planet rotates, which must inevitably rotate fastest because the amount of materials that it is made up of fell inward the greatest distance and gained the greatest velocity.

The angular velocity of a satellite based on this condition depends on two things and *only* two things: the amount of material in a nebula and the distance that material falls. The farther inward the material of a nebula falls, the more rapidly the body from which it condenses must rotate about the nebula center, and the more material that falls inward, the more rapidly the body from which it condenses must rotate about the center.

The material closest to the center which fell the greatest distance inward and contains most of the matter, forms the primary which rotates the fastest. "The inner satellite of Mars moves more rapidly than Mars rotates [to quote Velikovsky]," and therefore violates this fundamental gravitational process. The distance of that satellite to Mars means material from which it was formed fell inward to make up Mars and its satellite. Therefore, Mars, by this process, must be made of more material which fell inward farther causing it to revolve more rapidly than its satellites, if Newton's law

¹⁵¹¹Robert S. Richardson, *The Fascinating World of Astronomy*, (New York, 1960) pp. 190-191.

of gravity is to be obeyed. Far from obeying "all existing theories," the inner satellite of Mars is defying them by revolving around the center so quickly.

The only explanation for this and the only way to avoid defying the laws of gravity is for this satellite of Mars to have been born by a totally different process. The theory of formation by the process of condensation from a sphere of gas and dust would never permit the outer rings of a satellite to rotate around the center faster than its planet.

If the reader does not believe this is a basic rule of planetary and satellite formation, I refer the reader to Professor of Science Willy Ley, of Fairleigh Dickinson University, who stated in 1963 and again in 1966, years before Asimov raised this attack:

"The moons of Mars are interesting for two reasons. The first is that they were 'guessed' repeatedly before they were discovered. The second is that after their discovery it was found that a 'natural law,' which had been silently accepted by everyone, did not hold true in all cases. That 'law' was the statement that the period of revolution of a secondary must always be greater than the period of its primary; no planet could orbit the sun in a shorter time than the time the sun needed to turn on its axis; likewise, no moon could orbit its planet in a shorter period than the planet's rotation. But the inner moon of Mars has a period of revolution shorter than the Martian day."¹⁵¹²

Therefore, Ley exposes Asimov's absurd view and explains, "Chiefly because Phobos violates the rule about the relationship between the primary's rotation and the secondary's revolution, it was suggested that they [Deimos and Phobos] might be captured asteroids."¹⁵¹³ The reader should note that Ley calls the concept that Velikovsky had presented a 'law,' which makes it painfully clear that Velikovsky's statement on this matter is correct and Asimov was in serious error. And Ley specifically pointed out that this problem was known since these moons were discovered by Asaph Hall in 1877, over a hundred years before Asimov wrote this criticism. The fact that Asimov either did not understand this or willfully ignored it exhibits either gross ignorance of fundamental planetary formation theory, or a callous disregard for the truth. His effort to dismiss Velikovsky on the basis of such a dismal analysis is an absurdity. What theory of solar system formation explains how material falling inward would cause the most material that fell the greatest distance inward to form the primary and would then cause the lesser amount of material that fell a shorter distance inward to form the satellite would ever create a satellite going around its primary more rapidly than it rotates? To suggest this is sheer Fantasy and Science Fiction is something Asimov seems to be a master of

doing as he has in this case!

To show that Velikovsky couldn't be trusted to handle even simple data, Asimov then writes:

"The entire corpus of humanity's myth and legend yields sentences on every side of every question and sometimes one of them must be hammered a bit to make it fit. Velikovsky talks about Atlantis, for instance, on page 147, saying: 'Critias' the younger remembered having been told that the catastrophe which befell Atlantis happened 9,000 years before. 'There is one zero too many here.'

"So he [Velikovsky] removes it. What's a zero? Velikovsky makes the Atlantis catastrophe nine hundred years before Critias and now it fits his own chronology.

"Gentle Reader, place all the myths and legends of the human race at my disposal; give me leave to choose those which I want to use and allow me to make changes where necessary; and I will undertake to prove anything you wish proven."¹⁵¹⁴

¹⁵¹²Willy Ley, *Watchers of the Sky*, (New York, 1963 and 1966), p. 212.

¹⁵¹³*Ibid.*, p. 216.

¹⁵¹⁴Asimov, *op. cit.*, pp. 40-41.

All this shows is that Asimov has not done his homework because one of the major researchers of this topic over the years, Angelos Galanopoulos, has also equated the *Exodus* with the sinking of Atlantis in a work titled "Die ägyptischen Plagen und der Auszug Israels aus geologischer Sicht," translated means "The Egyptian Plagues and the Israelite *Exodus* from a Geological Viewpoint." This work was published in a respected journal on ancient history five years before Asimov attacked Velikovsky on this very point!¹⁵¹⁵ As Carl Sagan wrote about Galanopoulos:

"In a certain sense, the Galanopoulos explanation of the events in *Exodus* is more provocative than the Velikovsky explanation, because Galanopoulos has presented moderately convincing evidence that Thera corresponds in almost all essential details to the legendary civilization of Atlantis. If he is right, it is the destruction of Atlantis rather than the apparition of a comet that permitted the Israelites to leave Egypt."¹⁵¹⁶

In this book, Asimov has written the "Foreword" and, therefore, *had read* Sagan's statement. On the basis of this reading, *he had to know* that Galanopoulos equated the period of the *Exodus* with the legend of the destruction of Atlantis. But knowing of this equation, Asimov never retracted his accusation. Let us examine this further.

Of course, both Atlantis and Thera could have been destroyed at the same time by a giant comet-Earth interaction. But clearly Galanopoulos is equating the period of the destruction of Atlantis with the Israelite *Exodus*. Thus, we then come to the 9,000 and 900 year difference in the time scale. How could Galanopoulos turn a 9,000 year old event into one 900 years old without removing "one zero too many?" In fact, by analyzing the forms of numeration of ancient Minoan history in Cretan Linear script, Galanopoulos showed that this made excellent sense. "He found that all numerical references in the thousands seemed to be implausible, while those involving sums under 1000 were always entirely reasonable."¹⁵¹⁷

J. Mavor just cited showed that the symbol for 100 in Cretan Linear script of ancient times is a circle, (O), while 1,000 is a circle with lines emanating outward up and down, and right and left (O). When Velikovsky made his proposal he was making the very same one a noted scholar would make on this very point. Does Asimov attack Galanopoulos by saying, "So he removes it. What's a zero? Galanopoulos makes the Atlantis catastrophe nine hundred years before Critias and now it fits his chronology"?

Of course, Asimov does not attack him! To do so would expose the nature of Asimov's standards of criticism. Thoughtful Reader, place all the evidence on this issue at Asimov's disposal; give him leave to choose that research which he read about and the research he wants to use, and allow him to attack Velikovsky and ignore Galanopoulos, whom Asimov cited in Sagan's article. What one gleans is absurdity, because he never retracted his statement after citing Galanopoulos' research! Nor did he ever attack Galanopoulos for removing a zero and placing the *Exodus* at the time of the destruction of Atlantis!

Like his reading of Einstein and Pfeiffer, which does not touch him, even Sagan's words could not penetrate. When you're out to get Velikovsky, what impact does a reference by Sagan or Galanopoulos to *Exodus* and Atlantis occurring at the same time matter? Asimov makes the Atlantis catastrophe nine thousand years before Critias, and now it conforms to his own chronology and his attack on Velikovsky.

But let us continue with more of Asimov's absurdities. He argues:

" . . . let's not think of such gigantic catastrophes. Let's not think of altered orbits, of oceans leaving their beds and slopping over the continents. Let's not think of the *great* results of Earth's suddenly stopping its rotation when Joshua commanded the Sun to stand still (Not only would Joshua's

¹⁵¹⁵Das Alterter, Vol. 10, (1964), pages 131-137.

¹⁵¹⁶Carl Sagan, "An Analysis of Worlds in Collision," *Scientists Confront Velikovsky* (Ithaca, N.Y., 1977), p. 90.

¹⁵¹⁷James W. Mavor, Jr., *Voyage to Atlantis*, (Rochester, Vt., 1990), p. 21.

soldiers all have fallen down and rolled for a thousand miles, but the energy of rotation would have been converted into heat and have melted the Earth's crust.)"¹⁵¹⁸

This is the same argument employed by Dr. Cecilia Payne-Gaposchkin back in the 1950's who originally wrote in her privately distributed paper:

"If the biblical story which . . . Velikovsky seeks to establish is to be accepted at its face value, the rotation of the earth must have been stopped within six hours. All bodies not attached to the surface of the earth (including the atmosphere and the ocean) would then have continued their motion, and consequently have flown off with a speed of 900 miles an hour at the latitude of Egypt."¹⁵¹⁹

Payne-Gaposchkin, an astronomer, when she wrote this attack made a very stupid error and Asimov has followed her into this error. As de Grazia aptly pointed out in 1966, years before Asimov raised this criticism:

"If the earth, as she says first, decelerated within six hours, the inertial push on objects on the earth's surface would be 500 times smaller than their weight. A man of 160 lbs. would experience a forward push of 5 ounces. Dr. Gaposchkin now had a clear choice: someone had called the quantitative error to her attention. She might choose to recalculate the inertia of the slower stop, or misquoting Velikovsky, imply that he reported an instant stop."¹⁵²⁰

Now what did Payne-Gaposchkin do when she learned about her thoughtless error? Of course, she still wanted to get Velikovsky, so all she did was bite down and swallow hard and attack him by leaving out the matter of time, stating in her publicly published paper:

"Let us assume, however, that Dr. Velikovsky is right—that the earth did stop rotating. In that case, all bodies not attached to the surface of the earth (including the atmosphere and the ocean) would have continued their motion, and would have flown off with a speed of nine hundred miles an hour at the latitude of Egypt."¹⁵²¹

All of this information was written well before Asimov raised this attack on Velikovsky. Did he pay the slightest attention to evidence that contradicted his criticism? No! Now, if the readers do not believe me or de Grazia on this point, perhaps they will believe Carl Sagan who wrote again in the book in which Asimov wrote the "Forward":

"Much of the indignation directed toward *Worlds in Collision* seems to have arisen from Velikovsky's interpretation of the story of Joshua and related legends as implying that the Earth's rotation was once braked to a halt. The image that the most outraged protesters seem to have had in mind is that shown in the movie version of H. G. Well's story, 'The Man Who Could Work Miracles': the Earth is miraculously stopped from rotating but through an oversight, no provision is made for all objects not nailed down, which then continue moving at their usual rate and, therefore,

¹⁵¹⁸Asimov, *op. cit.*, p. 42.

¹⁵¹⁹Alfred de Grazia, "Scientific Reception," *The Velikovsky Affair*, (New York, 1966), p. 231.

¹⁵²⁰*Ibid.*

¹⁵²¹*Ibid.*

fly off the Earth at a speed of a thousand miles per hour. But it is easy to see [in Sagan's Appendix 2] that a gradual deceleration of the Earth's rotation at 10^{-2} g or so could occur in a period of much less than a day. Then no one would fly off Likewise, we see in Appendix 2 that the energy required to brake the Earth is not enough to melt it"¹⁵²²

Therefore, none of "Joshua's soldiers (to quote Asimov) would have fallen down and rolled for a thousand miles," nor would "the energy of rotation . . . have been converted into heat and have melted the Earth's crust," underlying Asimov's argument of "let's not think of such gigantic catastrophes. Let's not think of altered orbits Let's not think of the *great* results of Earth's suddenly stopping" The real message Asimov is conveying is "LETS NOT THINK!"

Because if one thinks about the time constraint for the supposed slowing of the Earth's rotation, the forces of a gentle deceleration would be 500 times smaller than the mass of anything on the Earth. If one thinks about 1/500th of the force placed on the average man, he would feel the gentle nudge of a few ounces of push. If one thinks about a few ounces of push knocking down full grown men and causing them to roll for a thousand miles, or the Earth's crust melting based on Asimov and Payne-Gaposchkin's thinking, one realizes that the entire argument is a colossal, thoughtless absurdity. All this was presented before Asimov wrote his attack and read Sagan's work, but he never retracted this criticism. This is fundamental physics; but what does physics mean to Asimov? Nothing!

Asimov next goes on to attack:

"But let us move on. Velikovsky needs a rain of burning fire to explain certain Biblical allusions and he finds a great deal of talk about such combustive events in his myths.

"You and I might suppose that the experience of a volcanic eruption is terrifying enough to account for such tales and can easily be magnified to a whole sky on fire, given the inevitability of poetic license. Velikovsky, however, does not believe in either poetry or metaphor. He wants a literal rain of fire and he uses comet Venus to explain it.

"On page 53, he says: 'The tails of comets are composed mainly of carbon and hydrogen gases. Lacking oxygen, they do not burn in flight but the inflammable gases passing through an atmosphere containing oxygen will be set on fire.'

"These are impressive sentences. The very phrase 'carbon and hydrogen gases' takes my breath away. Hydrogen is, indeed, a gas at ordinary cometary temperatures, but carbon is *not*. It is, in fact, among the least gaseous substances known and it takes a temperature of 4200° C (7500° F.) to make it gaseous.

"Now I am a chemist. If Velikovsky wants to say that Laplace's analysis of celestial mechanics is all wrong and that Venus can emerge from Jupiter and settle down in its present orbit, I will smile.

"But if he says carbon is a gas, *that's going too far*."¹⁵²³

But Velikovsky did not say "carbon is a gas." He said "carbon and hydrogen gases."

If only Asimov, who claims to have read *Worlds in Collision*, had not ignored Velikovsky's other statements in that book regarding the gases of Venus, he would have withdrawn this criticism because on pages 323-324, Velikovsky explicitly stated he was talking about hydrocarbons or petroleum gases, which are composed of hydrogen and carbon together. There Velikovsky specifically said, "On the basis of this [book's] research, I assume that Venus may disclose

¹⁵²²Sagan, *op. cit.*, pp. 63-64.

¹⁵²³Asimov, *op. cit.*, p. 43.

the presence of *hydrocarbon . . . gases* in its atmosphere Venus has petroleum gases." (Emphasis added). Here Velikovsky emphatically and implicitly clarified his earlier statement that the hydrogen and carbon gases in Venus' atmosphere which he predicted would be hydrocarbons.

To add further to this point, Dr. W. T. Plummer wrote in the journal *Science*, (March 14, 1969), p. 1191, "Velikovsky [predicted] that Venus should be surrounded by a blanket of petroleum hydrocarbons." Plummer, a scientist, published this fact for all the world to see in 1969, two years before Asimov published his attack in *Stars in Their Courses*. Neither Plummer nor the editors of the journal *Science* were ignorant; they correctly understood that Velikovsky was speaking of hydrocarbon gases and not, as Asimov suggests, carbon gases. Carbon does join with hydrogen to form hydrocarbons such as methane, which is the gas used in cooking on home ranges and ovens. Methane is made up of one carbon atom attached to four atoms of hydrogen. There are thousands of hydrocarbons which form gases. In fact, Carl Sagan, in his popular book *Comet*, pages 149–151, shows methane and several other hydrocarbons like acetylene are found in comets.¹⁵²⁴ And Asimov knew this all along. Realizing it was he that was "going too far," Asimov niggardly admits as much.

"Let's not be too hard though. As a matter of fact, the tails of comets seem to be made up, at least in part, of molecular fragments, some of which contain both carbon and hydrogen and are, therefore, 'hydrocarbon' in nature. It may be that this is what Velikovsky had in mind when he spoke of 'carbon and hydrogen gases.'"

There is no question that Velikovsky "may have," (to quote Asimov), "had hydrocarbons in mind." Velikovsky specifically stated that he had hydrocarbons in mind! But what do Velikovsky's precise statements mean to Asimov? When you're out to get Velikovsky, one may ignore, with total indifference, the statements made by Velikovsky in order to discredit him. It is not Velikovsky who has "gone too far," but Asimov who has gone to absurd lengths to pull this stunt.

Having thrown a tantrum over the "carbon and hydrogen gases" concept and realizing his error, Asimov shamefacedly admits Velikovsky had hydrocarbons in mind. Unwilling to accept his own misrepresentation and desperate to attack Velikovsky in some way, Asimov then goes on to charge,

"To be sure, this chemical analysis of comets' tails is the result of some very esoteric and sophisticated astronomical theories, and you might wonder how Velikovsky can come to accept them. After all, if astronomers are so far wrong on the simplest tenets of celestial mechanics, can they be trusted in the delicate nuances of spectroscopy? But then, the astronomical decision with regard to the chemical structure of comets' tails suits Velikovsky's theory, so he accepts it.

"(Gentle Reader, give me the chance to pick and choose among the findings of science, accepting this and rejecting that according to my lordly whim, and I will undertake to prove anything you wish proven.)"¹⁵²⁵

But this is precisely what Charles Darwin did to establish his theory of evolution. He required that the fossil record show innumerable lineages which he needed even though there were few such lineages discovered. So he *chose* to say that they did exist, and, in time, they would be found. He required that the age of the Earth be extremely old based on erosion evidence when Lord Kelvin claimed it was young based on thermodynamic physics. Darwin then *chose* his views of erosion over Lord Kelvin's physics. Darwin needed breeding experiments to show that evolution changed one species into another in spite of the fact that breeders could not do this and claimed it could not be done. But Darwin *chose* to say that it could be done. Darwin also required that changes in heredity were preserved and became new

¹⁵²⁴Carl Sagan, Ann Druyan, *Comet* (New York, 1985), pp. 149–151.

¹⁵²⁵Asimov, *op. cit.*, pp. 43–44.

characteristics in future generations in spite of the interpretation in the science of his time that heredity characteristics were lost through blending with the heredity characteristics within the species. But Darwin *chose* to accept that this blending did not occur and the new characteristics would survive. If Darwin had not *chosen* these concepts as opposed to others of greater scientific standing, he would never have produced his theory of evolution. Scientists are always choosing evidence in some cases and rejecting it in others. But if Darwin had only had the great insight of Asimov we would be without his theory today. (See my discussion of this in greater depth in my essay on Stephen Jay Gould.)

But the point Asimov is driving at is finally stated:

"But granted the hydrocarbons, can cometary tails really blaze up if they pass through Earth's atmosphere? Can they really cause rains of fire? No, sir, not a chance.

"Those comet tails are just about the thinnest gas you can imagine. Some tails have extended outward through space for a hundred million miles, but if all that glowing next-to-nothingness were condensed to the thickness of ordinary gases in our own atmosphere, they would perhaps fill a room or two.

"You know what happens when the Earth passes through a comet's tail? Nothing!

"How do I know? Because the Earth passed through one on a number of occasions. It passed through the tail of Halley's Comet in 1910. Many people refused to believe the scientific Establishment who said nothing would happen. They thought the end of the world would come. Or they thought the poisons in the comet's tails (they believed the spectroscopic analysis) would kill all life on Earth.

"And what happened? As I told you, Nothing!"¹⁵²⁶

Asimov again, realizing his explanation did not fit the situation, niggardly admitted:

"Of course, comet Venus was much huger than an ordinary comet. The ordinary comet has the mass of a small asteroid. Comet Venus had four-fifths the mass of the Earth itself. Its 'tail' must have been much more voluminous than that of an ordinary comet. Could it be that comet Venus' atmosphere did ignite on passing through the Earth and did set up rains of fire?"¹⁵²⁷

Therefore, Asimov admits he does not know whether or not an immense body with a great deal of hydrocarbon gas, such as proto-planet Venus, if it passed close to the Earth, and the immensely greater volume of materials following it, could cause fire to rain down from the sky.

Over 200 years ago the scientific establishment was claiming that rocks do not fall from the sky and attacked anyone who chose to accept the eyewitness accounts which claimed that rocks did indeed fall from the sky. Because the scientific Establishment was wrong on this account certainly does not prove Asimov is wrong in rejecting rains of fire from comets from the sky. But I say fire can fall from the sky and quite probably from comets. How do I know? Because it was observed and reported and fully described in the last century!

In the last century, Biela's Comet broke into pieces and the comet and its pieces came very close to the Earth. At that time, in the American north central states, fire fell from the sky! This was observed and well reported.

¹⁵²⁶Asimov, *op. cit.* p. 44.

¹⁵²⁷*Ibid.*

"In the year 1871, on Sunday, the 8th of October, at half past nine o'clock in the evening, events occurred which attracted the attention of the whole world, which caused the death of hundreds of human beings, . . . and which involved three different States of the Union in the wildest alarm and terror.

"The summer of 1871 had been excessively dry; the moisture seemed to be evaporated out of the air; and on the Sunday above named the atmospheric conditions all through the Northwest were of a most peculiar character. The writer [Donnelly] was living at the time in Minnesota, hundreds of miles from the scene of the disasters, and he can never forget the condition of things. There was a parched, combustible, inflammable, furnace-like feeling in the air. It felt as if there were needed but a match, a spark, to cause a worldwide explosion

"At that hour, half past nine o'clock in the evening, *at apparently the same moment*, at points hundreds of miles apart, in three different States, Wisconsin, Michigan, and Illinois, fires of the most peculiar and devastating kind broke out, so far as we know, by spontaneous combustion.

"In Wisconsin, on its eastern borders, in a heavily timbered country, near Lake Michigan, a region embracing *four hundred square miles*, extending north from Brown County, and containing Peshtigo, Manistee, Holland, and numerous villages on the shores of Green Bay, were swept bare by an absolute whirlwind of flame. There were *seven hundred and fifty people killed outright*, besides great numbers of the wounded, maimed and burned, who died afterward. More than three million dollars worth of property was destroyed.

"It was no ordinary fire."¹⁵²⁸ (Donnelly's emphasis)

It is believed that pieces of Biela's Comet, which had broken apart and split into two major pieces, and probably many smaller ones, was in the vicinity of the Earth at that time. It is further proposed that some of these smaller pieces of the comet entered the Earth's atmosphere and created the catastrophes described in a report of these events titled, *History of the Great Conflagration*, (Chicago, 1871).

According to this report:

"At sundown there was a lull in the wind and comparative stillness. For two hours there were no signs of danger; but at a few minutes after nine o'clock, and by singular coincidence, *precisely the time at which the [great] Chicago fire commenced*, the people of the village *heard a terrible roar*. It was that of a tornado, crushing through the forests. *Instantly the heavens were illuminated with a terrible glare. The sky, which had been so dark a moment before, burst into clouds of flame.* A spectator of the terrible scene says the fire did not come upon them gradually from burning trees and other objects to the windward, but the first notice they had of it was *a whirlwind of flame in great clouds from ABOVE THE TOPS OF THE TREES*, which fell upon and entirely enveloped everything. The poor people inhaled it, . . . intensely hot air, and *fell down dead*. This is verified by the appearance of many corpses. *They were found dead in the roads and open spaces, where there were no visible marks of fire nearby, with not a trace of burning upon their bodies or clothing.* At the Sugar Bush, which is an *extended clearing*, in some places four miles in width, corpses were found in the open road, between fences only slightly burned. *No mark of the fire was upon them; they lay there as if asleep.* This phenomenon seems to explain the fact that so many were killed in compact masses. They seemed to have huddled together, in what were evidently regarded at the moment as the safest places, *far away from buildings, trees, or other flammable material* and there to have died together." (Emphasis and capitalization added)¹⁵²⁹

¹⁵²⁸Ignatius Donnelly, *Ragnarok: The Age of Fire and Gravel*, (University Books reprint), (New York, 1970), pp. 413–414.

¹⁵²⁹James W. Sheahan, George P. Upton, *History of the Great Conflagration*, (Chicago, 1871), p. 372.

According to one individual:

"Much has been said of the intense heat of the fires which destroyed Peshtigo, Menekaune, Williamsonville, etc., but all that has been said can give the stranger but a faint conception of the reality. The heat has been compared to that engendered by a flame concentrated on an object by a blow pipe; but even that would not account for some of the phenomena. For instance, we have in our possession a copper cent taken from the pocket of a dead man in the Peshtigo Sugar Bush, [field] which will illustrate our point. *This cent has been partially fused*, but still retains its round form, and the inscription upon it is legible. Others in the same pocket, were partially *melted*, and yet *the clothing and the body of the man were not even singed*. We do not know in what way to account for this, unless, as is asserted by some, the tornado and fire were accompanied by electrical phenomena.¹⁵³⁰

In essence, then, we have all the phenomena associated with fire from the sky described in the old chronicles. The fire came from the sky with a tremendous blast of noise, frightening and overwhelming the people in stark terror. Driving them into the fields to escape the fire consuming all the trees and buildings, the frightened people came together to huddle in the last soul wrenching moments before the noise of the blast and fire from the sky overwhelmed them.

What would such an event played out upon whole regions of the planet have induced in the minds of those who survived this catastrophe? Those who experienced the fire in the last century said:

"The prevailing idea among the people was that the last day of the world had come. They were used to fires, but they had seen nothing like this in their lives. Their only interpretation of the terrifying roaring, the bursting of the sky with flame and the dropping down of fire from the heavens, consuming everything it touched, was that the Last Trump[et] had sounded and the Day of Judgement was upon them.

"No two people gave an exactly similar description of the great tornado as it struck and devoured the village. One said that it seemed as though 'the fiery fiends of hell had been loosened.' Another remarked, 'It came in great sheeted flames from heaven.' People spoke of 'a pitiless rain of fire and sand' of the atmosphere 'all afire,' of 'great balls of fire unrolling and shooting forth in streams.' The fire leaped over roofs and trees and ignited whole streets at once. No one could stand before the blast."¹⁵³¹

One survivor described the panic of the animals:

". . . for two hours before the fire reached him there was a constant flight across the ground of small animals . . . [Then] a horse dashed into the opening at full speed and made for the house . . . [He] could see him tremble and shake with excitement and terror, and felt a pity for him. After a moment, the animal gave utterance to a snort of dismay, ran two or three times around the house, and then shot off into the woods . . . "¹⁵³²

¹⁵³⁰*Ibid.*, p. 372.

¹⁵³¹Mel Waskin, *Mrs. O'Leary's Comet*, (Chicago, 1985), pp. 87-88. See also, *History of the Great Conflagration*, *op. cit.* p. 374.

¹⁵³²*History of the Great Conflagration*, *op. cit.* p. 391; see Donnelly, *op. cit.*, p. 417.

All the people and animals were seized by an overwhelming panic. Thus, we also have a description of animals and humans terrified and fleeing for dear life, perhaps into caves to escape the terror pursuing them.

But let us go now to the destruction of cities by the accumulation of several feet of ash, as described by Claude F. A. Schaeffer, all across the Near East and other parts of the globe; thus, we then come to archeology. As Alfred de Grazia pointed out in *KRONOS* in 1976:

"A completely wooden and overstuffed contemporary house will leave no more than ankle-deep ashes when it burns to the ground, and then only on its own foundation. A flourishing natural forest and the ground cover is estimated to provide 200 tons of organic matter per acre. When reduced fully by heat, it will give up 160 tons of water, gases and other compounds to leave 20 tons of carbon residue and 20 tons of oily distillates. Further reduced to fine cinder and ash, it would be less and have less volume. If spread over an acre, the residue would amount to perhaps a pound per square foot; its height would scarcely measure 6 inches in its freshly fallen state."¹⁵³³

But instead of finding up to several feet of ash in the strata of ancient cities such as Troy, de Grazia described "a bed of ashes that may have amounted to 15 to 20 feet" ¹⁵³⁴ Therefore, we are involved with heat of such intensity that it could almost literally melt and incinerate certain buildingstones and bricks, especially those made of mud, clay, and straw commonly used in the ancient Near East. As Donnelly explains with respect to the fire that burned Chicago down, the,

". . . flames that consumed a great part of Chicago were of an unusual character and produced extraordinary effects. They absolutely *melted* the hardest buildingstone, which had previously been considered fireproof. Iron, glass, granite, were fused and ran together into grotesque conglomerates, as if they had been put through a blast furnace. No kind of material could stand its breath for a moment.

I quote . . . from Sheahan & Upton's work:

"The huge stone and brick structures melted before the fierceness of the flames as a snowflake melts and disappears in water, and almost as quickly. Six-story buildings would take fire and *disappear forever from sight in five minutes by the watch* The fire also doubled on its track at the great Union Depot and burned half a mile southward *in the very teeth of the gale* *Strange fantastic fires of blue, red, and green played along the cornices of buildings.*"

Hon. William B. Ogden [the Mayor] wrote at the time:

"The most striking peculiarity of the fire was its intense heat. Nothing exposed to it escaped. Amid the hundreds of acres left bare there is not to be found a piece of wood of any description, and *unlike most fires, it left nothing half burned* The fire swept the streets of all the ordinary dust and rubbish, consuming it instantly."

¹⁵³³Alfred de Grazia, "Paleo-Calcinology: Destruction By Fire In Pre-Historic And Ancient Times," Part I, *KRONOS*, Vol. I, No. 4, (Winter 1976), pp. 28-29.

¹⁵³⁴*Ibid.*, p. 28.

"The Athens marble burned like coal! [citing the *History of the Chicago Fire*, Donnelly explains.]

"The intensity of the heat may be judged, and the thorough combustion of everything wooden may be understood, when we state that in the yard of one of the large agricultural-implement factories was stacked some hundreds of tons of pig iron. This iron was two hundred feet from any building. To the south of it was the river, one hundred and fifty feet wide. No large building but the factory was in the immediate vicinity of the fire. Yet so great was the heat, that *this pile of iron melted and run [sic] and is now one large and nearly solid mass.*"¹⁵³⁵ (Donnelly's emphasis)

Mayor Ogden sums up the intensity of the heat of the fire thus:

"The fire was accompanied by the fiercest tornado of wind ever known to blow here, and it acted like a perfect blowpipe, driving the brilliant blaze hundreds of feet with so perfect a combustion, that it consumed the smoke, and its heat was so great that a fireproof building sunk before it, almost as rapidly as wood. Nothing but earth could withstand it."¹⁵³⁶

The heat coming apparently from above was of such intensity that it was capable of rendering most materials into ash, and melted metals of all kinds. In spite of this overreaching catastrophe, in some of the towns entire buildings were miraculously untouched.

The explanation that all of this destruction of life and property is perfectly congruent with the concepts of cometary debris raining down along a few paths in one area encompassing three regions around Lake Michigan, is put forth by Waskin:

"It surely could not have been a coincidence that fires, which had been burning normally, that is to say under control and offering no serious danger to life, should suddenly, at exactly the same time on the same night, hundreds of miles apart, burst out with an unprecedented fury and become impossible to control . . .

"And how to explain the hot sand that rained down in the woods of Wisconsin . . .

"And then there were the balloons of [descending] fire . . . What caused clouds of searing flame to fall from the sky on people, buildings, farmland and wood?

"Fragments of a dead comet with its frozen gases, its cargo of sandy cosmic debris, which intersect Earth's orbit and crash into its surface in the worst possible place at the worst possible time—in drought-stricken regions where fires are already burning . . . This would explain all the peculiar features of the great three-headed fire of October, 1871.

"The wind where the Comet had landed was a gale. The gases in the Comet were freed from their frozen prison. Molecules of methane and acetylene, which . . . [can burn at] high temperatures [and] ignite [at] high temperatures, . . . than the five hundred or so degrees at which wood ignites—reached that high temperature quickly because of the blowtorch effect of the wind feeding oxygen to the flames. The fires at the three places where the Comet had struck were fueled.

"Sand grains from the Comet, which had been heated by its plunge through the atmosphere and by the fiery air into which they had fallen, rained on the earth. The tremendous heat of the fire ignited the clouds of gases which were trapped inside the Comet, the clouds exploded into flame and fire

¹⁵³⁵Donnelly, *op. cit.* pp. 421–422. See Sheahan & Upton, *op. cit.*, p. 121.

¹⁵³⁶Waskin, *op. cit.*, p. 130.

balloons were the result. When the cometary fuel was exhausted, the fires gradually burned themselves out."¹⁵³⁷

But Asimov says fire does not fall from the sky.

Therefore, if this type of phenomena occurred in very recent times, then a catastrophe of the magnitude described by Velikovsky would have brought a great many small comets to the Earth which would in some regions, though not in all, have brought fire from the sky and created evidence of such immense heat as described by the great amounts of ash found at sites across the ancient Near East. In this respect, Stephen D. Peet describes an ancient stone fort or, more exactly, a stone wall located on a hilltop surrounded by fifteen to thirty foot cliffs. The wall is about four to five feet high and from twenty to thirty feet broad at its base. The stones are angular in shape and not fitted together on the basis of their shapes but were apparently piled one upon the other to build up the wall. But what is most outstanding regarding this hill fort, according to Peet, is the stone structure.

" . . . [it] exhibit[s] the marks of intense heat, which has vitrified the surfaces of the stones and fused them together. Strong traces of fire are visible at other places on the wall, the point commanding the broadest extent of country. Here are two or three small mounds that seem burned through out. Nothing is more certain than that powerful fires have been maintained for considerable periods at numerous points on the hill."¹⁵³⁸

In effect, the heat applied to the outer wall of this hill fort was so great that it caused a glass-like material in the rock to melt and actually run down the wall cementing the stones together. The problem is to explain how such high temperatures could have been created by wood burning fires. As is well-known, fires of large wooden structures are seldom hot enough to melt stones. But this fort in the Americas is not unique. Others have been found in Scotland, Ireland, Brittany, and Bohemia. According to Charles Fort:

"The stones of these forts exist to this day, vitrified, or melted and turned to glass.

"The archaeologists [sic] have jumped from one conclusion to another, like the 'rapid chamois' . . . to account for vitrified forts, always restricted by the commandment that unless their conclusions conformed to such tenets as Exclusionism of the System, [excluding materials falling from the sky] they would be excommunicated. So archaeologists, in their medieval dread of excommunication, have tried to explain vitrified forts in terms of terrestrial experience. We find in their insufficiencies the same old assimilating of all that could be assimilated, [earthly explanations] . . . into the explanation that vitrified forts were made by prehistoric peoples who built vast fires—often remote from wood supply to melt externally, and to cement together, the stones of their construction. But negativeness always: [to heavenly explanations] so within itself a science can never be homogeneous or unified or self harmonious. So, Miss Russel, in the *Journal of the B.A.A.*, [*Journal of the British Astronomical Association*] has pointed out that it is seldom that stones, to say nothing of long walls, of large houses that are burned to the ground are vitrified . . .

"That the stones of these forts are vitrified in no reference to cementing them: that they are cemented here and there, in streaks, as if special blasts had struck, or played, upon them . . .

¹⁵³⁷Waskin, *op. cit.*, pp. 143–144.

¹⁵³⁸Stephen D. Peet, "Defensive Works of the Mound Builders," *American Antiquarian*, Vol. 13, (1891), pp. 213–216.

"Once upon a time something melted in streaks, the stones of forts on the tops of hills in Scotland, Ireland, Brittany and Bohemia

"But some of the vitrified forts are not upon tops of hills: some are very inconspicuous: their walls too are vitrified in streaks.

"Something once had effect, similar to lightning, upon forts, mostly on hills, in Scotland, Ireland, Brittany and Bohemia.

"But upon hills, all over the rest of the world are remains of forts that are not vitrified."¹⁵³⁹

The explanation, I suggest, to account for these unique meltings in mostly high areas is that like, the Peshtigo, Chicago fires, comets on almost parallel paths, made up of similar materials, passed across these regions. In spite of the view that these meltings were induced by humans, the more honest archaeologists admitted:

"The heat of the fire which produced such amazing effects, must have burned with the force of the strongest furnace; and from the general appearance of the cleft in the wall and these vitrified masses, I should be inclined . . . to attribute the catastrophe to lightning from heaven"¹⁵⁴⁰

I have gone to considerable length to show that there is ample evidence in the literature, prior to the time Asimov wrote this criticism, that fire does fall out of the sky and may very well be related to comets. After all, there are no volcanoes near Chicago to have created what was observed, and Biela's comet passed near the Earth at that time. But this evidence will never touch Velikovsky's critics.

Asimov's great admiration of Carl Sagan has caused him ultimately to proclaim "that he was an emotional, willing-to-die-for-him supporter of the Cornell astronomer."¹⁵⁴¹ That was a daring statement for Asimov to make. When one has a leader like Sagan one never knows where he will lead his unwary follower and then abandon him. Asimov states:

"Instead of all that, I just mention one little thing. There are many limestone caves in the world in which many stalactites and stalagmites have been slowly and precariously forming over a period of hundreds of thousands of years. They are quite brittle.

"If the Earth had stopped its rotation at the time of the Exodus, or if it had even slightly changed its period of rotation, every one of those stalactites and stalagmites would have broken.

"They did not! They are there! Intact and beautiful, as you will see for yourself if you visit any limestone cave. And those stalactites and stalagmites, standing there mutely, are stronger evidence against Velikovsky's theory than all Velikovsky's selected lines from myths and legends can possibly counter."¹⁵⁴²

Over the great cavern leading into hell in which can be seen stalactites and stalagmites projecting from the ceiling and floor, the great poet Dante said it was written, "Abandon Hope All Ye Who Enter Here."

¹⁵³⁹Charles Fort, *The Book of the Damned*, (Ace Book reprint), (New York, 1941), pp. 166-167.

¹⁵⁴⁰Anonymous, *Tower of Babel*, *American Journal of Science*, Vol. I, No. 37, (1839), pp. 352-353.

¹⁵⁴¹Fredric B. Jueneman "PC," *KRONOS*, Vol. I, No. 3, (Fall 1975), p. 77.

¹⁵⁴²Asimov, *op. cit.* pp. 42-43.

Martin Gardner wrote in the 1950's that these limestone formations would not have broken, but would have "altered the vertical lines of the long stalactites in Carlsbad."¹⁵⁴³ If this occurred in that area where these forms were tipped from the vertical, the stalactites and stalagmites would not have broken! Asimov says they are delicate and would break if shaken, but they would not break, according to Gardner, when an earthquake would have shifted the topography. As is shown in *Pensée*, the areas with the greatest number of limestone caves lie in belts in the United States which experience the greatest number of earthquakes!¹⁵⁴⁴ In spite of this, these forms did not break! Why? Perhaps Gardner understood, as Carl Sagan did, that the force from a gentle deceleration of the Earth would have very little effect on such structures. For as Sagan showed in his Appendix 2:

"But it is easy to see . . . that a gradual deceleration of the Earth's rotation at 10^{-2} g or so could occur in a period of much less than a day . . . even stalactites and other delicate geomorphological forms could have survived."¹⁵⁴⁵

Asimov ends his piece that to be serious about anything in Velikovsky's *Worlds in Collision*, "All you need is . . . abysmal ignorance . . ."¹⁵⁴⁶

The abysmal ignorance over and over again comes straight from Asimov. "But that's enough." (To quote Asimov):

"My space is used up and why go further? At least fifty more passages can be chosen from the book but I've proven my point. I think, that Velikovsky's theories are simply silly.

"In fact, rather than have the miracles of the Bible explained by such a farrago of broken astronomy, half physics and semi-chemistry. I would accept them exactly as given in the Bible. If I must choose between Immanuel Velikovsky and Cecil B. De Mille, give me De Mille, and quickly."¹⁵⁴⁷

Asimov's comments reflect his own errors more than anything related to Velikovsky's theory. His explications of Velikovsky's work in science are either science fiction or absurdity; his presentation regarding the formation of Mars' inner satellite is not broken astronomy but absurd astronomy and absurd gravitational physics; his analysis of "carbon and hydrogen gases" was not semi-chemistry but mental alchemy. When he first wrote this criticism, he placed it where it truly belonged and where it truly reflects its contents. He wrote this for a cheap pulp magazine, *Fantasy and Science Fiction*. That is all it ever was, and that is the realm in which it belongs. As for science, Asimov, whom I have previously admired for presenting so much science so clearly for so long, has presented only absurdity ad nauseam. In his great *Foundation* trilogy, Asimov wrote:

"Violence is the last refuge of the incompetent."

The violence in tone, in scholarly approach and in specific criticism of Asimov's work on Velikovsky has now been exposed in full. But I am deeply troubled to have to attack the work of a man whose books I have loved since I was

¹⁵⁴³Martin Gardner, "Monsters of Doom," *Fads & Fallacies in the Name of Science*, (New York: Dover, 1957), p. 29.

¹⁵⁴⁴*Pensée*, Vol. III, (Winter 1973), p. 49.

¹⁵⁴⁵Sagan, *op. cit.*, p. 64.

¹⁵⁴⁶Asimov, *op. cit.*, p. 46.

¹⁵⁴⁷*Ibid.*

young. If I have learned anything from Asimov it is that science is one of the most wonderful, interesting and important of all human endeavors. How dare he do this to me! To Velikovsky! To his readers! and to SCIENCE!!!

* * * * *

POMPOUS ASIMOV, By Irving Wolfe

In the previous chapter, Charles Ginenthal has dissected Isaac Asimov's scientific critiques of Velikovsky and shown that, as a scientist, he doesn't know what he's talking about. In this chapter, I will look at Asimov's attempts to deal with Velikovsky ideologically and historically, to show that, as a would-be historian and philosopher of science, he also does not know what he's talking about. The chapter will be divided into three parts. In the first, (composed of several sections), I will analyze Asimov's unsuccessful attempts to label Velikovsky a crackpot, to explain away the Velikovsky Affair and to offer facile pop-sociological reasons for Velikovsky's popularity. It will be shown that in these areas he is ignorant and futile. I will then in the second part look at one of Asimov's few attempts to be an actual scientist, and show how his failure there *and how mainstream science reacted to it* provide further evidence for the Affair. In the final section, I will review two of Asimov's short stories, to illustrate how, when he is being a science *fiction* writer, he gives great importance to the very sorts of catastrophic events which, in his roles as writer and teacher of science, he tries to deny.

Because these co-existing roles *which Asimov has freely chosen* clash so noticeably among themselves, I will argue that this bizarre bifurcation in the persona of Asimov becomes evidence for Velikovsky. That is to say, when Asimov's left hand denies what his right hand is doing, when the unconscious assertion of catastrophe in his art is so powerful while the conscious demolition of it in his science is so feeble, we may legitimately suspect that such self-contradictory behavior comes not from his intellect but from some deep passion. When we ask what this passion might be, it is not difficult in the light of what I wrote in the first chapter to guess at a possible origin, which is that the catastrophes are true and that Asimov is terrified of them and wants to forget them but cannot fully suppress them. They must therefore be both rejected and yet somehow acknowledged. That in capsule form comes from a theory I have been developing since the 1970's, (following Velikovsky's lead), in which our culture responds to the racial knowledge of catastrophe by erecting conscious activities of denial (science) and unconscious outlets of remembering (art, myth, religion). This would explain perfectly Asimov's paradoxical paranoid behavior, as well as that of Shapley, Sagan, Payne-Gaposchkin, Menzel, *et al.*—it is that Velikovsky's ideas hit a raw nerve in Science and those in pain reacted with fury and hatred. Some (the scientists) tried only to suppress it, but Asimov the scientist *and* fiction writer did both, encapsulating within himself the two normally-separated halves of our traumatized, dichotomized Western culture. The theory itself will be more formally advanced in my concluding chapter, but I have reviewed it now because it applies well here, for only such a model can account for the phenomenon of intelligent men of science, not merely Asimov alone, all behaving unintelligently toward Velikovsky *in the same way in the same situation*. I will argue that Asimov's unbalanced overall performance, wavering between irreconcilable extremes of affirmation and denial, constitutes a schizoid demonstration of the theory that we all possess an ineradicable but unconscious knowledge and therefore an unconscious fear of catastrophes, which amusingly is the very thing he, Asimov, is out consciously to disprove. That is to say, with regard to the desire to annihilate Velikovsky, Asimov becomes his own worst enemy.

As I said, Ginenthal in the previous chapter has taken care of Asimov's scientific critique of Velikovsky, contained in the article "Worlds in Confusion," (1969). The attack I will deal with here was published five years later in the science-fiction magazine *Analogue: Science Fiction/Science Fact*, (October, 1974, pp. 38-50). Lewis M. Greenberg, Editor in-Chief of the journal *KRONOS* at the time, called it

". . . a rather bizarrely muddled piece . . . in which he vilified Velikovsky in typical *ad hominem* fashion
No objective criticism was put forth."¹⁵⁴⁸

The article was called "CP," (Asimov's polite euphemism for "crackpot"), and in Greenberg's opinion it "goes beyond the pale and is heinous to the point of not even being worthy of any comment or rebuttal. Yet, it is so misrepresentative of Velikovsky and his ideas that some retort is necessarily called for in order to set the record straight."¹⁵⁴⁹ Engineer Frederic B. Jueneman published a counterattack in that same issue of *KRONOS*, which he called "PC."¹⁵⁵⁰ (He had been invited to write an article about Velikovsky for *Analog*, called "The Search for Truth," and Asimov's piece "CP" appeared immediately after it). In Jueneman's opinion, Asimov's article is quite plainly "immoral A grievous wrong has been committed."¹⁵⁵¹ The picture of Velikovsky he presents "is an artifice of his own design"¹⁵⁵² which he handles in a very unscrupulous manner, "using the standard time-honored ploy of misquoting, holding it up for ridicule, and then attacking the misquote."¹⁵⁵³ The article, says Jueneman, is a "pastiche" which Asimov "had created for himself, and . . . continued to stick pins into his idealized mannikin."¹⁵⁵⁴ Jueneman finds the article full of errors and concludes that it is "an ill-advised emotional and subjective attack"¹⁵⁵⁵ whose purpose is quite plain—"to discredit Velikovsky by any and all means."¹⁵⁵⁶ I agree with Jueneman, but we may carry it further—Asimov's attack is not merely "subjective and emotional,"¹⁵⁵⁷ it is bizarre. Let us count the ways.

1. Asimov as Would-Be Sociologist.

The first task Asimov sets himself is to establish that Velikovsky is a CP, a term he prefers to the more demeaning "crackpot." He begins with the dictionary definition, which is someone "espousing bizarre ideas,"¹⁵⁵⁸ and feels that this describes Velikovsky. (We will see in a moment whose ideas are the bizarre ones). Next comes a statement of Asimov's intention. His purpose in this analysis, so he says, is to be objective and scientific, rather than critical and derogatory, but look at the tone in which he puts this, which bodes ill for his desire to be rational:

"It is not my intention here to cite you chapter and verse of Velikovsky's bizarre notions and hold them up for you to see and chuckle at."

Why?

¹⁵⁴⁸*KRONOS* I. 3, p. 73.

¹⁵⁴⁹*Ibid.*

¹⁵⁵⁰*KRONOS* III. 3, 73-83.

¹⁵⁵¹*Ibid.*, pp. 73-74.

¹⁵⁵²*Ibid.*, p. 74.

¹⁵⁵³*Ibid.*, p. 78.

¹⁵⁵⁴*Ibid.*, p. 79.

¹⁵⁵⁵*Ibid.*, p. 74.

¹⁵⁵⁶*Ibid.*, p. 82.

¹⁵⁵⁷*Ibid.*, p. 79.

¹⁵⁵⁸Asimov, 1974, p. 38.

I have already done this in an article entitled "Worlds in Confusion."¹⁵⁵⁹

By using the term "bizarre," it is as if he cannot restrain himself from sarcasm, and that is not a good beginning, nor is the implication that *everyone* will simply "see and chuckle at these notions." Beyond the malice of these words, notice that there is a not-too-subtle pat on the back which he gives himself as a satirist, (implying that he *could* be vicious if he wanted to), but he tells us again that his purpose here is purely scientific: ". . . what I intend to discuss here is CP-ery in general, using Velikovsky only as a convenient example."¹⁵⁶⁰

Let us therefore take a look at Asimov's discussion of "CP-ery," to see if it is scientific or vicious. What stands out at once is that he considers CP-hood to be a solid, definite, recognizable phenomenon which everyone knows about fully *and which can therefore be taken for granted*. As a result, he casually tosses off a set of vague, over-general assertions about it.

"There are CPs everywhere, of course. What's more, almost everyone has a touch of the CP in him."¹⁵⁶¹

This is stated as a scientific datum. Not only that, but he tells us (with the same absolute air of certainty) that there are known gradations within the category.

"There are, however, degrees of CP-hood and there are certain distinguishing marks by which you can tell a *far-gone* CP . . . it is the far-gone variety to which I refer."¹⁵⁶²

The impression given is that he, Asimov, not only possesses a perfect taxonomy for the genus CP, but an infallible nose for spotting every species.

"I refer to the CP-in-science particularly, though what I say may well be applicable to other kinds of CP."¹⁵⁶³

(As if he is an expert on every possible type.) The foolishness of these remarks renders his simplistic arrogance and naive pomposity apparent, but he makes it much worse for himself as he goes on.

"The CP 'insists on considering himself infallible This very rigidity has the smell of CP-hood about it.'¹⁵⁶⁴

(We will leave it to the reader to decide who is the most rigid here, and who considers himself to be infallible).

¹⁵⁵⁹*Ibid.*

¹⁵⁶⁰*Ibid.*, pp. 37-38.

¹⁵⁶¹Isaac Asimov, "CP." In *Analogue: Science Fiction/Science Fact*, (October 1974), p. 38.

¹⁵⁶²*Ibid.*, p. 39.

¹⁵⁶³*Ibid.*, p. 39.

¹⁵⁶⁴*Ibid.*, p. 43.

"Another characteristic of the CP is his high selectivity when it comes to evidence."¹⁵⁶⁵

(We will see later that Asimov's attempt to downplay the extent of the Velikovsky Affair is callously selective.)

These points constitute the bulk of Asimov's evidence, and it is easy to see where his *process* of analysis originates. Asimov is a chemist, and he simply applies to CP-ery a structure of analysis modelled on standard high-school chemistry, in which every substance has *properties*. Analyze the material in front of you, find out what properties it has and you will know what substance it is. Without thought, without hesitation, Asimov the chemist simply transfers this mode of investigation to his efforts as Asimov the sociologist, a naiveté which allows him without any fear or doubt to talk as definitely about the CP and CP-hood and CP-ery and the CP-in-science, (not to mention the far-gone CP), as any chemist about a salt or any entomologist about a species of butterfly. It is an astonishing performance.

Asimov does it with an ease and confidence born of blissful ignorance, but a much wiser picture of this issue is offered by Henry Bauer in his book *Beyond Velikovsky*. We shall consider this now, as a direct contrast to (and, therefore, repudiation of) Asimov. Bauer is a very severe critic of Velikovsky, but, to his credit, he is equally severe about the shortcomings of many of Velikovsky's previous critics, especially Asimov. On the question of crank-hood in particular, Bauer quotes from a number of people notorious for their spiteful attacks on Velikovsky, such as Martin Gardner, Patrick Moore, Sprague de Camp, Murray Gell-Mann and Asimov, (plus those who simply lump Velikovsky with "fads" like quacks, flying saucers and ESP), and dismisses *all* of their attempts to prove Velikovsky a crank.

"The books just mentioned are representative of a burgeoning literature that aims to debunk unorthodox ideas. Typically, cranky notions are described but the supposed errors in them are rarely demonstrated."¹⁵⁶⁶

He seems to be describing Asimov in particular when he writes that "the authors assume that the preponderance of their readers will concur, without requiring proofs, that the notions are indeed cranky."¹⁵⁶⁷ The writing produced by these men, therefore, is hardly convincing.

"The effect is that we are presented rather dogmatically with descriptions of purported absurdities, with the clear implication that, if we have any sense, we also will view them as absurdities."¹⁵⁶⁸

That is what Asimov says. Much more cautiously, however, Bauer asks "How can one be so sure? . . . Who is qualified to make the judgement?"¹⁵⁶⁹ His answer is firm.

"In fact, of course, no one is qualified to make such a sweeping *ex cathedra* pronouncement about Velikovsky. We see in action here the tendency for human beings to speak with the language of certitude when all that is warranted is the expression of a personal judgement."¹⁵⁷⁰

¹⁵⁶⁵*Ibid.*, p. 46.

¹⁵⁶⁶Henry H. Bauer, *Beyond Velikovsky, The History of a Public Controversy*. (Urbana: Univ. of Illinois Press, 1984.

¹⁵⁶⁷*Ibid.*

¹⁵⁶⁸*Ibid.*

¹⁵⁶⁹*Ibid.*, p. 138.

¹⁵⁷⁰*Ibid.*

Bauer of course feels that *he* has attained a true perspective on the topic, (unlike those he just mentioned), but insists it was not done with any help from Asimov and friends.

"I found it no easy matter to reach an opinion about Velikovsky The critics of Velikovsky did little, if anything, to help me reach my conclusions—their *assertions* were of no assistance to me in my search for *evidence* and *proof*."¹⁵⁷¹

In essence, Bauer is saying that Asimov and these others were not only no help but showed him nothing of value with respect to Velikovsky. So much for Asimov's heroic achievement in the annals of CP-ery.

Bauer similarly debunks more of Velikovsky's critics, including Donald Menzel, Howard Margolis, Sir Harold Spencer Jones, Harrison Brown, (who said ". . . we can conclude with an unusually high degree of safety that Velikovsky's theories are . . . nonsense") and Otto Struve, who "assured us of his infallibility."¹⁵⁷² Similarly, Sprague de Camp "has not, himself, open-mindedly, judged Velikovsky's writings," and Martin Gardner "is indulging in wishful thinking, not accurately portraying a situation."¹⁵⁷³ *None* of these people, asserts Bauer, has ever demonstrated that Velikovsky is a crank, and this pointedly includes Asimov.

The problem for Bauer lies not merely with the ineptitude of Velikovsky's attackers, but with the difficulty of establishing the truth. "It is much easier to label a man a crank than to prove that he is one," says Bauer, because "one man's sense is another's nonsense."¹⁵⁷⁴ For instance, John Sladek in *The New Apocrypha* seeks to separate unusual from nonsensical ideas, "but . . . he does not tell us how that effort is, or can be made."¹⁵⁷⁵ Martin Gardner grants Velikovsky's ideas a certain (although low) probability, "but he immediately proceeds to translate that estimate of *probability* into a statement that is intended to convey conviction, *certitude*."¹⁵⁷⁶ Sprague de Camp acknowledges the difficulty of assessing scientific theories, "But, in practice, de Camp can arrive at conviction after all,"¹⁵⁷⁷ and Asimov concedes that everyone can be a crackpot, but then asserts "that nevertheless *he* knows a real crank when he sees one."¹⁵⁷⁸ Bauer has little respect for people such as these, who "will admit . . . the general possibility of fallibility, in this labelling" but qualify their reservations "to convey that this fallibility really does not apply in the particular case . . . [when Velikovsky] is being discussed."¹⁵⁷⁹ He mentions for example the inept theorizing of philosopher Laurence Lafleur, who in an article "Cranks and Scientists" in the journal *Scientific Monthly* tried to dismiss Velikovsky by setting up "a procedure for identifying cranks," an effort which, says Bauer, "Velikovsky destroys in witty fashion"¹⁵⁸⁰ in *Stargazers and Gravediggers*.

Opposed to these reductive dogmatists, who see no obstacles, Bauer cites the difficulties which were encountered by more discerning and objective men when they dealt with the problem of separating "genuine revolutionary theories from cranky ones."¹⁵⁸¹ For instance, I. Langmiur, in "Pathological Science," proposed criteria which Bauer says could also be met by true discoveries, while Fred Gruenberger, in "A Measure for Crackpots,"¹⁵⁸² tries

¹⁵⁷¹*Ibid.*

¹⁵⁷²*Ibid.*, pp. 130-140.

¹⁵⁷³*Ibid.*, p. 141.

¹⁵⁷⁴*Ibid.*, p. 140.

¹⁵⁷⁵*Ibid.*, p. 142.

¹⁵⁷⁶*Ibid.*, p. 143.

¹⁵⁷⁷*Ibid.*

¹⁵⁷⁸*Ibid.*, p. 142.

¹⁵⁷⁹*Ibid.*, p. 144.

¹⁵⁸⁰*Ibid.*

¹⁵⁸¹*Ibid.*, p. 144.

¹⁵⁸²Fred Gruenberger, "A Measure for Crackpots," *Science*, Vol. 145, 1964, pp. 1413-1415.

to arrive at a ranking which would separate theorist from crank. Bauer, however, finds Gruenberger's scores subjective and decides that

"Gruenberger's discussion . . . does not lead to the certain identification of cranks . . . there is no overall score above which one can place 'scientists' and below which we have 'cranks.'"¹⁵⁸³

Bauer therefore concludes "There is rarely a certain way of identifying a crank."¹⁵⁸⁴ We have only opinions.

". . . we can judge only probabilities. Thereafter each one of us makes a subjective judgement, converting probability into subjective certainty This is no easy lesson to digest."¹⁵⁸⁵

(We can imagine that Asimov, whose tone is composed entirely of certitude, would have found this very hard to swallow). To strengthen his case, Bauer reminds us that

"The names of those who are now respected as great innovators, but who were labelled by their contemporaries as cranks, charlatans, or wicked, are legion Moreover, Gardner reminds us that even charlatans may sometimes be right."¹⁵⁸⁶

In general, therefore, Bauer's opinion of Velikovsky's critics is devastating.

"In such complicated matters as the Velikovsky Affair, objective certainty is simply not to be attained When the accepted, conventional scientific wisdom embodied in contemporary experts pronounces a man a crank, that judgement may be wrong."¹⁵⁸⁷

Of specific efforts like Asimov's, he says

". . . much of the writing that purported to show him to be a crank contains errors, logical *non sequiturs*, wrong and misleading statements."¹⁵⁸⁸

The result, therefore, was a total failure on the part of these critics, (including Asimov):

". . . the case made against Velikovsky by his critics was neither sound nor comprehensive."¹⁵⁸⁹

¹⁵⁸³Bauer, *op. cit.*, p. 147.

¹⁵⁸⁴*Ibid.*, p. 149.

¹⁵⁸⁵*Ibid.*, p. 147.

¹⁵⁸⁶*Ibid.*, p. 148.

¹⁵⁸⁷*Ibid.*, p. 151.

¹⁵⁸⁸*Ibid.*, p. 39.

¹⁵⁸⁹*Ibid.*

That, says Bauer, is the reason for the unscientific invective which these people employed against Velikovsky. It is that

". . . it is not easy to prove the case. By using the pejorative term, we add the force of emotional conviction, to compensate for the lack of logical completeness . . . we use the big emotional guns of name calling."¹⁵⁹⁰

I have carefully summarized Bauer's analysis of crackpot-ism because it is Asimov who introduced the term in his article. He must consequently stand or fall by it, and Bauer's comments help us to perceive that on this point *Asimov simply does not know what he's talking about*, for he is completely ignorant of every caveat raised by Bauer. He shows no sign of even being aware of the studies Bauer quotes, much less of ever having read or digested them. On the contrary, Asimov exemplifies almost all of the errors which Bauer lists, showing no restraint or caution or awareness or humility, but only pomposity and dogmatism. In contrast to Bauer's misgivings about the topic, Asimov acts as if the crackpot is easily discernible and as if he (Asimov) is a qualified expert in discerning one and as if he has applied the proper tests perfectly and, therefore, as if his verdict on Velikovsky is infallible and anyone who disagrees with him would be a fool. It is astonishingly naive and, despite its smugness and arrogance, we must conclude that it is entirely wrong and that the pompous Asimov is, therefore, an utter failure at his *own self-assigned task*. His article is called "CP," his intention is to prove Velikovsky a "CP," but he does not even come close to the mark *except as it applies to himself*. Bauer's comment on the species consequently suits Asimov perfectly:

"When someone classes Velikovsky (or anybody else) as a crank, I expect that writer or critic to have his facts straight, to have reached an informed judgment . . . Lamentably, we are all fallible, and those who label Velikovsky 'crank' are as fallible as the rest of us—even if they may seem to be unaware of that."¹⁵⁹¹

As we have seen, Asimov rarely "has his facts straight" and, therefore, has not even approached "an informed judgement." The last word against Asimov, however, must be left to Asimov himself:

". . . those who have been most insistent on possessing a pipeline to ultimate truth, have been most howlingly mistaken."¹⁵⁹²

He does not perceive, of course, that, because he insists with utter certainty that *he* possesses the ultimate truth about "the CP-in-science," these words of his apply most damagingly to himself. If only he had had enough self-understanding (or humility) to follow his own advice, we would not be howling at this contradiction now.

2. Asimov as Would-Be Psychologist.

So much for Asimov the expert on far-gone CPs. Let us now turn to Asimov the would-be pop-psychologist, where we will find, alas, that he fares no better. The question he poses is this—"if Velikovsky's theories are so CP in

¹⁵⁹⁰*Ibid.*, p. 152.

¹⁵⁹¹*Ibid.*, p. 139.

¹⁵⁹²Asimov, 1974, P. 43.

nature, why do so many people accept them as true—or at the very least, as possibly true?"¹⁵⁹³ He then offers 4 or 5 answers, each more shallow and glib than the next.

2a. People find the theories fascinating, which "is but one more reason for suspecting CP-ery."¹⁵⁹⁴ You ask why? Asimov uses as his proof a statement which in his eyes seems to be virtually a scientific law. I cannot find it in any science textbook, alas, but he states it with his usual certitude: "If we were to settle questions by popular vote, the most astounding follies would be voted in by a landslide. There is not a CP notion in the world that doesn't attract many adherents."¹⁵⁹⁵ The syllogism seems to be this—People vote more often for folly. Velikovsky is very popular. Therefore, Velikovsky is folly, and whatever he says is not true. (Lovely, isn't it?) This is Asimov's First Law of CP-ery. (We *could* ask if the reverse also holds, *i.e.*, if that which is *unpopular* must be true, but I will let that pass). Then follows Asimov's Second Law of CP-ery, which I will present in his own memorable words: ". . . people who are naturally attracted to CP-ery . . . ask only that some belief be foolish in order to be ready to die for it."¹⁵⁹⁶ The implication is unavoidable: Velikovsky is foolish, (as Asimov feels he has shown), and that is, therefore, why people are attracted to him. No evidence is offered for this assertion that foolishness is inevitably popular or that people are ready to die for it—apparently we are simply supposed to take Asimov's word for it—but there is an irony in these words which cuts Asimov much deeper than Velikovsky. It is this: when Asimov said that people are ready to die for foolish beliefs, he seems to have overlooked that *in this very same article* he had mentioned the beliefs of Carl Sagan, of whom he had said "as far as I know, he has no emotional, willing-to-die-for-him supporters except me."¹⁵⁹⁷ The irony is that Asimov's Second Law applies most tellingly to himself, for every part turns out to be true: Sagan *is* foolish about Velikovsky, which must be why Asimov is willing to die for him. To be precise, Carl Sagan made so many foolish errors in his attacks on Velikovsky that Charles Ginenthal was not able to restrict an analysis of them to a chapter-length for this book, but had to write a separate book on them, *Carl Sagan & Immanuel Velikovsky*, (publ. New Falcon Press, 1995). Asimov's support has left both Sagan and himself dead in the water.

2b. Religion.

"A second cause of Velikovsky's popularity involves . . . the 'let's-not-get-God-angry' syndrome."¹⁵⁹⁸

Most people are brainwashed into believing religious nonsense in childhood, says Asimov, "Nor are the public schools allowed to disabuse them in their younger years."

"This means that most free-thinking adults . . . have had to break away from a religious view, and many of them feel a little guilty about it. It is difficult to feel *entirely* confident that one might not wake up in Hell some day, shrieking, 'Oh, my goodness, I was *wrong!*'"

"Then along comes Velikovsky and he tells you that all those miracles in the Bible are literally true You can go back to believing the Bible! What a relief! What's more, it's all 'scientific' which makes it all very intelligent."¹⁵⁹⁹

¹⁵⁹³*Ibid.*, p. 47.

¹⁵⁹⁴*Ibid.*

¹⁵⁹⁵*Ibid.*

¹⁵⁹⁶*Ibid.*

¹⁵⁹⁷*Ibid.*, p. 43.

¹⁵⁹⁸*Ibid.*, p. 49.

The implication, of course, is that Asimov is an adult free-thinker and, therefore, not a believer in miracles, and he righteously separates himself from these frightened backsliders by observing that in his opinion it is far, far better to go to hell for "An honest atheism" than to denigrate God by explaining His miracles as "natural phenomena only."

" . . . wow, will *that* get God mad! However, I don't suppose Velikovskians are any better at theological subtleties than at astronomical ones."¹⁶⁰⁰

We are to understand that he Asimov is *very good* at such subtleties and knows how to get God *less* mad, and to do it bravely too, sticking to his guns as an atheist and not crawling to God in false piety.

The pomposity, the absurdity, the ignorance and the sneering smugness of these passages are hard to miss, but alas there is more, for what underlies Asimov's facile generalization about "free-thinkers" versus people who believe in religion is a view of scientists which is typical of America in the 1930's and 1940's, a view he imbibed in his youth and which he still exhibits 40 years later. It is a myth, of course, (routinely treated as such in the sociology of science), which was fabricated by science to function as a self-serving portrait of its superiority to religion at a time when its divergence from religion was still under severe attack. To show how in the 1970's Asimov's mind-set is still stuck in the 1930's, here are (believe it or not) his own words on the topic:

"Billions of idiots on Earth believe in magic, in ghosts, in omens and the evil eye, in astrology, in any and every variety of folly that you ever heard of or can invent. Among all those billions are one or two tens of thousands who are rationalists and who accept only what their senses and their reason tell them."

Who are these superior, privileged people? The scientists, of course, and who is among them? Asimov, of course.

"We few are friendless and alone and it's cold out here."¹⁶⁰¹

Asimov, therefore, sees himself as one in 100,000 people and science as one in 100,000 visions of nature, lone individuals and lone groups fighting bravely for the Truth against the superstition-ridden forces of Religion, Folly and Velikovsky. It is a very self-serving and self-flattering picture, and it is probable that most lay-people at first glance would agree with it. (After all, Science *is* right, isn't it? And all scientists disbelieve religion, don't they?) The only problem with Asimov's holier-than-thou attitude, however, is that on closer inspection it is quite wrong about the attitude of men of science about religion, for, if we look only at Newton and Einstein, arguably the two greatest scientists of the modern era, (and somewhat better respected than Asimov himself), we find the opposite. Einstein never wavered *throughout his life* from the belief that whatever science discovered was merely a portion of "the Old One's" secrets, and Newton set out on his task to solve the riddle of motion *precisely to prove the existence of God*. Asimov, therefore, displays great ignorance of the history of science. Once again, he simply doesn't know what he's talking about, for, if he were correct, then Newton and Einstein are crackpots too!

2c. Envy.

¹⁵⁹⁹*Ibid.*

¹⁶⁰⁰*Ibid.*, p. 50.

¹⁶⁰¹*Ibid.*, p. 42.

"Remember that the hard scientists . . . deal with those aspects of the universe sufficiently simple to allow hard and fast laws to be established. Their work has a great deal of predictivity and has produced astonishing results. That gives them a lot of prestige, and rouses envy."

"On the other hand, you have all kinds of other scholars who are in the soft-sciences and the non-sciences . . . who can produce nothing in the way of results and can only talk to each other."

"It's only natural for some scholars in the fields outside the hard sciences to greet with glee the possibility that astronomers and physicists don't-know-so-much."¹⁶⁰²

What Asimov is attempting to do is to draw a line between hard scientists, (like himself, of course), who deal with reality, and soft scientists who dally with the non-actual, and to imply that *all* hard scientists oppose Velikovsky and *only* "soft scientists" support him.

"It is not surprising that sociologists and other miscellany of the sort have been in the forefront of the Velikovskian movement."¹⁶⁰³

Again, despite the boorish malice and ignorance of Asimov's description, (as if most of those who support Velikovsky can be dismissed as part of some sort of "miscellany,") it is less important here to debate where Velikovsky's followers come from (as Ginenthal said, Einstein to an extent supported Velikovsky, and was Einstein not a scientist?) as to note how wrong Asimov is *about what the hard sciences really are*. He has just spoken of the "hard and fast" results of science, especially by physicists and astronomers, but consider the following short observations made about astronomy and physics by "real" astronomers and "real" physicists, some of whom may be even better-known than Asimov.

1. "We can only see nature blurred by the clouds of dust we ourselves make." (Sir James Jeans)

2. The laws of science "are a description, not of nature, but of the human questionings of nature," and they "tell us nothing about nature, but only something about our own mental processes." (Sir James Jeans)

3. "Are theories 'out there?' I don't think so—theories are inventions." (Heinz Pagels)

4. "Physical concepts are free creations of the human mind." (Albert Einstein)

5. "Science is made by men." (Werner Heisenberg)

¹⁶⁰²*Ibid.*, p. 50.

¹⁶⁰³*Ibid.*

6. "... the mind has, by its selective power, fitted the processes of nature into ... a pattern largely of its own choosing; and in the discovery of this system of law the mind may be regarded as regaining from Nature that which the mind has put into Nature." (Sir Arthur Eddington)

Compare these to Asimov's naïve confidence in the certainty, eternity, and objectivity of scientific laws. As for the related concept that hard scientists describe the real world while non-hard scientists and humanists "can only talk to each other," here is a rather different view.

a. In science "What is real is what we regularly talk about" and, therefore, "When we create a new way of talking about the world, we naturally create a new world." (Bruce Gregory, astrophysics, Harvard).

b. "It is wrong to think that the task of physics is to find out how nature *is*. Physics concerns only what we can *say* about nature." (Niels Bohr)

Almost all of these descriptions of hard science by all of these world-renowned men, (most of them Nobel prize-winners), were published years before (most of them decades before) Asimov wrote in 1974, yet he, the author of *The Intelligent Man's Guide to Science*, seems wholly unaware of them, but merely echoes the naïve truisms of the American *scientism* which was popular in the 1940's. (What he seems unaware of most of all is that the "hard" sciences are not as hard as he thinks). Asimov, therefore, displays great ignorance of the philosophy of science. Here too, he simply doesn't know what he's talking about, yet it is on this foundation of virtual scientific illiteracy (to read him on science is like taking a trip backward in time) that he erects his pop-sociological and pop-psychological criticisms of Velikovsky.

3. Asimov as Would-be Biographer.

The next step in Asimov's attack on Velikovsky is to accuse him of having a secret private agenda behind his allegedly universal theories about nature. Velikovsky's work, says Asimov, is "a glorification of the Jewish role in history ... developed in the aftermath of the Nazi holocaust."¹⁶⁰⁴ Velikovsky examined ancient history in "an attempt to revise it in such a way as to raise the prestige of a long-downtrodden people," and as a result "Velikovskian theories may attract many Jews."¹⁶⁰⁵ This of course does not explain why so many non-Jews find Velikovsky interesting, but the theory is much more damaging to Asimov than that, for he seems to display almost no knowledge of what Velikovsky's historical theories actually say or of how they have been received by non-Jews (Once again he is talking about something he knows nothing about).

Asimov is guilty of three categories of error here. The first is a lack of fundamental research. Notice that in this 1974 article, he glibly speaks of "conventional (*i.e.*, real) historians," and states that

"Velikovsky's historical theories are taken no more seriously by real historians than his astronomical theories are by real astronomers."¹⁶⁰⁶

(We have just dealt with what "real astronomers" say a moment ago. Let us restrict ourselves here to "real" historians.) Asimov's equation is simple: "conventional" historical dogma is real and it rejects Velikovsky's radical revisions, which

¹⁶⁰⁴*Ibid.*, p. 48.

¹⁶⁰⁵*Ibid.*

¹⁶⁰⁶*Ibid.*, p. 48.

are false. It turns out that this is what he said in his 1969 article, "Worlds in Confusion," which shows that his thinking had not altered since then. That is to say, he read nothing new about Velikovsky's revised chronology in the interim, nor considered a thing, but merely repeats his own opinion from 1969. A closer look, however, indicates that this opinion was formed long before that. Remember where Ginenthal in the previous chapter had taken Asimov to task for his false and/or ignorant statements about Velikovsky's historical theories in the 1969 article, saying that he should have known about the mixed reception of Velikovsky's chronology because

"The information . . . was available to Asimov 17 years prior to his attack, and which . . . he claims to have read." (Asimov in Absurdity).

Ginenthal had sarcastically quoted Asimov as saying "some of my correspondents self-righteously demand that I *read* Velikovsky before I denounce him But, as it happens, I have *read* him, and I remain untouched."¹⁶⁰⁷ Ginenthal's evidence, however, demonstrates that Asimov in 1969 had *not* read Velikovsky's historical books, (where the data about the reception of his theories is to be found), giving the lie, therefore, to Asimov's pretense of careful research in 1974 on which he allegedly independently reached his own opinion then.

Second, the idea itself is not original with Asimov, (as Bauer has noted), for the notion that Velikovsky created his chronological revisions only to defend the historical veracity of the Old Testament was first put forward by Martin Gardner in 1950.¹⁶⁰⁸ I think, therefore, that we may legitimately surmise that Asimov had *not* carefully read Velikovsky's historical theories, neither in 1974 nor in 1969, but had only read mainstream opinions *about* them, and had read them in the early 1950's, and that his comments on Velikovsky and Jews in 1974 derive from no more than this fixed, unaltered, never-re-evaluated *received* prejudice which he merely echoes.

Third, Asimov's point makes no sense in terms of what happened. Charles Ginenthal has already pointed out in the previous article that Velikovsky's revised chronology was powerfully supported by Prof. Robert Pfeiffer, Dept. of Semitic Languages, Harvard, (who is, I should think, a little more knowledgeable in this field than Asimov). I will add only two more names to this list—Etienne Drioton, Director of Antiquities, the Louvre, and Claude Schaeffer, world-renowned French archaeologist, both of whom saw great merit in Velikovsky's historical theories. (See Chapter One.) The point is not whether Asimov's opinion of Velikovsky as a historian should override those of these better-qualified men, but to establish that it is hardly likely that experts of this stature would be interested in Velikovsky's theories *as serious history* if they were merely wild ideas concocted by a Jew to glamorize Jews. They would have had to have substance *as history* or they would have been seen through by these experts.

I agree that Velikovsky may have had a private agenda as part of his overall intention, but in my opinion the issue is far more complex than the naive picture Asimov has presented. I have made a more extensive study of the matter, which was published as a transcript of an oral presentation in the American journal *AEON*, as a longer version in the British journal *Chronology and Catastrophism Review* and in double-length in the anthology *The Interaction of Scientific and Jewish Cultures*, (Edwin Mellen Press, 1995). It looks at the question in terms of Velikovsky's private, family, social, cultural and political history as a Jew born in Russia late in the 19th century and faced with several alternative options (fundamentalism, conservatism, assimilation, Zionism) in his response to the modern world, the rise of anti-Semitism and the coercion of ideas by a German-dominated intellectual milieu. My conclusion takes all of these considerations into account. In the face of this complicated analysis, Asimov's puerile contention that Velikovsky merely concocted his historical ideas to enhance the Jews is not only inept but it may pathetically miss the point, which is that *the ideas might nevertheless be true*. If an Afro-American rewrites traditional U.S. history to show what really happened to the Blacks in America, or if a native of India re-writes traditional accounts of how the British treated the Indians, these new theories might certainly be "non-conventional" and "radical," and they most certainly would enhance the stature of Afro-Americans and Indians, but they may also be true. It is not a question of biased revisionism but the opposite, of *setting the record straight*, which I am sure was Velikovsky's quest, and most often it *has* to be done by a member of the "downtrodden" oppressees because the dogmatists in power will not do it. The question of motives is,

¹⁶⁰⁷*Ibid.*

¹⁶⁰⁸Martin Gardner, "The Hermit Scientist," *Antioch Review*, Vol. 10, pp. 447-457.

therefore, quite murky, for everyone has a mixture of them, (even the most "conventional" historians), and it cannot be resolved in Asimov's shallow supercilious manner. He doesn't know what he's talking about.

4. Asimov as Science Historian

We turn next to Asimov the would-be historian of science, for, in this attempted demolition of Velikovsky, beyond trying to prove that he is deficient as a scientist, Asimov also tries to give us *the real truth* about the Velikovsky Affair, (so that even for this reason we should have no pity for Velikovsky). Here again, he will turn out to be wholly incompetent, but it will be interesting to discover why. This part of his argument occurs in four steps, each equally ludicrous.

Step 1. Science is too weak to have done all the reprehensible things people said it did.

"The religious orthodoxy had, at its disposal, the stake, the rack, and the thumbscrew, to say nothing of the mob, which could be easily roused to hunt down the heretics But scientific orthodoxy—why, it is the weakest and most powerless orthodoxy ever invented."¹⁶⁰⁹

In the first chapter I showed that dozens of scientists massed in hysterical attack on Velikovsky, acting precisely like an aroused mob hunting the heretic. Therefore, when Asimov speaks of the "powerlessness" of Science, consider Harlow Shapley as an example—his tentacles spread into most of the important scientific journals, magazines and organizations of 1950, to say nothing of the Museum of Natural History and the Hayden Planetarium, as I have shown, to the point where he singlehandedly exerted a powerful influence over scientific thought. Is such a man without power? More specifically, is Asimov unaware of the many studies which have shown that Science as an institution functions as a power structure riddled by influence, prejudice and old-boy networks? I think not. In my opinion, when he protests so plaintively on behalf of poor little Science, he is being disingenuous.

Step 2. Then, after Science is presented as noble but weak, comes Asimov's attempt *to turn the whole Velikovsky Affair around* by presenting it as actually having been a heroic us-against-them situation, where the horrid determined Velikovskians are the indefatigable hordes of "them" and the poor scientists are "us."

"Velikovsky . . . has many thousands of devoted followers, who spew out endless nonsense in his name, who publish magazines devoted to spreading his irrationalities, and who write endless, furious letters And on the side of orthodoxy, what do we have? A few astronomers with no particular following and no experience in . . . polemics."¹⁶¹⁰

Shades of the Yellow Peril! (China turned Communist in 1949 and now there are hundreds of millions of them eager to destroy us!) That is what Asimov *as historian* is saying, that Velikovsky and his irrational followers are a menace to the chosen few (the scientists) who uphold the American and scientific way of life. What we have here, of course, has a powerful cultural prototype: it is the hysterical black-versus-white fear-mongering that was present often in America, especially during the McCarthy era, only now it is not monstrous insects or kill-crazed tomatoes which threaten the U.S., but throngs of obsessed Velikovskians, led in robot-like fashion by the Mad Doctor himself. Hard as it is to believe, that is the pop-cultural image Asimov is trying to evoke, that the crazed Velikovsky and his "thousands" of mesmerized

¹⁶⁰⁹*Ibid.*, p. 41.

¹⁶¹⁰*Ibid.*, p. 42.

cohorts are a great danger to America. The point is rather obvious: should we not, therefore, be *grateful* that Asimov and his men stand ready to protect us from this omnipresent menace? We see then what Asimov's objective is—it is to imply (no less) that, if there *is* evil in this situation, it lies in Velikovsky and not in the simple pure innocent scientists who misrepresented him, slandered him and attacked him, *for they are heroes*. (With a talent like this, Asimov should really have gone into politics).

Consider, however, the evidence. Does the AAAS, the largest scientific organization in the world, represent merely "A few astronomers"? Does Carl Sagan, whose books sell in the hundreds of thousands, have "no particular following"? *Or Asimov himself?* One wonders what on earth Asimov is talking about. Has he confused Velikovsky with the Reverend Moon and himself and his ilk with the lone sheriff courageously battling a large gang of vicious outlaws? Does he see himself as the boy who puts his tiny finger in the huge dike and saves his society? I suspect that something like this type of self-serving myth provides the melodramatic pattern for his argument. He has falsely taken a well-known and self-glorifying pop-image and tried to get us to view the Velikovsky Affair through the filter of this good guy-bad guy icon.

Step 3. Velikovsky's predictive success means nothing. He may have said for example that Venus' surface temperature would be much hotter than mainstream science expected, but that has since been explained by forces and causes having nothing to do with the presence of giant comets, (*i.e.*, it is not necessary to invoke catastrophe), and the same holds true for each of Velikovsky's assertions. They are pure chance, and Asimov knows just how they came about.

"Still, Velikovsky *did* make that prediction, and that just shows that it is difficult even for the most confirmed CP to write entire books without making some lucky hits. Do you think Velikovsky was the first to hit on a fact by accident? Do you suppose that one lucky hit, or five more like it, turns an entire tissue of folderol into gospel truth?"¹⁶¹¹

Obviously, if Asimov is out to destroy Velikovsky, he must discredit the predictions, and this is his attempt. Let us consider the many nonsensicalities it contains.

- 1.Asimov states it as a scientific phenomenon that even a "confirmed CP," *if he writes enough books*, will score "some lucky hits." This is presented as a proven law of nature, not as casual guessing.
- 2.People before Velikovsky who were in the wrong have "hit on a fact by accident." Velikovsky seems to have hit on some facts but is known by Asimov to be wrong. Therefore, it must be totally by accident.
- 3.Despite this, if one knows in advance, *knows for certain*, that what Velikovsky wrote is a "tissue of folderol," then any number of lucky hits cannot turn it "into gospel truth." He does not tell us just how he knows this, nor does he notice the irony of lumping "gospel" with "truth," he merely takes it for granted that *he* does know that Velikovsky is merely "lucky."

We could end this section here by simply noting that what Asimov says about the predictions is trivial, banal and foolish. To be more precise, *it is not scientific*, but the faults go deeper than that. In the first chapter, I presented a detailed discussion of the *issue* of Velikovsky's predictions. (Not the substance, but how they were received). I described how they were considered impossible when first said, how nevertheless virtually every one of them turned out to be right, how several established mainstream scientists recognized this and tried to get the attention of science focused

¹⁶¹¹*Ibid.*, p. 45.

on then, but how hostile the reaction was by Science in general. (All of this occurred well before 1974). Furthermore, Velikovsky's article "'Worlds in collision' in the Light of Recent Finds in Archaeology, Geology, and Astronomy," which describes many of his predictive successes, was published as a supplement in *Earth in Upheaval* in 1955. *The Velikovsky Affair* appeared in 1966, which contained, in addition to the main body of the work, an appendix devoted to this topic alone, "Additional Examples of Correct Prognosis," by Alfred de Grazia, and *Pensée* began publishing in 1971. This means that, had Asimov wished to do even a minimum amount of research on this topic, all of this material was available to him. There is no indication, however, that he read any of it, which is what allows him, after discussing *just one* of Velikovsky's successful predictions, (the surprising heat of Venus), to refer to "*that* prediction" and "to hit on a fact," and then to describe all the rest of Velikovsky's predictions, (of which he seems totally unaware), as "five more like it." It is evident that Asimov has not done his homework, he has merely tossed off a facile sneering opinion which repeats what was said by mainstream science 20 years before. His mind seems not to have moved forward since then, and it is upon that foundation of ignorance that he erects his attack upon Velikovsky's predictions.

Step 4. The so-called Velikovsky Affair has been grossly exaggerated. This lies at the heart of Asimov's attack, for the Affair, if true, would be a death blow to Science as Objective Disinterested Truth. Call him a CP, pooh-pooh the Predictions and the Affair, offer pop-sociological (and non-scientific) reasons for his popularity, and he is destroyed. That is Asimov's plan, and we will turn now to a central part of it, his attempt to trivialize and justify the Affair. His argument is so ludicrous that I am forced to reproduce most of it in his own words.

"Some scientists are highly idealistic . . . and 1950 was a bad year for them. McCarthyism was beginning; anti-intellectualism was in the air; scientists were getting a bad press from the super-patriots."¹⁶¹²

That according to him was the socio-political background, and then came Velikovsky.

"Harvard astronomer Harlow Shapley, a strong liberal, had been sniped at by the bully-boys, and to him, Velikovsky's claims of proving the Biblical miracles by a farrago of astronomical illiteracies were something that would simply damage science further."¹⁶¹³

This in his mind not only explains but justifies what occurred next.

"So he exploded, and demanded that Macmillan not publish "Worlds in Collision" as a scientific textbook. If they did, he threatened, he would lead a move to withdraw other textbooks from their list."¹⁶¹⁴

It may have been an error, says Asimov, but it was for a good cause.

"This, alas, was a mistake on Shapley's part. Macmillan capitulated (a mistake on their part) and turned the book over to Doubleday."

¹⁶¹²*Ibid.*, p. 40.

¹⁶¹³*Ibid.*

¹⁶¹⁴*Ibid.*

"That was the extent of the persecution and the attempt at censorship. Wrong though the reaction of some astronomers was, there was no attempt made to suppress the book as a book; merely to withdraw from it any official label as 'scientific.' This was still wrong, but it falls far short of true persecution."¹⁶¹⁵

That's all there was, says Asimov, and everything else is a fabrication by the Velikovskians and their "self-pitying martyred claims." Here is how he portrays it:

"Velikovskians have labored hard to build a legend of one brave man standing single against all the forces of orthodoxy."¹⁶¹⁶

Legends, however, are no match for Asimov the scientist. What he does not realize, however, is that this is just how he had ironically portrayed science a moment ago, as "powerless and few" against the "thousands" of Velikovsky. Which is it, we wonder.

This argument is so ignorant and reductive that we hardly know what to say. Consider the counter-evidence: Alfred de Grazia and his co-authors discovered so many instances of scientific misbehavior, by so many people in addition to Shapley, and over so many years, that they were able to publish *an entire book* on the subject. Velikovsky wrote *Stargazers and Gravediggers*, a 300-page book, just on this topic, and, when I tried merely to *summarize the highlights* of all that material for this volume, I produced an opening chapter over 50 pages long, in which I showed not only that the irrational response persisted into the 1960's, as *The Velikovsky Affair* proved, but that it has continued right up to the present with the same bitterness and animosity, *which is why we have produced this book* on top of all the rest. Here, however, is what Asimov says:

- a) The only actor in the Affair was Harlow Shapley.
- b) He only acted concerning *Worlds in Collision*.
- c) He only acted once, in 1950.
- d) He only acted regarding Macmillan.
- e) He only acted to prevent the book being published as a scientific text.
- f) All the rest is a "legend."

As I said, all one has to do is read the fully-documented evidence presented in the opening chapter of this book to perceive that what Asimov says is grossly untrue. If, however, that evidence is not enough, (because it comes from a supporter), here are a few comments on the Affair culled from a very different source, the work of Henry Bauer, who is a severe critic of Velikovsky. To Bauer, who did a great deal of research on the topic, (unlike Asimov), the Affair is so important and so extensive that he devotes whole sections of his book *Beyond Velikovsky* to it, and the reader will please note that all of Bauer's remarks which I now quote are phrased in the *plural*, meaning *many* people were involved. First, there is a quick demolition of Asimov on Velikovsky.

"I could not find, 25 years after the publication of *Worlds in Collision*, a satisfactory discussion of the merits of Velikovsky's work."¹⁶¹⁷ [*Both of Asimov's articles had by then been published*].

¹⁶¹⁵*Ibid.*

¹⁶¹⁶*Ibid.*

Then comes a precise list of scientific malfeasance.

"Velikovsky's critics were not only ineffective; many of them also behaved offensively. It cannot be gainsaid that literally inexcusable steps were taken to prevent the expression of an opinion: a respected publisher was boycotted, individuals were caused to lose their livelihoods, advertisements for books and journals were refused, previously available meeting rooms were withdrawn."¹⁶¹⁸

"Many of Velikovsky's critics argued tendentiously and untruthfully while accusing Velikovskians of doing just that: acts of intellectual dishonesty on the part of the critics . . . the bulk of the criticism was dogmatic labelling and name-calling, not the reasoned discourse in which scientists claim to partake."¹⁶¹⁹

". . . the ineptitude of Velikovsky's critics and the extreme and unethical behavior of some of them."¹⁶²⁰

"Velikovsky's critics committed blunder upon blunder They 'argued' dogmatically, *ex cathedra*, by analogy, *ad hominem*, with ridicule, in most every other way than by calm, comprehensive discussion of the relevant points."¹⁶²¹

"The tone of *ex cathedra* assurance was laced at times by ludicrous hyperbole Not unrelated to argument *ex cathedra* is the device of argument by analogy Are these arguments? Certainly not reasoned ones Arguments *ex cathedra*, by analogy, and *ad hominem* were spiced with ridicule."¹⁶²²

And the critics made fools of themselves in a number of ways: . . .

"By saying that any verification of Velikovsky's predictions meant nothing, 'since the idea is wrong' By making such sloppy mistakes as misspelling names, misquoting Velikovsky, criticizing points on which established authorities agreed with Velikovsky, and misrepresenting Velikovsky's argument."¹⁶²³

(Asimov is guilty of almost all of this.)

¹⁶¹⁷Bauer, *op. cit.*, p. 180.

¹⁶¹⁸*Ibid.*, pp. 180-181.

¹⁶¹⁹*Ibid.*, p. 181.

¹⁶²⁰*Ibid.*, p. 194.

¹⁶²¹*Ibid.*, p. 210.

¹⁶²²*Ibid.*, pp. 211-213.

¹⁶²³*Ibid.*, pp. 213-214.

"I find in the record that the critics were themselves guilty of many of the things for which they castigated Velikovsky and his supporters . . . Quite inexcusable were the innumerable occasions on which critics of Velikovsky misquoted or misrepresented him. It seems incredible that salient parts of Velikovsky's scenario should not be known to those who comment on these specific points, yet that continues to be the case."¹⁶²⁴

"Velikovsky was criticized for being dogmatic, for claiming to be never wrong. Chapter 8 offers many examples of science writers and scientists who dogmatically declare something to be pseudo-science, and who claim never to be wrong in their declaration."¹⁶²⁵

Just like Asimov. Carl Sagan in particular, whom Asimov had unfortunately singled out as "The most effective critic of Velikovsky,"¹⁶²⁶ comes in for very heavy debunking.

"Velikovsky was often criticized for being too simplistic. So are the statements of Sagan quoted above, and his attempted calculation of the probability of Velikovsky's scenario . . . the translation of the calculated low probability into a judgment that the event will not occur, or could not have, is completely fallacious . . . And Sagan has continued to publish his critique in slightly different variants, none of them taking cognizance of numerous published objections."¹⁶²⁷

Bauer's summary of the Affair is, therefore, quite firm:

"Critics of Velikovsky boycotted a publishing house in the attempt to suppress a book: the Velikovskians have not done that. Critics of Velikovsky were instrumental in the loss of livelihoods by Atwater and Putnam: the Velikovskians have not so harmed anyone. Journals of science have refused to publish articles, letters, and advertisements from supporters of Velikovsky: *Pensée*, *KRONOS*, and *S.I.S. Review* have given space to critics of Velikovsky and have even solicited contributions from them."¹⁶²⁸

This is a very damning assessment. In the face of Bauer's testament, what are we to make of Asimov's version of the Affair? We know that many of the sources detailing the repugnant events in the first phase of The Affair, as I listed them a page or two ago, plus seven or eight subsequent issues of *Pensée*, were available to Asimov in 1974 had he wanted to consult them, for this is a major part of the material Bauer himself used, *yet there is not a single scintilla of evidence of them in Asimov's article*. In my opinion, therefore, only two possibilities arise: if Asimov did not do his homework, he is incompetent, and, if he did, he is a liar. The only question is—Which is true? If I were asked for my *personal opinion*, then, taking into account that Asimov had been an active science writer since the 1950's, and that the subject of the Affair had been well-known in science circles since that time, and that Velikovsky had been getting a much better press in the 1960's following the space-probe and moon-landing results, to the extent that in the 1970's a special session at one of the important AAAS annual symposia was to be devoted to Velikovsky *just when Asimov wrote on Velikovsky*, I consider it highly unlikely that Asimov could not have known many of the details of the Affair. He had at the very least to have been aware of de Grazia and *Pensée*. I would then have to guess that, in my personal opinion,

¹⁶²⁴*Ibid.*, p. 224.

¹⁶²⁵*Ibid.*, p. 225.

¹⁶²⁶*Ibid.*, p. 42.

¹⁶²⁷Bauer, *op. cit.*, pp. 224-225.

¹⁶²⁸*Ibid.*, p. 226.

Asimov is a liar, and, if we seek a model to explain this, he himself unwittingly provides it for us. Remember, I have quoted Asimov accusing Velikovsky of revising the history of ancient Israel only to glorify the Jews at the time that they were being persecuted by the Nazis, so as to raise their prestige after they had been downtrodden (*i.e.*, Velikovsky is accused of distorting the data to suit an agenda). To judge Asimov *by his own theory*, is it not evident that this is just what *he* is doing, that he is deliberately re-writing the history of the Velikovsky Affair to raise the prestige of Science at a time when it is under fierce attack, and that this is *his* true agenda? Given his glaring omission of many damaging things about the behavior of American science he should have known about, this interpretation seems to me to be unavoidable. He is trying to whitewash what his fellow scientists did. Therefore, when Asimov writes that he sympathizes with Velikovsky about his Jewish bias but that this will not make him change his mind about the valuelessness of Velikovsky's ideas, the sentiment rebounds directly against himself:

"I understand, I sympathize but, alas, understanding and sympathy do not convert nonsense to sense."¹⁶²⁹

No, Isaac, they do not, and it is your own words which teach us how to judge you. As you sow, so do you reap, and you come out much the worse for it. Your historical work is nonsense.

5. Defending Happiness

Up to here, we have been considering the performance of Asimov as a mainstream science writer criticizing the outsider Velikovsky, and, when we take note of what he has offered, mistake after mistake, blunder after blunder, our main response is to wonder how he was able to commit so many errors and present so many pathetic ineptitudes in so short a space as one article. There *is* a consistent answer, of course, and it is what I have been advocating throughout this book—that a fear of what Velikovsky's theories imply (that our world is not safe) makes the self-appointed defenders of Science like Asimov (who are the recipients of a 300-year-old dogma that the world is *provably* safe) leap into action to destroy the heresy, regardless of the cost to their intelligence, honor or reputation. They do it because it keeps us happy, and that I say is the central cause of the Velikovsky Affair throughout, (for to believe Velikovsky would make us unhappy), and Asimov is no more than a prime example of it. Should anyone think, however, that I have exaggerated or distorted in my review of Asimov, we can give the final word to Bauer. Here is how he begins his assessment:

"Isaac Asimov misreported several aspects of the Affair, and his mode of emphatic writing with sweeping generalizations gives the reader scant opportunity to choose his own view from the facts at hand."¹⁶³⁰

Bauer then quotes Asimov as saying "if anyone reads *Worlds in Collision* and thinks for one moment that there is something to it, he reveals himself to be a scientific illiterate," to which Bauer responds sarcastically

". . . in the company, no doubt, of assorted scientifically illiterate professional scientists who have given more than a moment's thought (for example, Bass, Burgstahler, Michelson, Miller, Ransom)."¹⁶³¹

Later, referring to the statement by a Velikovskian that Asimov's article "CP" contained 134 mistakes, Bauer adds humorously

¹⁶²⁹*Ibid.*, p. 48.

¹⁶³⁰Bauer, *op. cit.*, p. 231.

¹⁶³¹*Ibid.*

"Perhaps it happened to the right victim: Asimov himself referred to 'hundreds of places where Velikovsky is wrong,' in 'at least fifty more passages . . . from [*Worlds in Collision*],' but he failed to specify by page or quotation any of those 'hundreds' or 'at least fifty.' Perhaps Asimov didn't bother because his estimate was so conservative; after all, Menzel knew of 'thousands of other erroneous suppositions and conclusions.' What an effort must have gone into counting all of them."¹⁶³²

Ultimately, therefore, in Bauer's opinion, Asimov is futile as a critic of Velikovsky.

"Asimov wrote that 'Macmillan planned to publish the book as part of its *textbook* line . . . as a scientific textbook.' That is quite wrong . . . on a significant point."¹⁶³³

"Asimov also said that 'Velikovsky doesn't accept the laws of motion, the law of conservation of angular momentum, the law of conservation of energy and other such trivialities.' That is an irresponsible statement: Velikovsky has never expressed a disbelief in those laws."¹⁶³⁴

In the end, therefore, Bauer raises suspicions about Asimov *identical to mine*.

"As it is, though, Asimov's account is so inaccurate and misleading that he stands revealed of either ignorance or misrepresentation when he writes about the Velikovsky Affair."¹⁶³⁵

The reader may choose either, but in both, Asimov has made a fool of himself.

6. Intermezzo.

In order that the reader may better understand how despicable Asimov's attacks on Velikovsky were, I offer here a summary of a very different piece of criticism. The author is Prof. Robert Jastrow, director of the Institute for Space Studies at NASA, professor of earth sciences (Dartmouth) and professor of astronomy (Columbia). His article was published in *The New York Times* in December, 1979, (page 22E), very shortly after Velikovsky died, and it is a model of polite, objective, reasoned discourse. This is not to say that Jastrow supports Velikovsky, for he does not. Where he differs from Asimov, however, is in his respectful tone, his much greater knowledge and his downright fairness. This piece was written only five years after Asimov's "CP," but the contrast between them is polar.

Jastrow begins with a summary of Velikovsky's major ideas, without sarcasm, without sneering, without innuendo. He is perfectly aware of the major tenets of traditional scientific belief which those ideas contradict, but, unlike Asimov, who merely laughed at Velikovsky, Jastrow saw the possible benefit to science—such ideas, he said, "made his theory even more radical, exciting and potentially fruitful."¹⁶³⁶ He then reviews how Velikovsky's ideas were "received in the scientific community."

¹⁶³²Bauer, *op. cit.*, p. 242.

¹⁶³³Bauer, *op. cit.*, p. 140.

¹⁶³⁴*Ibid.*

¹⁶³⁵Bauer, *op. cit.*, pp. 141-142.

¹⁶³⁶Robert Jastrow, "Velikovsky, a Star-Crossed Theoretician of the Cosmos." *The New York Times*,

"Scientists described him as a 'quack,' 'charlatan,' and 'fraud,' and his work has been called 'rubbish,' 'nonsense,' and 'trash.'"

"*Scientific American* and the American Philosophical Society published attacks on the theory but refused to print Dr. Velikovsky's rebuttals. *Science*, the official journal of the American Association for the Advancement of Science, returned another manuscript to Dr. Velikovsky unopened."

"*The New York Times* said scientists were behaving 'as though they have been stung by a hornet from outer space.'"¹⁶³⁷

Carl Sagan, (Asimov's idol), is singled out for extremely severe criticism by Jastrow, who begins quite ironically, referring to the session on Velikovsky at the 1974 AAAS conference.

"Reporters and scientists agreed that . . . Sagan was the most effective anti-Velikovskian on the panel. *Science* called Professor Sagan's discourse 'amusing and totally devastating.'"

Jastrow, however, presents a very different picture of the tone in which the discussion was conducted.

"A reading of Professor Sagan's paper and the reports on the meeting suggests that the panel and the audience were mocking the 78-year-old man."¹⁶³⁸

After setting Sagan up, Jastrow then delivers the knockout punch.

"Later Dr. Velikovsky had his day when he spotted a major scientific boner in Professor Sagan's argument."¹⁶³⁹

Sagan had calculated that the chance of the events described in *Worlds in Collision* occurring was 1 in 10^{22} , because he erroneously believed that each near-approach was independent,

"Dr. Velikovsky pointed out that the collisions are not independent; in fact, if two bodies orbiting the sun under the influence of gravity collide once, that encounter enhances the chance of another, a fact well known in celestial mechanics."¹⁶⁴⁰

(i.e., Sagan does not know that but ought to have known it, and as a result he stands ironically accused of the very fault he and Asimov attributed to Velikovsky).

(December 2, 1979), p. 22E.

¹⁶³⁷*Ibid.*

¹⁶³⁸*Ibid.*

¹⁶³⁹*Ibid.*

¹⁶⁴⁰*Ibid.*

"Professor Sagan's calculations, in effect, ignore the law of gravity. Here Velikovsky was the better astronomer."¹⁶⁴¹

Jastrow, therefore, admits quite readily, on the topic of the Affair, that "There is no question that Dr. Velikovsky was shabbily treated," but he goes on regardless to ask the correct question—"Who is right? . . . Did Venus collide with the Earth in historic times?" (If only Asimov and his ilk had been able to stick to the point that way).

Jastrow's answer is in the negative, for the reason that "Much of the criticism of Velikovsky is better founded." He acknowledges that "Three predictions based on the theory have turned out to be correct," but adds that "Numerous other predictions turned out to be false" and lists seven which he feels are crucial, in the light of which he feels forced to reject Velikovsky's hypothesis.

"It would be very interesting if the facts were otherwise; nothing could be more exciting than to witness a revolution of scientific thought in our own lifetime. Unfortunately, the evidence does not support this possibility."¹⁶⁴²

Lewis Greenberg (founder and editor of *KRONOS*) and Charles Ginenthal (founder and editor of *The Velikovskian*) both assure me that Jastrow's scientific objections are wrong and can be rebutted, but I have deliberately avoided citing them or the counter-arguments because, as I have been saying throughout the book, that is not the issue here. We are debating the *reception* of Velikovsky's ideas, not their substance and, therefore, even though there is a great difference in *scientific* quality between Jastrow's criticism and Asimov's, it is the difference in behavior which is more relevant here. In a word, Asimov's critique is cheap, shallow, pompous and vain. It is arrogant, simple-minded and scurrilous, and in the end not merely unsuccessful, or even nonsensical, but unethical and malicious. In contrast, Jastrow is reserved, balanced, gentlemanly and scholarly, which is the way scientific debate should be carried on, (but seldom was regarding Velikovsky). Even though, therefore, he may turn out to be wrong too, (according to the Velikovskians), his work nevertheless stands as a rebuke to the ill-tempered meanness of Asimov, for he can be debated civilly and he can recognize merit. To him, for example, even though Velikovsky is judged incorrect, he is not, as he was to Asimov, a fraud and a crackpot and an object of ridicule, but "a man of extraordinary talents" who "devoted his powers of scholarship and intellect to a remarkable thesis."¹⁶⁴³ Asimov was totally unable to ever even remotely approach this generosity of spirit (or depth of knowledge).

7. Asimov as Would-Be Scientist.

To return to Asimov, the first sections have shown that, whenever Asimov confronts Velikovsky, he makes a fool of himself. This is very significant evidence for the Velikovsky Affair, because Asimov is not normally a fool. It is true that he can be shallow and facile, that he runs with the pack and that he is mostly an echo of received opinion, but he is not often ridiculous, yet he routinely ends with egg on his face each time he tries to debunk Velikovsky. This must indicate that at such moments he is consumed by passion, not reason, which is precisely what this book wishes to demonstrate.

In this section, I shall turn from Asimov's pseudo-scientific attacks upon Velikovsky and look at one of Asimov's own attempts to be a scientist. What we shall find, ironically, is that it too serves as evidence for the Affair.

I am referring to the question of why there are no moons orbiting the planet Mercury. As plasma physicist C. J. Ransom reported in the journal *KRONOS*, (II, 3, pp. 81-83), Asimov, in his 1959 book *Of Time and Space and Other*

¹⁶⁴¹*Ibid.*

¹⁶⁴²*Ibid.*

¹⁶⁴³*Ibid.*

Things, offered an explanation derived from the theory of the "Roche limit," the distance within which any satellite of a planet would be broken to pieces by the tidal effects of that planet. Speaking of Mercury, Asimov said,

"The maximum distance at which it can expect to form a natural satellite against the overwhelming competition of the nearby Sun is well within the Roche limit. It follows from that, if my reasoning is correct, that Mercury *cannot* have a true satellite, and that anything more than a possible spattering of gravel is not to be expected."¹⁶⁴⁴

What Asimov argued, as Ransom encapsulates it, is that

". . . inside that limit, a satellite would be broken up by the tidal effects of the planet; outside of Mercury's Roche limit, where the gravitational force of the Sun is much greater than that of Mercury, a satellite would be unable to resist the Solar pull and thus fail to achieve a stable orbit around the planet."¹⁶⁴⁵

Mercury, therefore, has no moons. This was Asimov's theory about Mercury, and what follows is a very interesting phenomenon: once Asimov said it, he immediately treated it as proven, *as if he had done the calculations and they bore him out*. It then became a prediction for which he wants credit.

". . . as far as I know, nobody has endeavored to present a reason for this or treat it as anything other than an empirical fact. If any Gentle Reader, with a greater knowledge of astronomical detail than myself, will write to tell me that I have been anticipated in this, and by whom, I will try to take the news philosophically."¹⁶⁴⁶

He himself, he is saying, considers it to be true, and it appears that, for well over a decade, Asimov's conjecture was apparently *never* re-investigated or proved. As a result, as Ransom recounts it,

"Asimov's fallacious argument influenced nearly a generation of amateur and professional astronomers and physicists. Many would not seriously consider looking for a satellite around Mercury because it had been 'proven' that none could exist."¹⁶⁴⁷

Seventeen years later, Bruce E. Bushman published a letter in *Physics Today*, (February 1976, Vol. 29, No. 2) calling attention to Asimov's prediction. "I want to report an explanation of the fact that Mercury has no satellite."¹⁶⁴⁸ He then put forth Asimov's prediction, treating it as if it had long been proven.

¹⁶⁴⁴C. J. Ransom, "On Mercury Without a Moon." *KRONOS*, Vol. II, No. 3, 1977, p. 81.

¹⁶⁴⁵*Ibid.*

¹⁶⁴⁶*Ibid.*

¹⁶⁴⁷Ransom, *op. cit.*, p. 82.

¹⁶⁴⁸Bruce E. Bushman, "Letter," *Physics Today*, Vol. 29, No. 2, 1976, p. 11.

"Asimov shows the fact that the Roche limit . . . prohibits a moon from existing close enough to Mercury to avoid an appreciable tug-of-war with the Sun's gravitational force."¹⁶⁴⁹

Bushman calls this "a significant theoretical discovery in physics and astronomy,"¹⁶⁵⁰ even though it only came from "a leisurely exercise in a chair,"¹⁶⁵¹ and he wonders how Asimov's fellow scientists feel about it.

Nine months later, the response was in, and it was devastating for Asimov. The Readers were not Gentle, and they displayed a much "greater knowledge of astronomical detail" than Asimov. In the November 1976 issue of *Physics Today*, (Vol. 29, No. 11), William H. Jeffreys of the University of Texas wrote that "Asimov's explanation of Mercury's lack of a satellite cannot be upheld,"¹⁶⁵² and that the error occurs because Asimov *does not understand the nature* of the motion of moons, including our own. He notes, for instance, that Asimov "expresses surprise that the Moon is stable, since the ratio for that body is 0.46, much less than the value of 30 typical for other satellites in the solar system"¹⁶⁵³ (*i.e.*, our Moon's ratio is over 70 times less than the average, which should mean not only that Mercury can have no moons, but that *our Earth should not have one either*). Jeffreys' opinion on this matter is quite different from Asimov's. "There is, in fact, no problem with the stability of our own Moon," says Jeffreys, because "The acceleration of the Moon *relative* to the Earth due to the Sun is . . . a tidal acceleration,"¹⁶⁵⁴ which Asimov seems to have completely misunderstood.

"This is seen very clearly when the full equations of motion are written out."

—implying that Asimov had not done that—

"for which consult a standard text such as Brouwer and Clarence."¹⁶⁵⁵

Asimov is being lectured to like a child. He is being told to at least do the fundamental homework first, as if he does not even know the standard beliefs found in standard textbooks in the field, which is what leads to his errors. In direct opposition to Asimov, therefore, Jeffreys asserts that our "Moon is well within the stable region," and, as far as moons of Mercury are concerned,

"It is clear that there is a large region where a satellite of Mercury could have a stable orbit."¹⁶⁵⁶

The put-down could hardly be more complete.

In the same issue, a second letter was published, by Joseph G. Burns of the NASA Ames Research Center in California, which similarly debunks Asimov's explanation as ridiculous—"if this were true, NASA would not even

¹⁶⁴⁹*Ibid.*

¹⁶⁵⁰*Ibid.*, p. 12.

¹⁶⁵¹*Ibid.*

¹⁶⁵²William H. Jeffreys, "Letter." *Physics Today*, Vol. 29, No. 22, (November 1976), p. 92.

¹⁶⁵³*Ibid.*

¹⁶⁵⁴*Ibid.*

¹⁶⁵⁵*Ibid.*

¹⁶⁵⁶*Ibid.*

consider a Mercury orbiter."¹⁶⁵⁷ Burns' explanation for the lack of Mercury moons involves the slow axial spin of the planet, which must eventually pull a satellite inward or downward until it is eliminated by plunging into the larger body. Asimov, says Burns, is not even close, as "a simple calculation shows,"¹⁶⁵⁸ (implying that Asimov had failed to do this simple work). "A more likely explanation," writes Burns, (implying that Asimov's explanation is wholly *unlikely*), "is the action of solid-body tides," (which Asimov seems ignorant of). Asimov's errors are therefore shown to be elementary and obvious, and his theory nonsense.

"Satellites of Mercury thus have the vast range between the Roche limit and the outer boundary in which they can reside safely without suffering the fates proposed by Asimov."¹⁶⁵⁹

A third letter, by J. J. Condon of the Virginia Polytechnic Institute, is equally damaging. Condon pointedly tells Asimov that "The Roche limit is calculated for a *fluid* body," not a solid one, and, therefore, that "a solid moon within the Roche limit could be held together by the cohesive strength of the material composing it,"¹⁶⁶⁰ (*i.e.*, Asimov does not know what he's talking about). The only effect would be that such a Moon "would probably be irregular in shape," which means that "Asimov has not shown that Mercury could have no moon, but only that it can't have a big round one!"¹⁶⁶¹ This is not at all what Asimov had wanted to prove.

These are schoolboy mistakes, fundamental errors, serious "boners," yet look at how it was handled by *Physics Today*. In the same issue of the journal, immediately following these devastating critical letters, appeared Asimov's short and feeble reply. "My original article on the subject was published some time ago," he says, and "I discovered the error in my analysis."¹⁶⁶² When Bushman's letter appeared, "I wrote to Bruce Bushman promptly and told him I was wrong." He then concludes "May it be the only time that I am to find myself egregiously wrong, but I strongly suspect it won't be."¹⁶⁶³ (On that prediction he would be devastatingly correct).

What a transparent tissue of self-serving lies his response is! Are we to believe that Asimov discovered his own error in 1959 and then waited 17 years to publish it? Or, to go to the opposite extreme, that he had found it *just before* Bushman's letter was published in 1976, which would enable him to write to Bushman "promptly," *already knowing* of his own error? This is nonsense. Are we also to believe that the three destructive letters of response all arrived at the same time, as if they had gestated together for nine months? More so, even if they did, why did Asimov wait *until they were about to be published* in November before he wrote his retraction? Why did he not publish it in *Physics Today* immediately after Bushman's letter in February, if he were already aware of his errors through his own analysis? Does anyone consider this behavior "scientific"? Or plausible?

I hardly think so. It is unlikely that the three refutations arrived at *Physics Today* at precisely the same time. What is more likely is that the journal withheld publishing them until Asimov could frame a suitable reply. It was, that is to say, considerate and humane treatment of an insider. Here was one of the few times in his life that Asimov had tried to be an original scientist, rather than a teacher of science or a popularizer of science dogma or a writer of science fiction, and he had failed miserably. He was caught with his pants down, in public, but look how gently his chums at *Physics Today* let him off the hook. Having been exposed as guilty of a major howler, (to use the sort of sneering language Asimov applied to Velikovsky), he is given what hardly amounts to a gentle slap on the wrist. He is allowed to say "I discovered the error in my own analysis," which implies that he is as good as the professional physicists and astronomers who castigated him, that he doesn't need them to teach him science, that he is as good as they are and that he found his error first. He is then permitted to acknowledge the error in the journal with no questions raised as to why it was not

¹⁶⁵⁷*Ibid.*

¹⁶⁵⁸*Ibid.*

¹⁶⁵⁹*Ibid.*

¹⁶⁶⁰*Ibid.*, p. 93.

¹⁶⁶¹*Ibid.*

¹⁶⁶²*Ibid.*

¹⁶⁶³*Ibid.*

admitted earlier, and lastly he is given the opportunity to end the debate with a modest self-deprecatory joke, *i.e.*, to "always leave 'em laughing," so as to deflate forever any residual criticism of his previous behavior. *Physics Today* could hardly have been more helpful and decent.

To an objective observer, however, the whole thing must seem patently implausible, for there is no evidence that Asimov ever tried to prove his prediction. On the contrary, his own words in 1959 belie him, for, from the moment he came across his idea, *he seems never to have doubted it*. It is true that he begins with the qualification "if my reasoning is correct,"¹⁶⁶⁴ and in the next paragraph he states only that he "has endeavored to present a reason for" Mercury's lack of satellites,¹⁶⁶⁵ but within one sentence these perfunctory gestures of diffidence have changed to words of blind certainty. Consider this sentence, which I have quoted before. It begins

"If any Gentle Reader, with a greater knowledge of astronomic detail than myself, will write to tell me—"

One expects, if the man is a true scientist, that his next words would be "That I am wrong," after which, like the model scientist he thinks he is, he would "try to take the news philosophically." What we get in contrast is only a reference to *priority*, as if the idea had already been unequivocally established and it was only a matter of who thought of it first:

". . . will write to tell me that I have been anticipated in this."

Priority is all he fears. He never doubts in print that he could be wrong, only late. Like a one-man prosecutor, judge and jury, he takes himself at his own word without hesitation or question as absolutely correct.

Bushman displays the same unquestioning attitude toward Asimov that Asimov does. In his letter, he does not ask for respondents to assess whether the prediction is true, (which would have been the correct scientific procedure), but only "whether this explanation . . . has been anticipated."¹⁶⁶⁶ That is to say, he takes it for granted that the prediction must be correct *because it comes from Asimov*, and accepts it without citation or proof, which he does not even ask for when he sees none has been provided. (So much for the myth of scientific verification). The concept was believed because of the power of Asimov's name, and we have to ask ourselves how much of science is done this way, with belief founded on reputation.

We next have to consider what the consequences should be of Asimov's failure. In the article "CP," Asimov had felt free to label Velikovsky a "CP" because of the alleged faults in Velikovsky's scientific argumentation, to which Velikovsky continued to adhere. As Ransom observes, however, *if both men were to be treated equally*, then this enormous blunder by Asimov, which he defended for close to two decades, "would end Asimov's career as a serious thinker and label him as a 'CP'."

"For years, opponents of Velikovsky have sought a flaw in some portion of his work so that they could . . . claim that it proved that everything Velikovsky wrote was wrong."¹⁶⁶⁷

Asimov certainly believed in this tactic, and, as Charles Ginenthal has shown, this is precisely how Henry Bauer has approached Velikovsky too. Ransom adds, however, as does Ginenthal, that

¹⁶⁶⁴Isaac Asimov, "Of Time and Space and Other Things." 1959, p. 96.

¹⁶⁶⁵*Ibid.*

¹⁶⁶⁶Bushman, *op. cit.*, p. 13.

¹⁶⁶⁷Ransom, *op. cit.*, p. 82.

"During those same years, these opponents made very serious mistakes related to their own theories . . . They ignore their own mistakes, though, and assume that if Velikovsky ever makes one mistake, it proves that he is a charlatan, unscholarly or both."¹⁶⁶⁸

Must we, therefore, not apply this rule to Asimov too, as he has done to Velikovsky, and say that, if Asimov is so wrong about Mercury, then we have to dismiss *all* of his books as incorrect? No, we should not, for one wrong is not righted by a second. It was unjust to dismiss Velikovsky for such a reason, and it is equally unjust to do that to Asimov, (however much fun it might be). Each of Asimov's contentions must be allowed to stand on its own, as must Velikovsky's. What we can do, however, is use mainstream science's response to Asimov's Mercury fiasco as further proof for the Velikovsky Affair.

Consider the astonishing contrast between how Asimov and Velikovsky were treated by mainstream science in similar circumstances. When Asimov published his Mercury prediction, it was hailed at once by the fellow members of his club, for this demonstrated that he was not merely a science writer but could now be described as a serious scientist too. (He was one of them). So much was made of it that

". . . when, in 1974, Mariner probe data were misinterpreted to leave the impression that a satellite of Mercury had been discovered, a physics student at the University of Ottawa told me he did not believe it, because Asimov had 'proven' this to be impossible."¹⁶⁶⁹

Velikovsky, as I outlined in the first chapter, published close to 70 predictions, not merely one, *almost all* of which have turned out to be correct, yet none of these has been greeted with instant unquestioning acclaim by mainstream science. On the contrary, they were all instantly dismissed as irrelevant to his reputation and to the validity of his large theories, *even though none of them was wrong individually*, and it is the radical disproportion between these different responses by mainstream science to predictions made by an insider and an outsider which is of interest to us.

Let us take, as a quick example, Velikovsky's predictions concerning the Moon. Because he believed that the Moon had to be involved in the cataclysms which he postulated, and had therefore suffered detectable physical and chemical effects, he offered a set of predictions of what might be discovered on that cosmic body. Each of the phenomena he expected derived from his general theory and should, therefore, serve as evidence for it if it were found.

"All of these suggestions were made before the first manned lunar landing, and many of them before anyone thought seriously of going to the Moon."¹⁶⁷⁰

Every one of these predictions was extremely radical, in that *all* of them "defied prevailing opinion about the Earth's natural satellite."¹⁶⁷¹ It was, therefore, extremely courageous of Velikovsky to risk the validity of his theory on these predictions which mainstream science considered impossible or unlikely at the time he made them, yet that is what he did with boldness and determination.

Let us review the most important ones.

¹⁶⁶⁸*Ibid.*

¹⁶⁶⁹*Ibid.*

¹⁶⁷⁰C. J. Ransom, *The Age of Velikovsky*, KRONOS Press, 1976, p. 142.

¹⁶⁷¹*Ibid.*

- a)*Remanent magnetism.* If a powerful external magnetic field alters the magnetic alignment of particles in a substance in the presence of temperatures hot enough to melt the substance, the new alignment will *remain* in the substance after cooling. Science did not expect this on the Moon, but Velikovsky did, reasoning that some sections of the Moon's surface must have cooled while still influenced by the large external magnetic fields of Mars or Venus. When this was found, mainstream science tried to explain it in a number of ways *except Velikovsky's*, but even that arch anti-catastrophist, Nobel geochemist Harold Urey, wrote in 1973 that the remanent magnetism on the Moon was "One of the most unexpected discoveries of the Apollo program."¹⁶⁷²
- b)*Thermal gradient.* Science held the Moon to be a cold and long-dead body. Velikovsky on the contrary believed that the Moon had only *recently* been subjected to interplanetary catastrophe and must, therefore, still be appreciably warm, and he predicted that measurements would indicate that heat flows from the interior out to the surface *even at present*. This was discovered.
- c)*Argon.* Velikovsky believed that "the interactions of the Moon with Mars could have left argon in excessive amounts on the Moon; and this would then yield unusually high ages for samples dated by the potassium-argon method."¹⁶⁷³ This is just what occurred when moon rocks were first brought back to Earth. The excess of argon was "unexpected" and made dating "experimentally uncertain." Dates yielded were "embarrassingly high" and often "it has not been possible to calculate a realistic age for the sample."¹⁶⁷⁴ Even more puzzling was the abundance of Argon 40 compared to Argon 36, which does not agree with the traditional view but "is consistent with Velikovsky's reconstruction."¹⁶⁷⁵
- d)*Carbides.* Velikovsky felt that the interaction between the Earth and the huge tail of Venus when it was a cometary body or proto-planet caused hydrocarbons to rain down on Earth, (leading, for instance, to his theory about *manna*). If hydrocarbons also fell on the Moon and were subsequently heated, then carbides may have been formed. Both of these substances have been found on the Moon.
- e)*Radioactivity.* Velikovsky reasoned that, if giant electrical discharges had occurred between Mars or Venus and the Moon, then there might be "localized hot-spots of radioactivity" on the Moon, particularly near the crater Aristarchus.¹⁶⁷⁶ These hot-spots were consequently discovered by the lunar-lander probes, just as Velikovsky had predicted.

"Gamma-ray spectrometer measurements made by Apollo 15 and 16 instruments indicted that the Aristarchus region was one of three locations showing enhanced radioactivity."¹⁶⁷⁷

Here are five predictions made by Velikovsky, each of which proved to be correct. There is no doubt about his priority, for the statements are all documented, first in *Worlds in Collision*, then in several memos to H. H. Hess, (1967, 1969), who was Chairman of the Space Science Board, and especially in an article in *The New York Times* (1969) which was published *just before* the first landing on the Moon, in which Velikovsky pointedly listed the unusual phenomena which he expected would be discovered. What is important (as even Bauer recognized) is that he made these predictions well before there were any indications that could lead to them.

¹⁶⁷²Ransom, *op. cit.*, p. 144.

¹⁶⁷³Ransom, *op. cit.*, p. 146.

¹⁶⁷⁴Quoted in Ransom, *ibid.*

¹⁶⁷⁵*Ibid.*, p. 147.

¹⁶⁷⁶*Ibid.*, p. 154.

¹⁶⁷⁷*Ibid.*

"Little or no observational data about these characteristics were available at the time Velikovsky wrote, and one could, therefore, regard these statements as predictions of what would be found."¹⁶⁷⁸

They are original and inventive.

Consider now how mainstream science responded. Velikovsky had published close to 70 predictions, each of which was quite revolutionary. Almost all of them have turned out to be correct and most were openly called "unexpected" or "surprising." They were nevertheless *all* dismissed as "lucky guesses" providing no support for his overall theory and, therefore, he continued to be called a pseudo-scientist and ignoramus. Asimov published only *one* prediction, but it was immediately accepted as true even though no proof for it was ever advanced. It is not difficult to guess at the cause. Despite their success, Velikovsky's predictions were pounced on, faulted and denied because he did not share the prevailing dogmas of mainstream science, whereas Asimov's assertion was accepted without question, without doubt and *without independent verification* simply because he shared the cherished dogma. As a result, Velikovsky was rejected as a crank despite his successful record, whereas, when Asimov was proved to be very wrong, *Physics Today* glossed it over as a triviality, with hardly a murmur and with hardly any damage to Asimov's reputation. No one called Asimov a charlatan or a fraud, no one labelled him a CP, and, most important of all, no one insisted that all his writings were false and that he be expunged from the scientific community. Moreover, Asimov himself was as guilty as any scientist concerning Velikovsky's predictions, as can be seen from his mean, niggardly estimate of Velikovsky's success.

"This is not to say that some of Velikovsky's 'predictions' haven't proved to be so However . . . if anyone wants to take credit for Velikovsky's lucky hits, they had better try to explain the hundreds of places where he shows himself not only wrong but nonsensical."¹⁶⁷⁹

Leaving aside the question of what these "hundreds" of nonsenses were, (because Asimov never bothers to cite them), it is hard not to think that, had Asimov published any *one* of Velikovsky's predictions about the Moon, which were so daring and so original, the response would have been adulation, while Velikovsky's record of dozens of correct predictions was almost never allowed to be even *mentioned* in the pages of mainstream journals. In this way too, the relation between Asimov and Velikovsky stands as evidence for the Velikovsky Affair, and I hardly think he would find this much grounds for laughter.

8. Asimov as Science-Fiction Writer

There is yet an even deeper irony to the story of Asimov. In the first part of this chapter, (sections 1 to 4), we looked at Asimov the science *writer* and found that he disproved *himself*, not Velikovsky. Then, in the next part, (section 7), we looked at Asimov the would-be *scientist* and found that the way in which he was treated, compared to what happened to Velikovsky, ironically provided more evidence for the Velikovsky Affair. Now, in this eighth section, we will look at Asimov the creative artist, the writer of science *fiction*, and we will find that, here too, in the third of his occupations, he ironically serves up even more powerful evidence for the Affair. We shall discover that, in each of these roles, Asimov unintentionally becomes a symbol, an exemplar, of a culture (ours) which *cannot admit catastrophe*.

In order to substantiate that, I must begin by outlining my theory about the self-protective traumatic origin of art and science, which derives of course from Velikovsky. My theory has been presented in a number of articles, and can be summarized as follows: it is my belief that we are a collectively traumatized species, in that we have in us somehow (whether genetically or culturally or both) a racial memory of those times when the Earth was subjected to the immense blows of global cataclysms—sudden darkness and fire falling from the sky and whole forests ablaze and irresistible

¹⁶⁷⁸Bauer, *op. cit.*, p. 15.

¹⁶⁷⁹Quoted in Bauer, *op. cit.*, p. 51.

floods and tremendous explosions and oxygen deprivation and fierce winds and terrifying sounds and widespread mutilation and death. Because (if Velikovsky is right) these events seemed to be occurring everywhere, it would have seemed to us (each time a giant catastrophe occurred) as if the entire universe had become disordered, which is a terrifying awareness. It is Velikovsky's concept that we all are the inheritors of this knowledge, which means that the human race continues to be a victim of the catastrophic traumas, just like those who experienced them originally.

We must then ask what sort of compromise have we effected with reality that allows us to function in our daily lives despite our unconscious awareness that at times normal reality goes haywire, with frightening and devastating results. There are in my opinion only two major classes of response discoverable in human society. The first, typical of most Eastern and Oriental cultures, is to acknowledge the reality of catastrophe by incorporating a belief in cyclical world destruction into religion, (as in Buddhism and Hinduism). Contrary to that is the second type, the reaction of Western culture, which is the denial of catastrophe.

That I believe is what we in the West seem to have collectively done, just like individual survivors of a great trauma also do—we try to deny the horrible memories. There is a problem with this tactic, however, because, if the memories are very powerful, they are not easily or permanently suppressible. We may think that we put them away in dead storage, so to speak, when we repress them into the unconscious, but, as Freud warned, many sorts of everyday anxieties could threaten to awaken the buried terror and bring it to the surface, the conscious. We, therefore, have to evolve a method for pushing the fear back down each time it threatens to emerge.

In my opinion, (following Velikovsky), we achieve that goal through the combined effects of science and art, both of which we have constructed. That is to say, science, (or the conscious part of our collective psyche), denies catastrophe through alleged reason, while art, (or the collective unconscious), revisits the catastrophes, but always in disguise and each time with a happy or at least closed ending. In this way, we are allowed to believe consciously not only that catastrophes *do* not occur but cannot, while subconsciously we return to the trauma, as Freud said we must. The process is very attractive, for, in this *compulsive repetition*, (as Freud called it), we either alter the roles to make ourselves the aggressors and not the victims, or we end the story in reconciliation and rebirth, or both. It is the best of both worlds if it works.

I will discuss this theory more extensively in my last chapter, where I argue that the urge to admit, versus the powerful urge to deny, is the foundation of the battle between organized science and catastrophic theory. To sum it up concisely here, I think that we must reverse the popular notions which our society has of art and science, which is to say, the idea that science produces truth while art produces lies. That is a concept which has been created by science to serve itself. In my opinion, (as I have tried to show in many articles), the contrary is more accurate—science *in its largest sense* proceeds via small truths about nature to a big lie about the world, whereas art proceeds by means of lies or artistic creations to big truths about the world. In this sense, the ultimate picture which classical science gives us of the world is false, while the deepest symbolic layer of art recreates that which is true, and it is only by means of this joint function of art and science together that Western culture can survive without going crazy. This does not mean that we are collectively sane. On the contrary, we *have* to be crazy in that we live like neurotics in denial and substitution, but out of this craziness we are enabled to function in ordinary life, just as a successful neurotic might. The only way we can escape from this condition of collective neurosis would be to accept consciously that the catastrophes occurred, which would liberate us from the trauma, but, as I explain in my final chapter, as long as we cannot do that, we will continue to be the victims of our collective past. That is the condition of Western culture.

In a traumatized individual, some part of the total organism (we do not know which) composes the conscious denial, while a second part (also unidentified) creates the unconscious remembering. We take this for granted in individual psychology, even though we have not located the mechanisms. In our society as a whole, it is my opinion that science is the part of us which does the denying while art does the remembering, and that is why we have created them. Neither is autochthonous, (*i.e.*, growing of its own accord). We have made them both, which has always been admitted about art but is only now coming to be understood about science. They are equal productions of the human mind, created for equally self-serving purposes.

In most instances, different individuals and groups perform these separate roles for the whole society—there are scientists and there are artists, and, as C.P. Snow lamented, seldom do the twain meet. In the case of Asimov, however, it will be shown that they not only meet but merge. That is to say, our society has a multiple personality, with the left hand (or art) ignorant of what the right hand (or science) is doing, because a trauma survivor who cannot accept the truth can only continue to exist if the denial function is not known by the repetition function. They must be kept apart. In

Asimov, however, we shall find that the two functions co-exist in mutual ignorance—everything he denies consciously as a scientist he creates subconsciously as an artist. Because he is unaware of what he is doing, he becomes in his single self a living breathing illustration of the paradoxical catastrophic response which our society, as a whole, has developed and, therefore, *in this dimension too* he unknowingly ironically supports the very idea (catastrophism) that he wants fervently to reject.

To demonstrate this, let us turn to an examination of Asimov's fiction. Physicist Robert W. Bass, whose specialty is celestial dynamics, is very well qualified to assess Velikovsky's planetary theories. In a 1975 article entitled "Can Worlds Collide?,"¹⁶⁸⁰ he poses the issue in terms of Asimov's criticisms.

"Is Velikovsky the world's greatest archaeoastronomer? Is he a prescient psycho-historian? Or is he, as Isaac Asimov has argued . . . a tragically self-deluded zealot . . . vainly seeking to rationalize the physically impossible?"¹⁶⁸¹

Before dealing with the question of the stability of the Solar System, (on which the whole *scientific* issue depends), he comments on Asimov's fiction *vis-à-vis* Asimov's attacks on Velikovsky.

"Ironically, the 'young Asimov,' at least in his fiction, was far more tolerant of Velikovskian theories. In . . . Dr. Asimov's best novel, *Pebble in the Sky*, the Good Doctor in 1950 portrayed a *lone archaeologist* whose wild theories about Earth's history not only outraged the establishment but left him in the position of being quite literally the only thinker in the entire galaxy who had surmised the actual truth."¹⁶⁸²

Where did this solitary, ostracized scientist find his evidence and his first insights?

". . . the essential clue to this truth had to be dredged out of ancient religious writings."¹⁶⁸³

This is precisely like Velikovsky, and it is not the first or only time Asimov *as writer of fiction* has come up with Velikovskian themes, as can be seen in the acclaimed 1941 short story *Nightfall*, in which

"The Good Doctor in his early period treats us to the vivid spectacle of an unusual cosmic alignment . . . which, once every 2050 years, brings about the destruction of an entire planetary civilization and simultaneously induces a collective amnesia."¹⁶⁸⁴

Bass is referring here ironically to Velikovsky's theory of collective amnesia, in which, after the terrifying experiences of huge catastrophes have been buried in religion, myth, art and folklore, later generations are not able to recognize the historical content of these creations because the conscious memory has been suppressed by an amnesiac forgetfulness and avoidance. It is amusing to Bass, therefore, that Asimov should have unknowingly put this Velikovskian concept

¹⁶⁸⁰Robert W. Bass, "Can Worlds Collide?" *KRONOS* 1.3, (Fall 1975), pp. 59-72.

¹⁶⁸¹*Ibid.*, p. 59.

¹⁶⁸²*Ibid.*

¹⁶⁸³*Ibid.*

¹⁶⁸⁴*Ibid.*

into a piece of his own fiction. Bass recognizes, of course, why Asimov-the-scientist later made a career switch away from these sorts of ideas.

"Of course, now that Asimov has evolved into a pop-oracle who prolifically purveys received opinion on every subject . . . he has seen fit to modulate his youthful boldness and close ranks with orthodox academe."¹⁶⁸⁵

Bass is, therefore, highly suspicious of Asimov's real beliefs, and, like Ginenthal, is sad to have to reprimand him for his academic misbehavior, but feels it is scientifically necessary.

"I am a great Asimov fan and eagerly devour most of his writings with enjoyment and profit. What I demur from is his tendency in recent years to portray holders of conclusions which are presently only minority views . . . as academically deficient, incompetent scholars."¹⁶⁸⁶

Bass tries to end the matter pleasantly by inserting a conciliatory joke in a footnote.

"I have decided to take a charitable view of Isaac's excesses and to repeat to myself . . . that when the world's leading science popularizer referred to Immanuel as 'CP' he was unconsciously trying to acknowledge him as a 'Champion Philosopher.'"¹⁶⁸⁷

To Bass, Asimov's fiction is evidently catastrophic.

What is glaringly absurd about Asimov, therefore, is that, while he excoriates Velikovsky for catastrophic notions, he cannot see the mote in his own eye. Lewis M. Greenberg recognized this in his article "Phobia, Amnesia, and the Psyche," where he begins with a quote from Asimov himself which describes his phobic split-mindedness pointedly.

"There never can be a man so lost as one who is lost in the vast and intricate corridors of his own lonely mind . . . There never was a man so helpless as one who cannot remember."¹⁶⁸⁸

Greenberg, too, refers to collective amnesia, a theory which Velikovsky put forth "to reasonably explain Mankind's inability to consciously remember its catastrophic experiences,"¹⁶⁸⁹ and then sets out to analyze two Asimov short stories in the light of trauma and amnesia. The first, which Bass also treated, is *Pebble in the Sky*, in which the protagonist is transported without warning to a different planet and into the distant future. He still remembers his old existence on Earth, however, and therefore fears that he is an amnesiac, but after much effort he finally learns where and *when* he is. This panics him, and he retreats almost to amnesia, escapes by reflection but begins to revert once more when he is led to wonder why he was sent off from Earth. Was he a dangerous criminal there, or an official whose guilt could not be

¹⁶⁸⁵*Ibid.*, pp. 59-60.

¹⁶⁸⁶*Ibid.*

¹⁶⁸⁷*Ibid.*, p. 60.

¹⁶⁸⁸Lewis M. Greenberg, "Phobia, Amnesia and the Psyche." in *KRONOS*, Vol. I, No. 1, (Spring 1975), pp. 21-26.

¹⁶⁸⁹*Ibid.*

exposed? Was his recourse to amnesia on the new planet "the method . . . to escape the realization of some tremendous guilt?"¹⁶⁹⁰ (It is Velikovsky's theory that a collective feeling of guilt in us for having provoked the catastrophes, and a consequent desire to appease Heaven so they will never recur, is the origin of religion.)

The protagonist, who alone knows the truth about the history of humankind, finds the new planet gripped by forgetfulness. Almost no one remembers the past, and few are prepared to believe that their race came from the planet Earth, which is considered a mere "pebble," a "pigpen" infested with radioactivity. What's more, scientists on the new planet can even conveniently "prove" that humankind evolved somewhere else and that nothing catastrophic occurred on Earth, even though it is described as that "radio-active hulk."¹⁶⁹¹ As Greenberg sums it up:

"The devastation and horror . . . caused the terrestrial survivors as well as the interstellar colonists to *forget* their own cultural and genetic heritage."¹⁶⁹²

A feeling that the Earth was *guilty*, says Greenberg, and a desire *not to remember this*, has shunted the true past into oblivion.

"Mankind has undergone an experience which wiped out . . . the very memory of its terrestrial origin. Mother Earth has become its own unwanted step-child."¹⁶⁹³

Collective amnesia, collective suppression, guilt, false scientific proof, it is all here in Asimov, just like Velikovsky.

The second story is "Nightfall," (1941), where the six-sunned planet called Lagash is plagued by a cycle in which each culture that develops is destroyed by unexplained fire that leaves no memory behind. A small group, the Cultists, say in their *Book of Revelations* that the destructions occur when their world is enveloped in darkness every 2050 years. When the light of the suns disappears, say the Cultists, stars are seen for the first time, and the sight is so unnerving that the people turn into brutes and destroy their civilization. The scientists pay no attention to these "religious" stories, however, but no "scientific" clue to the recurrent disasters is available to them until a surprised group of astronomers discovers that the theory of gravitation cannot account for the motions of Lagash in its solar system. These *scientists*, after many failed attempts to derive mathematical theories to explain this difficulty, finally call in the head Cultist, a *theologian*, who tells them what science did not know, but which religion was aware of all along: that there exists a companion planet, non-luminous and unseen, which periodically perturbs the system, at which time a giant planet-wide eclipse occurs, plunging all of Lagash into total darkness while the Moon increases many times in apparent diameter and the rest of the sky becomes glaringly visible. (The parallels with Velikovsky are self-evident).

Because the time for the next great eclipse is approaching, says the Cultist, certain segments of the population and the sacred books have been sent into hiding because it is expected that the total blackness will drive the people "mad, completely and permanently!"¹⁶⁹⁴ Those exposed to the sudden, never-before-experienced darkness would crave light and consequently set everything afire, and he warns that the Observatory must be preserved. A rebel cultist, however, tries to destroy the scientists' cameras set up to record the event, accusing the astronomers "of undermining religious faith by presenting scientific backing for Cultist beliefs,"¹⁶⁹⁵ (just like the Velikovsky Affair), and the sacred book from which he proclaims passages as the eclipse occurs is dismissed by the resident psychologist as "a mass of distortion, even if it is

¹⁶⁹⁰*Ibid.*, p. 22.

¹⁶⁹¹*Ibid.*

¹⁶⁹²*Ibid.*, pp. 21-22.

¹⁶⁹³*Ibid.*, p. 21.

¹⁶⁹⁴Quoted in Greenberg, p. 24.

¹⁶⁹⁵*Ibid.*, p. 25.

based on fact,"¹⁶⁹⁶ just like Asimov. When the light is almost gone, panic develops in the streets, as prophesied, and then, in the total darkness, the brilliant array of Stars are seen for the first time, vast and numerous.

"Thirty thousand night suns shone down in a soul-searing splendor . . . cold in its awful indifference."¹⁶⁹⁷

Everyone is struck with awe, terror and insanity and *the knowledge that up until then they had not known anything*. They fear that the darkness will last forever, and they set fires to give light, and soon their civilization will be destroyed as the previous ones were.

The similarity of these themes to what Velikovsky presented in *Worlds in Collision* is hard to believe. In these stories we find that Asimov has anticipated not only the major elements of Velikovsky's *planetary* theory but also, very surprisingly, much of what took place in the Velikovsky Affair—*i.e.*, the lone maverick who knows the truth which is hidden in a despised religious book, the periodic destructions accompanied by eclipses and total darkness, the Earth as a dunghill of devastation, the hidden but nagging sense of guilt and a desire for reconciliation with the forces of the cosmos, the sudden enormous bodies seen looming in the sky, the collective panic and frenzied self-destruction, the "proof" that none of these things ever occurred and of course a collective amnesia regarding the records of them that have survived. It is as if, despite all of Asimov's conscious attempts to destroy Velikovsky *and* catastrophism, he unconsciously sensed deep down every dimension of what turned out to be the Velikovsky story, scientific, religious, social and psychological.

No matter how uninclined to psychologizing we might be, this astonishing closeness between Asimov and Velikovsky must make us suspect that Asimov is so bitter in his conscious attacks on Velikovsky because he unconsciously harbors those same ideas. In the words of Hamlet's mother Gertrude, we may say he "doth protest too much": *i.e.*, like the Queen, he is trying to destroy concepts which are threatening to destroy him.

To be fair to Asimov, however, I will add one further point here. When we notice the virtual identity of Asimov's fiction with Velikovsky's theories, it is almost as if Asimov were *reading* Velikovsky when he wrote those stories. "Nightfall," however, was composed in 1941 and, therefore, we should more properly say that Asimov on certain points of catastrophic theory has actually *anticipated* Velikovsky and should be given *priority* over Velikovsky, so closely does his fiction correspond. Unlike his unfortunate theory about Mercury's lack of satellites, this would be a priority he has richly earned, and I for one would be happy to support it.

9. Final Evaluation

Thus endeth our exposition of Asimov, and we will now review what we have found. The first point we must take note of in the Asimov phenomenon is his foolishness. He is not normally stupid, but he is stupid concerning Velikovsky. If we were mean-spirited, (*i.e.*, like Asimov), we could now simply say that, if the man has so misunderstood Velikovsky's ideas, if the man has so misunderstood the Velikovsky Affair, if the man is so ignorant of the history and philosophy of science, then he is a fool and we may dismiss him out of hand. After all, when he tells us he has the infallible litmus test for good and bad science, and knows how to apply it perfectly, and then merely objectively reports the results, is he not aware of his own foolishness? Does he not know that he made a fool of himself scientifically, that he made a fool of himself as an alleged expert on CP-hood, that he made a fool of himself as a historian of science? Yet he seems to parade his folly eagerly, as if he cannot help himself, as if he cannot stop, and that is the reason we must do more than merely dismiss the pompous Asimov. We must ask *why* he behaves this way, why he shreds himself as a would-be scientist, why he goes out of his way to be a fool in public, unstopably and unaware. (After all, no one *forced* him to publish "CP").

¹⁶⁹⁶Quoted in Greenberg, p. 25.

¹⁶⁹⁷Quoted in Greenberg, p. 26.

As a start, I would guess that this man is not really laughing in scornful amusement, but is angry, and rage leads him, obliterating sense and common sense and manners. He is driven by moral anger, and therein lies the solution to the deepest cause of the Velikovsky Affair, for he displays in 1974 the same blind outrage and self-destructive lack of reason that I have catalogued in the first chapter. The data there, however, dealt with the period closer to 1950, whereas Asimov is writing 25 years later, indicating that the disease has persisted unabated and, as such, it stands as strong evidence both for the origin and for the continuing existence of the Affair. And Asimov has ended up only proving Velikovsky.

Everything else we have discovered about him leads to the same conclusion. For instance, we have evaluated him wearing all of his hats, as *scientist*, as *science writer*, as *sociologist* of science and as *science fiction* writer, and the results have been dramatic. In a word, we have seen that in the first three roles, where he believes himself to function as the very Voice of Science, he displays a pathological aversion to catastrophic concepts, whereas, in the fourth role, acting as himself, he espouses them openly and fully. This disparity within himself, far from demolishing Velikovsky as he admittedly intended, only strengthens the argument for catastrophism, for it reveals something very significant about himself, which can be summarized step by step.

First, Ginethal has shown us that Asimov *as a scientist* fails ignominiously in his attacks on Velikovsky, and I now have done the same for his non-scientific attacks. He ends with egg firmly covering all of his face. When however we look at his non-Velikovskian scientific writings, we find that he is not a fool. Even though he is mostly traditional and seldom profound, rarely is he absurdly wrong, yet here, *each* time he confronts Velikovsky, he demeans himself. This has to indicate that a powerful layer of blind passion underlies his Velikovskian excesses.

Second, when we notice that this absurd behavior parallels what we find in each of the anti-Velikovsky scientists we treat in this book, each of whom felt he represented the values of mainstream science and then acted irrationally and almost madly concerning Velikovsky, we recognize that this condition is endemic in Science. It is a common frenzy. The most probable cause for it, however, cannot be a common idiocy among these scientists, for many have achieved high professional distinction *when they were not treating catastrophism*. It therefore has to be the ideas which Velikovsky represents, and we can thereby begin to perceive a vast terror in the heart of mainstream science, totally unsuspected by science, concerning Velikovsky's ideas.

Third, therefore, is the thought that, if these normally sane people are so psychotic about catastrophism, then perhaps it is because the catastrophes *did* occur, which would make the common psychosis a frenzied attempt on the part of Science as a whole to *not have it said*. If true, then the next insight is inescapable—if such unconsciously-terrorized men had done their conscious best to suppress this knowledge and were then suddenly faced with a person who proclaimed it, they would react precisely as Shapley, Payne-Gaposchkin, Menzel *et al.* did: they would try to destroy the truth-speaker. That is the model I derive from the data provided by the seeming paradox of Asimov, whose self-opposed acts *ironically make the Velikovsky Affair probable*. If catastrophes even a bit like those described by Velikovsky did truly occur, if they deposited within all of us since then a continuing but unsuspected terror, then Asimov the scientist-cum-creative-artist, (the affirmer and denier at the same time), could not help trying to annihilate Velikovsky and fail. He like Shapley appears to be the unknowing victim of the same phobia, and their ignominious self-destructions indicate that catastrophism may be true.

We must wonder, next, why he wrote this piece so late in the day, almost 25 years after the original *Worlds in Collision* furor. One clue could be that it appeared at the same time as the notorious AAAS Symposium on Velikovsky. Perhaps Asimov thought that, with it, he would drive the final nail into Velikovsky's coffin and do it better than any of the invited Symposium speakers, of which he was not one. Poor Asimov. In the end, of course, he only succeeded in making a fool of himself, like Sagan, like Shapley, and that is what is significant here, that he bugled things so badly as a scientist while also violating the very principles of science, civility and reason which elsewhere he tried to uphold.

What then should we finally think of the pompous Asimov? In his article on Velikovsky as a CP, he concluded this way:

"Heraclitus of Ephesus was known as the 'weeping philosopher' because he wept over the follies of mankind. Democritus of Abdera . . . was known as the 'laughing philosopher' because he laughed at the follies of mankind. Ever since, one can choose either to weep or laugh at folly."¹⁶⁹⁸

One can easily guess Asimov's choice:

". . . over such meaningless nonsense as the Velikovskian theories, no one without a heart of stone and a brain of lead can do anything but laugh—and laugh—and laugh."¹⁶⁹⁹

The sentiment is crude and ugly, as always, but now it is we who must make this choice, about Asimov. Having seen him debunked, not merely by superior men like Einstein and Bohr, or even by such much more highly qualified scientists like Hess, Kallen, Motz, Pfeiffer, Schaeffer and Drioton, or yet by qualified catastrophists like Bass and Ransom, but also by fierce anti-Velikovskians like Sagan and Bauer, should we not laugh right back? Perhaps, but it must be mixed with pity, for the man does not seem able to help himself. His juvenile sneering and petty meanness seem to come from deep inside, where we may guess that a great terror lies, and that is what the Velikovsky Affair is all about ultimately, that the *excess* of the reaction, its nonsensical *extremity*, betrays that what is attacked is really what is believed. Asimov's final status, therefore, is very ironic, (opposite to what he intended), for, the more that he tries to demolish Velikovsky, the more he unintentionally furnishes evidence for the Velikovsky Affair which he wants to disprove. In that sad sense, the laugh is ultimately on him.

* * * * *

STEPHEN JAY GOULD AND IMMANUEL VELIKOVSKY, By Charles Ginenthal

"For what a man rather were true he more readily believes."

Francis Bacon
1620 Aphorism x/ix
Novum Organum

¹⁶⁹⁸Isaac Asimov, *Pebble in the Sky*, Fawcett Crest (paperback), New York: Doubleday, 1950, p. 50.

¹⁶⁹⁹*Ibid.*

"You have no idea of the intrigues that go on in this blessed world of science. Science is, I fear, no purer than any other region of human activity though it should be."

L. Huxley

Life and Letters of Thomas

Henry Huxley (London, 1900)

p. 97.

"Though he means to be honorable, he is so bigoted that he cannot act fairly."

Charles Darwin

Cited by Gertrude Himmelfarb

Darwin and the Darwinian

Revolution

(1968), p. 360.

Three years after the AAAS symposium on Velikovsky, Stephen Jay Gould, Professor of Geology and science historian at Harvard University, took aim at Velikovsky with an essay, "Velikovsky in Collision."¹⁷⁰⁰ In this critical analysis, Gould brings to bare what he conceives to be Velikovsky's faults, essentially the wrong way to pursue science. Martin Gardner has dubbed Gould's essay "the strongest criticism" of Velikovsky.¹⁷⁰¹ In the following essay I will examine this strongest of criticisms.

One way not to do science is, of course, not to contradict oneself by stating one thing in one place and just the opposite somewhere else. This is hypocrisy. For example, let us compare the following statements by Gould on theory and how acceptance of a theory creates problems related to evidence. Gould states:

"Many readers may be disturbed by my argument for the primacy of theory. Does it not lead to dogmatism and disrespect for fact? It can, of course, but it need not."¹⁷⁰²

On the opposite side Gould also states:

¹⁷⁰⁰Stephen Jay Gould (A), "Velikovsky in Collision." *Ever Since Darwin*, (New York, 1977) pp. 153-159.

¹⁷⁰¹Martin Gardner, "Welcome to the Debunking Club," *The New Age*, (Buffalo, N.Y., 1991), p. 67.

¹⁷⁰²Stephen Jay Gould (B), "The Validation of Continental Drift," *Ever Since Darwin*, (New York, 1977), p. 167.

"I would say usually, theories act as straitjackets to channel observations toward their support, and to forestall data that might refute them. Such theories cannot be rejected from within for we will not conceptualize the potentially refuting observations."¹⁷⁰³

As one may plainly see, Gould admits that theories usually blind an observer to facts which do not fit into one's dogmatism or preconceived views, and which lead to "disrespect for facts" or that contradict those views. However, Gould holds out the possibility that it "need not." But he is also aware that what leads to dogmatic analysis of facts act as "straitjackets" to the mind to channel observation and forestall data that might refute the theory by a different interpretation of those facts. In essence, Gould argues for the "primacy of theory" even though it usually acts as a mental straitjacket.

A straitjacket, I need not remind the reader, is a mechanism of control, a restraint to repress or suppress ideas or theories one deems unwelcome.

Gunnar Myrdal, the great social scientist, was well aware of the fact that, on a psychological level, scientists are human and, therefore, are as biased as anyone. He states,

"Like people in general . . . scientists are apt to conceal valuations and conflicts between valuations by stating their positions as if they were simply logical inferences from the facts."¹⁷⁰⁴

"Opposing the most honest determination on the part of all concerned and, primarily, on the part of scientists themselves to be open-minded, the common need for rationalization will tend . . . to influence the concepts, models, and theories applied; hence it will also affect the selection of relevant data, the recording of observations, the theoretical and practical inferences drawn explicitly or implicitly, and the manner of presentation of the results of research."¹⁷⁰⁵

"The result [of these processes] is systematic biases in our work."¹⁷⁰⁶

What Myrdal points out is a fundamental social and psychological fact that scientists see their valued theories as correct and draw their inferences to related concepts from them as if their inferences are facts. They rationalize from these inferences what is the relevant data, etc. and the end result is "systematic bias" in their work. Myrdal admits more directly what Gould wishes to suggest can be overcome, namely, one's scientific prejudices. This straitjacket, or biased approach, I believe, well describes the mechanisms of Gould's criticisms of Velikovsky.

Velikovsky and psychologists well know that humor is a way to deal with unbidden and misapprehended tension. It can also be a weapon to humiliate another and his views. Gould begins his criticism with the following:

¹⁷⁰³Stephen Jay Gould (C), "Dinosaurs in the Haystack," *Natural History*, (March 1992), p. 6.

¹⁷⁰⁴Gunnar Myrdal, *Objectivity in Social Research*, (New York, 1969), p. 50.

¹⁷⁰⁵*Ibid.*, p. 53.

¹⁷⁰⁶*Ibid.*, p. 44.

"Not long ago, Venus emerged from Jupiter, like Athena from the brow of Zeus—literally! It then assumed the form and orbit of a comet. In 1500 B.C., at the time of the Jewish exodus from Egypt, the earth passed twice through Venus' tail, bringing both blessings and chaos; manna from heaven (or rather from hydrocarbons of a cometary tail) and the bloody rivers of Mosaic plagues (iron from the same tail). Continuing its erratic course, Venus collided with (or nearly brushed) Mars, lost its tail, and hurtled to its present orbit. Mars then left its regular position and almost collided with the earth about 700 B.C. . . .

¹⁷⁰⁷

After mocking Velikovsky's theory by offering this glib, almost grotesque parody of it, Gould disclaims his mockery and projects it as an appropriate, responsible scientific summary of Velikovsky's work.

"This may sound like the script of a very poor, late-late movie on TV; nonetheless, it represents the serious theory of Immanuel Velikovsky's *Worlds in Collision*. And Velikovsky is neither crank nor charlatan—although to state my opinion and to quote one of my colleagues, he is at least gloriously wrong."¹⁷⁰⁸

I direct the reader to Gould casting the onus for his caricature onto Velikovsky by saying "it represents the serious theory of Immanuel Velikovsky." That is, Gould writes the disparaging parody of Velikovsky's theory and then calls it the serious work of Velikovsky.

But this tasteless, insulting caricature cannot be Velikovsky's doing. This presentation is more aptly a smear, a form of denigration to deride Velikovsky and Velikovsky's material before analyzing it. By insulting Velikovsky and his life's work in this boorish, low introduction to it displays, in reality, Gould's own psychology and his own need to "straitjacket" this material into an acceptable form to fit his own needs.

Gould, of course, is an historian of science and is also aware of the damage that caricature or parody of catastrophist theory can do. It is merely a tactic of "cardboard[ing]" or stereotyping an opponent and his ideas. And one may well ask, what is Gould's own understanding and attitude toward this type of parodied caricature of catastrophists and catastrophist theory? In his very fine book, *Times Arrow Times Cycle*, Gould goes out of his way to unmask this as a propagandistic form of disrespectful insult. For in this book, and in others, he castigates Lyell, a lawyer, and the others who followed in Lyell's footsteps, for using this technique over and over again. That is, it is "The Big Lie" couched in terms so as to wreck havoc on one's scientific opponents by repeating the false and caricatured rendition of their catastrophist theory over and over again.

"If Lyell cast his vision in cardboard, later retelling of the great dichotomy became even more simplistic. First of all, the two sides received names—catastrophism for the vanquished, uniformitarian for the victors. Names warp

¹⁷⁰⁷Gould (A), *op. cit.*, p. 153.

¹⁷⁰⁸*Ibid.*

any remaining subtlety into neat packages. *Secondly, the catastrophist position became more foolish and caricatured in the constant retelling.*"¹⁷⁰⁹ (Emphasis added)

In Gould's review of Jeremy Rifkin's book, *Algeny*, he is deeply disturbed because,

"The basic argument of *Algeny* rests upon a parody of an important theme advanced by contemporary historians of science against the myth of objectivity and inexorable scientific progress. [Gould then delineates the theme and charges] But in Rifkin's hands this theme becomes a caricature."¹⁷¹⁰

Gould further castigates Rifkin because "[h]e caricatures my own theory of punctuated equilibrium as a sudden response to ecological catastrophe."¹⁷¹¹ He also shows his ire at Rifkin because "Rifkin does not understand Darwinism and his arguments refute an absurd caricature, not the theory itself."¹⁷¹² Gould further appears offended by criticism based on Rifkin's "dishonest argument and nasty caricature."¹⁷¹³ Why then is it so wrong for Rifkin to parody and caricature a scientific theme and also Gould's theory, but it is perfectly acceptable for Gould to parody and caricature Velikovsky's hypothesis in a most tasteless manner? What Gould so cordially dislikes in Lyell's and Rifkin's approach is "caricature" or parody. But when he employs the same weapons on Velikovsky it is apparently perfectly all right and there are no double standards at work.

Just like Rifkin, Gould is retelling Velikovsky's position making it appear "more foolish and caricatured" and giving it the "name," "gloriously wrong," as opposed to Gould's "victors" or heros.

What is the cardboard or stereotype game that Lyell played? According to Gould, Lyell used Biblical fundamentalism or Bible baiting to bash his opponents. As Gould aptly remarks,

"In particular, Lyell noted that the 5000-year time scale of Genesis had faded from respectability by 1800, and that his scientific colleagues could only stand accused (at most) of not allowing sufficient millions [of years] in their revised estimates But later textbooks have usually blurred this distinction and imagined that the catastrophists of Lyell's *own day* still adhered to the Mosaic chronology Catastrophists, in short, became biblically motivated miracle mongers, actively preventing the establishment of geology as a proper science."¹⁷¹⁴

Here are a few examples of Lyell's Bible baiting catastrophists which he applied to 19th century scientists. For example, in writing of Scilla (1670) he states, "[l]ike many eminent naturalists of his day, Scilla

¹⁷⁰⁹Stephen Jay Gould (D), *Times Arrow Times Cycle*, (Cambridge, Mass., 1987), p. 112.

¹⁷¹⁰Stephen Jay Gould (H) "Integrity and Mr. Rifkin," *An Urchin in the Storm*, (New York, 1987), p. 230.

¹⁷¹¹*Ibid.*, p. 233.

¹⁷¹²*Ibid.*, p. 232.

¹⁷¹³*Ibid.*, p. 237.

¹⁷¹⁴Gould (D), p. 112.

seems to give way to the popular persuasion that all fossil shells were effects and proof of the mosaic deluge."¹⁷¹⁵ "In short, a sketch of the progress of geology is the history of a constant and violent . . . struggle of new opinions against doctrines sanctioned by the implicit faith of many generations, and supposed to rest on scriptural authority."¹⁷¹⁶

However, Gould denies this entirely for 19th century catastrophists.

"Contrary to popular belief, no serious nineteenth century scientist— not even the most theological catastrophist— argued for the direct intervention of God in the earth's affairs. All accepted the constancy of natural law, God . . . did not need to meddle by miracle with the subsequent history of the earth."¹⁷¹⁷

Gould describes this approach elsewhere, with the following behavior on Lyell's part, to call his scientific adversaries Biblical fundamentalists.

"Lyell relied upon two bits of cunning to establish his uniformitarian view as the only geology.

"First, he set up a strawman to demolish. By 1830, no serious scientific catastrophist believed that cataclysms had a supernatural cause, or that the Earth was 6000 years old."¹⁷¹⁸

Gould decries this tactic since, he understands, "it seems unjust that catastrophists, who almost followed a *caricature of objectivity and fidelity to nature*, should be saddled with a charge that they abandoned the real world for their *Bibles*."¹⁷¹⁹ (Emphasis added) Not only does Gould decry Lyell's caricature of catastrophists who followed a caricature of objectivity and fidelity to nature, but he roundly castigates such behavior. ". . . Georges Cuvier is Lyell's catastrophist enemy, [*h*]e accepts the biblical chronology (or at least an earth of very short duration) . . . he works probably consciously for the church against science. WHAT A VULGAR MISREPRESENTATION!"¹⁷²⁰ (Emphasis and capitals added)

The question, quite naturally, arises: Does Gould play the same Bible baiting— Biblical fundamentalist, bashing game he so deeply deplores as used by Lyell, when he comes to criticize Velikovsky? Unfortunately he resorts to the same tactics, the same cunning game, the same strawman to establish his view of Velikovsky's geology. Gould states Velikovsky "begins with the working hypothesis

¹⁷¹⁵Charles Lyell, *Principles of Geology*, Vol. I, (1867), p. 37.

¹⁷¹⁶*Ibid.*

¹⁷¹⁷Stephen Jay Gould, "Eternal Metaphors of Paleontology," *Patterns of Evolution*, ed. A. Hallam, (Amsterdam, 1977), p. 7.

¹⁷¹⁸Gould (A), *op. cit.*, p. 149.

¹⁷¹⁹Stephen Jay Gould (E), "The Stinkstones of Oenigen," *Hen's Teeth and Horse's Toes*, (New York, 1983), p. 105.

¹⁷²⁰Gould (D), *op. cit.*, p. 113.

that all stories reported as direct observation in the ancient chronicles are strictly true—if the Bible reports that the sun stood still then it did¹⁷²¹

But Velikovsky made no such claim regarding *all* ancient stories. Gould has presented not a single citation regarding Velikovsky's method written by Velikovsky to smear the man with the same smear Lyell and others used over a hundred years ago. While it was a "vulgar misrepresentation" for Lyell and others to saddle Cuvier and the other catastrophists with Bible fundamentalism in the past, it is not wrong for Gould to Bible bait and smear Velikovsky with this same "vulgar misrepresentation" today that "all stories reported as direct observations in the ancient chronicles are strictly true." My, how times have changed, and, might I add, how standards of ethical criticism do also to fit Gould's straightjacketed criticism. Robert Anton Wilson, who is not a Velikovskian, discusses this aspect of Velikovsky's position with respect to myths as follows:

"Dr. Velikovsky examined the myths of the ancients and speculated that they might contain a few facts—*sombunall* [some but not all] in our terms. Facts exaggerated, facts distorted in re-telling, facts embellished by poets, but facts that could still be deduced by comparing various myth systems and noting what they have in common. For instance, there are over 120 flood legends in addition to the one in the Old Testament. They come from every part of the world—Asia, Africa, Australia, Russia, Scandinavia, Ireland, North America, South America, Polynesia. Throw out the local details and you have one constant: the idea that there once was a flood. So maybe there was. And maybe a comet created it

"The terror might be this: *if one Bible story is even partially confirmed, the whole damned religious business might come back and be at our throats again.*

"I [Wilson] redundantly remind you again that this book is not, *per se*, advocating any particular heresies, but only examining *why* certain ideas are *taboo* and *verboten*, and why otherwise rational people conspire to suppress them

"Think of the New Inquisition's [scientific establishment's] fury against any scientist, however formerly reputable, who dares to assert statistical evidence of that damnable 'ESP' I think the anxiety is—if we allow "ESP" into the category of the thinkable or possible, who knows what other 'spooky' stuff might ride on its tail?

"Similarly, with Velikovsky: let in Noah's flood, and the next thing you know the Holy Ghost and the virgin birth might be back

". . . Newton . . . thought his model reconciled science and the Bible. Many others have had similar notions. The value of a model depends on its scientific utility, not on whether its spokespersons *think* it supports or contradicts the Bible

". . . Many readers of Velikovsky, whom I have met, are not aware that he said, or is thought to have said, that his views support the Bible. These readers, who are not notably stupid, have rather an opposite impression. They

¹⁷²¹Gould (A), *op. cit.*, p. 154.

think Velikovsky 'supports' not the Bible, but the general idea that *some* myths of all peoples are based on historical events. In other words, this group of readers did not get the message 'The Bible is true' but rather a different message 'Some myths contain some truth.' This is hardly unthinkable; since the excavation of Troy, Homer is now recognized as containing *some* truth—sombunall [some but not all]

" . . . To proceed from '[s]omething like Noah's flood once happened' to 'the whole Bible is true' is not very logical, and I can't find anything, like it in any book of Velikovsky's that I've read; and it would be just as logical, and just as illogical to proceed from "something like the Polynesian flood story once happened" to '[t]he whole Polynesian mythology is true,' and Velikovsky does not say that either as far as I have read him."¹⁷²²

However, this is precisely the argument Gould makes that all myths are true. Paul Feyerabend states the following about ancient man's science in the time when myth was history. The believers of myths, he argues,

" . . . invented fire, and the means of keeping it. They domesticated animals, bred new types of plants . . . invented rotation of crops . . . crossed the oceans in vessels . . . and demonstrated a knowledge of navigation Thus, if science is praised for its achievements, then myth must be praised a hundred times more fervently because *its* achievements were incomparably greater. The inventors of myth started *culture* while rationalists and scientists just *changed* it."¹⁷²³

Even Gould is well aware of this historical nature of analysis from ancient myths which was used by his hero, Cuvier. Gould states:

"Cuvier's methodology may have been naive, but one can only admire his trust in nature and his zeal for building a world by direct and patient observation, rather than by fiat, or unconstrained feats of patient imagination. His rejection of received doctrine as a source of necessary truth is, perhaps, most apparent in the section of the *Discours Preliminnaire* that might seem, superficially, to tout the *Bible* as infallible—his defense of Noah's flood. He does argue for a worldwide flood some five thousand years ago, and he does cite the Bible as support. But his thirty page discussion is a literary and ethnographic compendium of all traditions from Chaldean to Chinese. And we soon realize that Cuvier has subtly reversed the usual apologetic tradition. He does not invoke geology and non-Christian thought as window dressing for 'how do I know, the Bible tells me so.' Rather he uses the Bible as a single source among many of equal

¹⁷²²Robert Anton Wilson, *The New Inquisition*, (Scottsdale, Ariz., 1987), pp. 70-72.

¹⁷²³Paul Feyerabend, *Science in a Free Society*, (London, 1978), pp. 104-105.

merit as he searches for clues to unravel Earth's history. Noah's tale is but one local and highly imperfect rendering of the last major paroxysm."¹⁷²⁴

The only naiveté that Gould exhibits is his assumption that the ancient myths have nothing to tell us about the astronomical history of the ancient world. As I pointed out in my discussion of Morrison and Chapman, there are scientists who have begun to examine these ancient sources to find their underlying meaning, while Gould, based on nothing, implies this material has nothing to tell. Ralph Abraham, of the University of California, describes this know-nothing attitude displayed by Gould thus:

"Velikovsky saw what other scholars were unable to see, because he relied on evidence that they had chosen to neglect: the accumulated records of human experience. Natural scientists, who scorned these records put themselves in the position of the early astronomers who held that no truly respectable scholar should resort to the telescope. They denied a creative new idea on the grounds of dogma [their view of science] which they took to be truth."¹⁷²⁵

Gould will still not recognize this new evidence, but knows that his hero, Cuvier, employed it. While Velikovsky is pummeled for employing myths and legends, Cuvier is not. Why not? Because Gould has an ax to grind. He has his mind made up regarding Velikovsky and his approach to evidence. Again, we run into the double standard critics of Velikovsky employ.

In essence, Gould recognized every aspect of Velikovsky's approach to myth in the work of 19th century geologists. Every propagandist tactic employed by uniformitarians of that era starting with Lyell—who unleashed the misinformation that catastrophists were Bible fundamentalists, as were those who spread this propaganda thereafter, he knew were spreading a lie. All this Gould well knows and well understands, but he cannot restrain himself or his criticism from the straitjacket approach of spreading the very same kind of Bible baiting misinformation and propaganda with which he smears Velikovsky.

Gould, as I see it, is simply playing the game of guilt by association. Tom Van Flandern describes this tactic in his chapter, "The Unscientific Method."

"Another popular principle of the Unscientific Method is 'discrediting by association.' *e.g.*, 'My opponent associates with organized crime figures, [or supports Biblical fundamentalists] so don't listen to what he says.' It is most effective when your audience [scientists, academics and the public] is predisposed to make the association anyway

"Discrediting by association is often promulgated effectively through the use of ridicule. Its purpose is to dissuade potential allies of an idea when a meaningful scientific response is not readily available. The mark of the insecure layman posing as a scientist is the regular use of ridicule, particularly when it replaces

¹⁷²⁴Gould (E), *op. cit.*, pp. 105-106.

¹⁷²⁵Ralph Abraham, *Chaos, Gaia Eros*, (San Francisco, 1994), p. 194.

substantive responses and critical thinking. *Even a truly crankish idea deserves better than ridicule from one who calls himself a scientist.*¹⁷²⁶ (Emphasis added)

Gould's critical approach to Velikovsky contains essentially all these elements, and as such, justifies John Anthony West's comment. "Trying to find an open-minded scientist is like trying to find a Fundamentalist Christian who loves his enemies."¹⁷²⁷

Not only does Gould use a contradictory standard of criteria with respect to his use of Lyell's stereotyping, but he then turns to exonerate Velikovsky's earlier detractors some of whom were from his school, Harvard University. Gould admits "Velikovsky was surely ill treated by certain academics who sought to suppress the publication of his work."¹⁷²⁸ Somehow it is difficult for Gould, an historian of science, to admit Velikovsky's work was repeatedly misrepresented by these same individuals. After comparing Velikovsky to Bruno and Galileo, he claims a prosecutor such as "Torquemada was evil; Velikovsky's academic enemies, merely foolish."¹⁷²⁹

In this instance, one would think that the scientific community was "merely foolish," not "evil," when it blackmailed and boycotted MacMillan and Company by forcing it to suspend publication of *Worlds in Collision* and then to incinerate the remaining copies of the book still in its possession. Forcing the burning of books with which the scientific community does not concur, *à la* Hitler, or of the book burning mentality of the Inquisition, apparently is not "evil," just "merely foolish." Of course, Gould is quick to remind his readers, "Velikovsky won both publicity and royalties."¹⁷³⁰ What he does not remind his readers is that James Putnam, who handled *Worlds in Collision* for MacMillan, did not receive publicity and royalties and was forced from his job as editor by the scientific community for his association as the book's editor. This, too, one may consider "merely foolish," and not "evil." What Gould has failed to mention, as well, with respect to these foolish scientists and academics, is that Gordon Atwater, the Curator of the Hayden Planetarium and Chairman of the Department of Astronomy of the American Museum of Natural History, did not receive royalties or fame, and was also removed by the establishment from his job for the unpardonable scientific sin and heresy of writing a favorable review of *Worlds in Collision*, and for planning and announcing a planetarium program to depict the events of Velikovsky's book. And one of these "foolish" individuals "even walked into my [Atwater's] office and spit in my [Atwater's] face."¹⁷³¹ Gould, most certainly does not tell his readers this or how these "merely foolish" scientists and academics then blacklisted Atwater for the rest of his life from ever finding employment in the field of science. Having a scientist and academic like Gould call "fascist" and "totalitarian" behavior "merely foolish" is clearly revisionism of some sort. But Albert Einstein understood that Gould's description of the suppression of Velikovsky's book was not "foolish," but was decidedly "evil." As reported by Bernard I. Cohen in *Scientific American* in July 1955, "Einstein was sorry that scientists in the U.S. had protested to publishers about the publication of such a book. He thought that bringing pressure to bear on a publisher to suppress a book was an EVIL THING TO DO."¹⁷³² (Capitalization added) While Eric Larabee, in a letter to *Scientific American* (May 1956), called the suppression *via* "the threat of boycott is . . . a disgrace to American science, and will remain so long after the substance of the dispute has . . . dissolved . . . ,"¹⁷³³ such actions are not "merely foolish" but are "vulgar" and "vile." Speak of caricature!

¹⁷²⁶Tom Van Flandern, *Dark Matter, Missing Planets and New Comets*, (Berkeley, Calif., 1993), pp. 357-358.

¹⁷²⁷*Ibid.*

¹⁷²⁸Gould (A), *op. cit.*, pp. 153-154.

¹⁷²⁹*Ibid.*, p. 154.

¹⁷³⁰*Ibid.*

¹⁷³¹Clark Whelton, "The Gordon Atwater Affair," *S.I.S. Review*, Vol. 4, No. 4, (Spring 1980), pp. 75-76.

¹⁷³²I. Velikovsky, *Stargazers & Gravediggers*, (New York, 1983), p. 298.

¹⁷³³*Ibid.*, P. 315.

The question that always intrigues me with regard to Velikovsky's critics is: Did they knowingly misrepresent Velikovsky's evidence? And so, we come to the "pathetic" and "foolish" behavior of a Harvard professor and science historian. Here I quote Gould attacking Velikovsky for employing in his book, *Earth in Upheaval*,

" . . . the exclusive use of outdated sources: before 1850 most geologists invoked general catastrophes as the major agent of geologic change. These men were not stupid and they argued their position with some cogency."¹⁷³⁴

On the other hand, Gould cautions elsewhere:

"Scholars often make the false assumption that contemporary cases must provide optimal data, while the records of scientific work steadily decrease in breath and reliability as they grow older and older. We might, therefore, suppose that to understand science, a historian or sociologist should study debates and discoveries now in the making. Yet a moment's thought about our technological age should expose the fallacy in such an idea."¹⁷³⁵

It is true that much of Velikovsky's data is supplied by older sources, but because a source is old does not make it invalid or wrong. For example, as evidence of an immense oceanic tidal flood, Velikovsky discusses bones of whales found north of Lake Ontario on page 47 of *Earth in Upheaval*, by citing J. D. Dana's *Manual of Geology*, 4th edition of 1894, page 983. Attacking Velikovsky for using "outdated sources" does not change the fact that Dana did properly present an indisputable fact that whale bones north of Lake Ontario were actually found, and a reasonable, rational and appropriate deduction to be made for such a finding is a recent oceanic tidal flood which carried the whale there. Bones erode fairly rapidly and the ice cap covered this region perhaps 10,000 years ago. It is, I suggest, Gould's aim to dismiss an entire book's worthiness based on such a tactic.

However, the real question is: Did Velikovsky, as Gould charges, use "exclusively outdated sources" for his citations and evidence? Is Gould also guilty of spreading misinformation and propaganda? Let us remember that *Earth in Upheaval* was published in 1955 and Velikovsky did his research for the book from about 1950 to 1955. Here, then, is a list of the citations in *Earth in Upheaval* dated close to this time:

Page 1,	F. Rainey, <i>American Antiquity</i> , V	(1940)
Page 2,	F. C. Hibben, <i>American Antiquity</i> , VIII	(1943)
Page 3,	<i>Ibid.</i> , for pages one and two	
Page 41,	C. O. Dunbar, <i>Historical Geology</i>	(1949)
Page 43,	V. Stefansson, <i>Greenland</i>	(1942)
Page 43,	R. F. Griggs, <i>Science</i> , XCV	(1942)

¹⁷³⁴Gould (A), *op. cit.*, p. 157.

¹⁷³⁵Stephen Jay Gould (H), "The Power of Narrative," *An Urchin in the Storm*, (New York, 1987), p. 75.

Page 45,	D. H. Campbell, <i>Science</i>	(January 16, 1942)
Page 45,	C. E. P. Brooks, <i>Climate Through The Ages</i>	(1949)
Page 48,	R. F. Flint, <i>Glacial Geology and the Pleistocene Epoch</i>	(1947)
Page 56,	<i>Ibid.</i> , for R. F. Flint, <i>Glacial Geology</i>	(1947)
Page 61,	R. Moore, <i>Man, Time and Fossils</i>	(1953)
Page 61,	J. S. Lee, <i>The Geology of China</i>	(1939)
Page 63,	H. Petterson, <i>Tell Us (Quarterly Review of Geophy)</i>	(1949)
Page 71,	F. K. Mather, <i>Biography of the Earth, in Science</i>	(January 16, 1942)
Page 75,	A. Heim, A. Gausser, <i>The Throne of The Gods</i>	(1939)
Page 76,	H. de Terra, T. T. Paterson, <i>Studies of the Ice Age in India and Associated Human Cultures</i>	(1939)
Page 79,	D. N. Waldia, <i>Geology of India</i>	(1939)
Page 82,	A. Posnansky, <i>Tiahuanaca</i>	(1945)
Page 83,	H. P. Moon, <i>The Transactions of the Linnean Society</i>	(1939)
Page 84,	F. C. Hibben, <i>Treasure in the Dust</i>	(1951)
Page 85,	Don Ternel, <i>Travel</i>	(April, 1945)
Page 88,	W. J. Miller, <i>An Introduction to Historical Geology</i>	(1946)
Page 94,	P. LeCler, <i>Sahara</i>	(1954)
Page 98,	Douglas Johnson, <i>The Origin of the Carolina Bays</i>	(1942)
Page 98,	W. F. Prouty, <i>Bull. of the Geol. Soc. of Amer.</i>	(1952)
Page 100,	M. Ewing, <i>National Geographic Magazine</i>	(November, 1949)
Page 104,	Petterson, <i>Scientific American</i>	(August, 1950)
Page 105,	Petterson, <i>Tell Us</i>	(1949)
Page 105,	Petterson, <i>Westward Ho With the Albatross</i>	(1953)
Page 109,	Barbara Bell, <i>Science News Letter</i>	(May 24, 1952)
Page 123,	Brooks, <i>Climate Through the Ages</i>	(1949)
Page 125,	F. A. Vening Meinesz, <i>Nederlandsche Akademie</i>	(1943)
Page 126,	K. A. Pauly, <i>Scientific Monthly</i>	(1952)
Page 143,	A. McNish, <i>Terrestrial Magnetism</i>	(1939)
Page 143,	H. Manley, <i>Science News</i>	(July, 1949)
Page 145,	S. K. Runcorn, <i>Scientific American</i>	(1955)
Page 150,	O. Struve, <i>Scientific American</i>	(1952)
Page 153,	Petterson, <i>Tells Us</i>	(1949)
Page 156,	C. Schuchardt, <i>Vorgeschichte</i>	(1943)
Page 157,	F. Johnson, <i>Radiocarbon Dating</i>	(1952)

Page 158,	<i>Science</i>	(September 24, 1954)
Page 158,	<i>Science</i>	(April 8, 1955)
Page 162,	C. W. Wolfe, <i>This Earth</i>	(1949)
Page 169,	I. Rouse, <i>Transactions of the New York</i>	(1950)
Page 172,	Brooks, <i>Climate Through the Ages</i>	(1949)
Page 173,	Brooks, <i>Climate Through the Ages</i>	(1949)
Page 177,	O. Paret, <i>Dan Nuer Bild</i>	(1948)
Page 181,	L. Don Leet, <i>Causes of Catastrophes</i>	(1948)
Page 182,	P. H. Kuenen, <i>Marine Geology</i>	(1950)
Page 184,	E. Janssens, <i>Historie</i>	(1946)
Page 185,	H. Goodwin, <i>Transactions of the Royal Society</i>	(1940)
Page 189,	S. Martinatos, <i>Antiquity</i>	(1939)
Page 193,	Claude F.A. Schaeffer, <i>Stratigraphie</i>	(1948)
Page 197,	J. Gastang, G.B.E. Gastang, <i>The Story of Jericho</i>	(1940)
Page 198,	Schaeffer, C.F.A. <i>Stratigraphie</i>	(1948)
Page 199,	R. E. Mortimer Wheller, <i>Archeology</i>	(1944)
Page 199,	<i>Jour. of the Royal Society</i>	(December, 1950)
Page 200,	Schaeffer, C.F.A. <i>Stratigraphie</i>	(1948)
Page 205,	A. L. Kroeber, <i>The Maya</i>	(1940)
Page 205,	L. C. Easley, <i>American Antiquity</i>	(1943)
Page 206,	L. H. Johnson, <i>Scientific Monthly</i>	(1952)
Page 207,	L. C. Easley, <i>American Antiquity</i>	(1943)
Page 212,	Dunbar, <i>Historical Geology</i>	(1949)
Page 215,	Brooks, <i>Climate Through the Ages</i>	(1949)
Page 219,	H. Nilsson, <i>Synthetische Art Bildung</i>	(1953)
Page 219,	Weigelt, <i>et al, Nova Acta Leopoldina</i>	(1941)
Page 227,	L. H. Johnson, <i>Scientific Monthly</i>	(1952)
Page 228,	Easley, <i>American Anthropologist</i>	(1946)
Page 230,	Easley, <i>American Antiquity</i>	(1943)
Page 230,	Hibben, <i>Treasure in the Dust</i>	(1951)
Page 231,	Easley, <i>American Anthropologist</i>	(1946)
Page 231,	Easley, <i>American Antiquity</i>	(1943)
Page 235,	C. C. Gillispie, <i>Genesis and Geology</i>	(1951)
Page 253,	Muller, <i>Genetics Medicine and Man</i>	(1947)
Page 254,	"Botany," <i>Britannica Book of the Year</i>	(1944)

If all this wasn't enough to prove that Velikovsky did not use *exclusively* "out of date sources," Gould carefully omitted the fact that Velikovsky added a "Forum Address" to the book presented at Princeton University on *recent evidence* for his theory, some of which was dated as late as April 5, 1955 (page 297), April 15, 1951 (page 296), and so on. I have presented these citations to show that all Gould had to do to know of these source dates was simply thumb through the book to find them scattered from the beginning to the end.

Now, how does Gould feel about this form of misrepresentation as criticism? One of his critics, Jeremy Rifkin, is taken to task by Gould because "Rifkin does not respect the procedures of fair argument. He uses every debater's trick in the book to mischaracterize and trivialize his opposition," ¹⁷³⁶ How is stating that Velikovsky exclusively used outdated sources, which is a clear misrepresentation on the part of Gould, qualify as respect for the procedures of fair argument? It simply does not! And how does Gould's use of the debater's trick to caricature Velikovsky's theory qualify as good characterization of Velikovsky's work? It simply does not! Since when have the debater's manipulations by Gould become justified?!

As I pointed out earlier, Gould is an historian of science, and to misrepresent the work of a man at its source is worse than "foolish," to say the very least. There is the undocumented story of Thomas Jefferson who is reputed to have said, "[i]t is easier to believe that . . . Yankee professors would lie than that stones fall from heaven." ¹⁷³⁷

As it turns out, stones do fall from the sky and the *Continuing Velikovsky Affair* proves Yankee professors do, indeed, lie! And I remind Gould, as a science historian, to heed his own words of warning,

"I WOULD ARGUE THAT WE MISREPRESENT HISTORY AT OUR PERIL AS PRACTICING SCIENTIFIC RESEARCHERS." ¹⁷³⁸ (Capitalization added)

But Gould need never worry or fret about this lie because he can rest assured that his colleagues in science, academia and the scientific press will not condemn his behavior. They are, after all, professionals and honorable and, like Gould, will understand that Gould was merely being "foolish" and not "evil" in misrepresenting Velikovsky on the matter of his sources being outdated. They are all honorable men and Gould is one of them.

Before leaving this matter about the expertise of old scientific evidence, let us briefly touch on the case of sunspots to enlighten us on old sources.

"It seems that late in the 19th century, the German solar astronomer Gustav Friedrich Wilhelm Sporer collected records of old sunspot sightings. He was startled to discover that for about 70 years, beginning around 1645, sunspot sightings were great rarities. Apparently the Sun was, for this strange interlude, virtually spotless

"A Victorian astronomer, E. Walter Maunder, who studied sunspots from the Royal Greenwich Observatory in England, double-checked the historical record and saw that Sporer was right In the 1600's they had been reluctant

¹⁷³⁶Gould (H), *op. cit.*, pp. 234-235.

¹⁷³⁷John G. Burke, *Cosmic Debris* (Berkeley, Calif., 1986), p. 57.

¹⁷³⁸Gould (D), *op. cit.*, p. 114.

to accept a blemished Sun; when they finally had to, they decided that the Sun was at least constant. In the 1800's they [scientists] hated [Heinrich] Schwabe's announcement of a variable Sun. When they swallowed that at last, they decided that the Sun was at least consistent – that is, varying in a regular and reliable cycle. After that, says [John] Eddy, 'no one seemed to question again the universality of the 11 year sunspot cycle or whether it had always been a dominant feature of the Sun.' The way one tries to bury a bad dream, astronomers forgot about the strange interlude when the Sun shed its spots. No one listened to Sporer or to Maunder, either

"So Eddy triple-checked Sporer and Maunder—and soon decided they were right to trust the quaint records of the early astronomers. 'Rare old books published more than three centuries ago give us a direct record of what the Sun was like in the past, and one that we can believe,' says Eddy. 'One of them was written by Christopher Scheiner, a Jesuit priest, and published in the year 1630. He was a contemporary of Galileo, and one of those who was awed by the new discovery that the Sun was covered with spots His book is entirely about sunspots, and it's 784 pages long, written entirely in Latin Scheiner's drawings of the face of the Sun are as good as any that are done today. It was his book more than any that convinced me that we could trust the astronomers of the early and middle 17th century when they showed us that the Sun, at the time, was undergoing a major period of change."¹⁷³⁹

In addition, Christoph Clavius, also a Jesuit who lived during the 16th century, made an observation of a solar eclipse which took place on April 9, 1567. His data has been employed by scientists to analyze the concept that the size of the sun may be shrinking."¹⁷⁴⁰ If one were to dismiss data because it is old, then astronomers would never be able to retrocalculate the motions and behavior of celestial bodies. This approach is simply throwing out the baby with the bath water.

What I wish to stress is that it is not the age of scientific evidence nor the source –Clavius and Scheiner's old Latin texts—nor the work of 19th century geologists Velikovsky often cited that is in question. The real question is one of evidence, not the age of the evidence, nor the source. Scheiner and Clavius were Jesuit priests and believers in the Bible, fundamentalists; but their evidence was correct and helped make a scientific breakthrough.

The game Gould is playing is evasion. By calling this evidence and its sources out of date allows him not to deal with it; in the same way, putting Velikovsky in with believers of the Bible allows him to evade these mythic materials so that he does not have to provide counter evidence. What Gould, like all the others, provides in this case, is unsubstantiated statements. He asks the reader to accept his word as the real truth for his position, but presents no evidence to substantiate his statement about whether or not the older sources are correct or not. This is sheer authoritarianism.

In order to show that Velikovsky's physical evidence, presented in *Earth in Upheaval*, is clearly wanting, and that the catastrophic evidence Velikovsky presents is invalid, Gould argues,

¹⁷³⁹Johnathan Weiner, *Planet Earth*, (Toronto, 1986), pp. 247-248.

¹⁷⁴⁰Analyses of historical data suggest the Sun is shrinking," *Physics Today*, (September 1979), pp. 17-19.

"If the testimony of [ancient] human narrators provided the evidence for *Worlds in Collision*, then the geologic record itself must suffice for *Earth in Upheaval*. Velikovsky's entire argument hinges on his reading of geological literature. This, I feel, he does rather badly and carelessly. I will focus upon the general faults of his procedure, not the refutation of specific claims. [Emphasis added]

"First the assumption that similarity of form reflects simultaneity of occurrence: Velikovsky discusses the fossil fishes of the Old Red Sandstone, a Devonian formation in England (350–400 million years old). He cites evidence of violent death—contortion of the body, lack of predation, even signs of 'surprise and terror' engraved forever on fossil faces. He infers that some sudden catastrophe must have extirpated all these fishes; yet, however unpleasant the death of any individual, these fishes are distributed through hundreds of feet of sediment that record several million years of deposition!"¹⁷⁴¹

Here Gould makes the claim that over several millions of years, very gradually, millimeter by millimeter, sedimentation on the water bed covered and buried each of these untold numbers of fish. The rate of deposition in the water was extremely slow.

". . . Piggot and Urry found that the rate of [sediment] accumulation alongside the mid-Atlantic ridge was eleven centimeters in a thousand years. In the center of the western basin of the North Atlantic, the rate for an impure . . . ooze is four centimeters in a thousand years. A core of . . . ooze from the Caribbean gave a slower rate, namely 0.6 centimeters in a thousand years and a core of "red clay" from the Pacific, five hundred kilometers [350 miles] from the coast of California yield a rate of 0.5 centimeters in a thousand years."¹⁷⁴²

It would take literally hundreds or thousands of years to cover each of these fish at these rates of burial even in a fresh water environment.

When fish die they fill with gas and float to the surface where other organisms prey upon their dead, bloated carcasses.

What Gould has not explained is how these fish, each over several hundred to several thousand years, could remain on the water bed, mostly uncovered by sediment, without decaying and rotting away as they were ever so slowly buried. What Gould has also not explained is why bottom dwelling, scavenging fish, bacteria and other organisms living just above the water bed, and other organisms living in the top layers of the water bed oozes, failed to eat these fish over this same period of time.

In essence, Gould's view requires that these fish were, in some way, impervious to rotting in water over thousands of years. Apparently, these were special fish with built-in rot resistance of their cells or were

¹⁷⁴¹Gould (A), *op. cit.*, p. 155-156.

¹⁷⁴²David B. Ericson, Goesta Wollin, *The Deep and the Past*, (New York, 1964), p. 131.

unappetizing to all bottom and top dwelling scavengers. And this unique, uniformitarian, non-rotting, non-scavenging was going on for several millions of years so that these fish, with their soft tissues, would be preserved in Old Red Sandstone in untold numbers. Velikovsky, citing Hugh Miller's, *The Old Red Sandstone* (Boston, 1865), page 222, states,

" . . . some terrible catastrophe [was] involved in [the] sudden destruction [of] the fish of an area at least a hundred miles from boundary to boundary, perhaps much more. The same platform in Orkney as at Cromarty is strewn thick with remains which exhibit unequivocally the marks of sudden death. The figures are contorted, curved; the tail in many instances is bent around the head; the spines stick out; the fins are spread to the full, as in fish that die in convulsions. The Pterichthys [(an extinct fishlike animal with winglike projections and with the anterior of the body encased in bony plates)] shows its arms extended at their stiffest angle, as if prepared for an enemy. The attitude of all ichthyolites [any fossil fish] on this platform are attitudes of fear, anger and pain. The remains, too, appear to have suffered nothing from the after-attacks of predaceous fishes; none such seem to have survived. The record is one of destruction at once widely spread and total."¹⁷⁴³

Now anyone knows that fossil remains are generally made up of the bones, shells, teeth and other hard parts of the organism. These take longer to decay and thus survive the early scavenging of bacteria. But in the Old Red Sandstone, this is hardly the case. The fish fossils are primarily preserved showing that their soft tissues were barely decomposed or eaten away. We have probably all seen fish die in home aquariums. They float to the surface bloated with gas and, if not removed, their soft tissues begin to decay. Why this did not occur in the fresh waters where the Old Red Sandstone formed Gould does not explain.

Gould has told us the 19th century catastrophists "were not stupid," that they followed "a caricature of objectivity," but when it comes to the Old Red Sandstone they, like Velikovsky, analyzed it "rather badly and carelessly." What do modern researchers say about fossilization in water? Here, I suggest, it is Gould's analysis which has been done "rather badly and carelessly." This is what Giovanni Pinna, the Deputy Director of the Museum of Natural History in Milan, who, like Gould, accepts gradualism, nevertheless states:

"In fact, when an organism dies, the substances that compose its soft parts undergo more or less *rapid decay*, due to such factors as attack by bacteria and erosion by water . . . PARTICULARLY THE SEA If an organism is to be preserved it must be protected from destructive agents *as quickly as possible* And the sooner that this consolidation [burial] occurs, the more likely it is that the organism will be preserved."¹⁷⁴⁴ (Capitalization added)

J. D. Dana wrote long ago,

¹⁷⁴³Immanuel Velikovsky, *Earth in Upheaval*, *op. cit.*, p. 20.

¹⁷⁴⁴R. L. Wysong, "Geologic Evidence," *The Creation-Evolution Controversy*, (East Lansing, Mich., 1976), p. 555.

"Vertebrate animals as fishes, reptiles, etc., which fall to pieces when the animal portion is removed, *require speedy burial* after death, to escape destruction from this source (decomposition and chemical solution from air, rain water, etc.) as well as from animals that would prey upon them."¹⁷⁴⁵ (Emphasis added)

Pat Shipman, in her introductory text on fossils explains: "What happens to a bone in water . . . is also influenced by its size, especially at the extremes of the size range. Very small bones are unlikely to remain in a sedimentary environment long enough to be buried and preserved . . ."¹⁷⁴⁶ But the problem is that the soft tissues were preserved. About this Shipman also remarks, "soft tissues are . . . rarely preserved."¹⁷⁴⁷ But over a vast area, untold numbers of fish with their soft tissues intact were preserved in the Old Red Sandstone, which clearly suggests catastrophe.

In spite of the fact that many fish of the Devonian period were placoderms, that is, fish which were *partially covered* by a thick, plate-like armor, plays little role in all this. Though the head and part of the trunk were armored, the end of the trunk and tail were not so armored and yet the tails were as well preserved as the rest of the fish in the Old Red Sandstone fossils, which suggests sudden burial. Nor did the fish have internal bony skeletons, but cartilage skeletons. Once dead, all the destructive processes described above would come into play. Fish eat other fish, dead placoderms were prey to other organisms and chemical forms of destruction in water.

Now, although the Old Red Sandstone is believed to have been deposited in fresh water, this changes nothing. What the authors above explain is that fresh water, as well as sea water, would quickly disintegrate animal flesh and dying fish.

Neither Giovanni Pinna, nor J. D. Dana, who are not creationists (although their statements were presented by creationists), nor Pat Shipman suggests, as Gould would have us believe, that the burial of untold thousands upon thousands of fish was a gradual process. If that were the case, they could not become fossilized; they would be eaten by scavengers of all kinds. If we turn Gould's argument around and assume, for a moment, that these burials all through the Old Red Sandstone occur slowly, then this same gradual uniformitarian process would also have to occur *in all similar sediments laid down by water*. But this is hardly the case. Yes, in several places around the world there are other sedimentary beds and they do contain similar fish fossils in great numbers as in the Old Red Sandstone. But if the death and burial of these fish was as basic a process as Gould suggests, then all sedimentary rocks formed in fish filled waters would also be filled with fish in the very same manner as the Old Red Sandstone. But this is not the case in any manner at all.

For example, Willy Ley describes how fossil fish were probably deposited rapidly in sediments in Germany by a catastrophe during the age of the dinosaurs.

"Every once in a while, possibly as the result of a submarine earthquake, the sea washed across the coral reef, and the wave carried sea life with it, . . . a swarm of small leptolepis fishes The leptolepis fish usually have the

¹⁷⁴⁵George McCready Price, "Graveyard," *Evolutionary Geology and the New Catastrophism*, (Mountain View, Calif., 1926), p. 234.

¹⁷⁴⁶Pat Shipman, *Life History of a Fossil*, (Cambridge, Mass., 1981) p. 23.

¹⁷⁴⁷*Ibid.*, p. 193.

positions of fish which, frantically trying to reach water again, suffocated in the fine dust that was blown from the dunes.

"The condition of the various fossils fits into this explanation. The sea forms are always intact; they may be in positions resulting from the death struggle of suffocation, but no parts are missing."¹⁷⁴⁸

What Ley is suggesting is that for these fish to be completely preserved like those of the Old Red Sandstone, they had to be buried rapidly during a catastrophe. This catastrophic interpretation Gould cannot accept for the fish in the Old Red Sandstone because it demands a major catastrophe compared to that presented by Ley.

Although Gould presents the Devonian formation in which these fossil fish of the Old Red Sandstone are found as representative of perhaps 50 million years of gradual sedimentation, Digby McLaren has concluded that late in the Devonian, at the end of the Frasnian stage, the era ended in a major extinction by a large global catastrophe.¹⁷⁴⁹ In his presidential address,¹⁷⁵⁰ he invoked the impact of a large meteorite for the mass extinction which could have destroyed the fish in a catastrophe and at the same time formed strata such as the Old Red Sandstone. He claims "[f]resh water or turbid [sandy or cloudy water] are both fatal to the types of organisms that disappeared at the close of the Frasnian."¹⁷⁵¹ But sandy water could have created the Old Red Sandstone.

To explain the catastrophe, McLaren states:

"I shall, therefore, land a large or very large meteorite in the Paleozoic Pacific at the close of the [Devonian] Frasnian. Presumably on impact with the ocean surface or at a certain depth below the surface, the missile will explode with an enormous release of energy . . . the turbulence of the tidal wave and accompanying wind, followed by the gigantic runoff from land would induce a turbid [sandy] environment The hypothesis of meteoric impact in the ocean explains equally the non-extinction of many forms of both marine and terrestrial life."¹⁷⁵²

Why should Gould condemn Velikovsky's approach to the Old Red Sandstone as done rather poorly but does not condemn McLaren for suggesting the same process?

In essence, a distinguished paleontologist claims that a celestial impact created an enormous amount of sediment laden water during the Devonian and caused a large episode of extinction. If a catastrophe could do this, a catastrophe most certainly could have caused so much sediment to accumulate in areas so that it literally trapped and buried the fish observed in the Old Red Sandstone.

¹⁷⁴⁸Willy Ley, "Salamanders and Other Wonders"; (New York, 1955) p. 143.

¹⁷⁴⁹D. J. McLaren, "Bolides and Biostratigraphy," *Bull. Geol. Soc. Am.*, Vol. 94, (1983), pp. 313-324.

¹⁷⁵⁰D. J. McLaren, "Presidential address: Time, life and boundaries," *J. Paleontology*, Vol. 40 (1970), pp. 801-815.

¹⁷⁵¹*Ibid.*, p. 811.

¹⁷⁵²*Ibid.*, p. 812.

In fact, A. Hallam, of the University of Birmingham, England, admits: "There is now widespread acceptance that sedimentation is frequently a short-lived episodic process interrupted by much longer intervals of non-deposition; this is especially true of *siliciclastic* sediments in fluvial, lacustrine and marine environments. In many instances, nothing may happen for many years until a local [or non-local] catastrophic storm or tsunami event [occurs] . . ."¹⁷⁵³ The point is that it is admitted that sedimentation may not be the gradual process Gould invokes, but a catastrophic one. But Gould will not consider any of this as valid because it would contradict his uniformitarian interpretation. Gould, himself, discusses the concept of sedimentation rates with respect to the extinction of the dinosaurs and the iridium influx from space in terms of "predisposing biases."

". . . if you are testing sedimentation rates at the end of the Cretaceous, the assumption initially made was that the iridium influx was a constant, cosmic gentle rain from heaven that could [then be applied to] test differences in earthly rates. A totally different phenomenon [catastrophe] arose from an empirical surprise, not from an attempt to speculate, *à priori*, that extraterrestrial causes [were involved]. In retrospect we were right, but I'm not sure it was for the right reasons. Dave [Raup] liked it [a catastrophic interpretation] because he was interested in random processes; I liked it because I'm interested in punctuational processes. In other words, we had predisposing biases that made us look upon Alvarez [s catastrophic rapid sedimentation iridium rate at the end of the Cretaceous] favorably."¹⁷⁵⁴

Gould clearly has dogmatically refused to examine the evidence from another perspective. He maintains, in view of the evidence, that fish should have decomposed or been eaten by scavengers long before so many were entombed in sediment.

Why? Because Gould's uniformitarian analysis for the creation of fossil fish in the Old Red Sandstone defies the simple facts of nature, which he claims he is upholding. It is, in fact, the very thing that he accuses Velikovsky of having done. Essentially it is really Gould, and not Velikovsky, who begins with a working uniformitarian hypothesis He then attempts to find some physical explanation, however bizarre, regarding preservation of the innumerable fish with their soft tissues fossilized by *unique individual burials* in untold numbers that would render his interpretation mutually consistent and true. And I add, most scientists would do exactly the opposite in using the limits of physical possibility to judge. Did so many fish discover a unique method of dying and burying themselves rapidly to escape predation and decay? But even if the process Gould seemingly advocates is correct, where are the billions of other fish in every one of the other similar sediments formed in other sediment areas by this process under similar conditions? This is the kind of straitjacket conceptualization in which Gould has enmeshed himself.

Only by assuming his gradualist hypothesis as pure truth and ignoring McLaren's hypothesis can Gould then project this assumption as true and ignore the very nature of rapid deposition thereby projecting onto all the rest of Velikovsky's catastrophic evidence the same view to render it false. Is Gould really unaware that this is, in fact, all that he is doing? Please note, Gould cites no literature or evidence to suggest

¹⁷⁵³A. Hallam, "Catastrophism in Geology," *Catastrophism and Evolution*, ed. S. V. M. Clube (Cambridge, Eng., (1989) p. 35.

¹⁷⁵⁴William Glen, "On Mass-Extinction Debates: An Interview with Stephen Jay Gould," *The Mass Extinction Debates: How Science Works in a Crisis*, (Stanford, Calif., 1994), p. 257.

otherwise; not even McLaren or Hallam, who would be important references on this matter; he merely makes a bald claim and presents *the claim as fact when it is, in reality, his interpretation*.

This is the kind of bald statement Gould decries when employed by others. In his book review of Jeremy Rifkin, he attacks Rifkin's statement on Thomas Hunt Morgan's assessment of natural selection as a bit of circular reasoning by asking,

"... how do we know this? Rifkin cites no evidence ... even of any contemporary comment. He quotes Morgan himself only from secondary sources."¹⁷⁵⁵ But Gould has cited no sources, not even secondary ones, with regard to Velikovsky. What Gould has done is maintain a double standard with regard to citations. Velikovsky's citations are misrepresented as being exclusively out of date, but Gould raises criticisms based only on his own authority. He charges that Rifkin "cites no evidence ... even of any contemporary comment" which he attacks, but then goes on to misrepresent Velikovsky's citations giving no sources at all; again, this is a double standard of evidence.

In his and Niles Eldredge's theory of *Punctuated Equilibria*, he claims that this is exactly how *an assumed theory of uniformity distorts facts*:

"... We do not encounter facts as data (literally 'given') discovered objectively. All observation is colored by theory and expectation (See Vernon 1966, on the relation between expectation and perception). For a radical view, read Feyerabend's (1970) claim that theories act as 'party lines' to force observation in present channels, unrecognized by adherents who think they perceive an objective truth.

"... Theory does not develop as a simple and logical extension of observation; it does not arise merely from the patient accumulation of facts. Rather, we observe in order to test hypotheses and examine their consequences. Thus, Hanson (1970, pp. 22-23) writes: 'Much recent philosophy of science has been dedicated to disclosing that a "given" or a "pure" observation language is a myth-eaten fabric of philosophical fiction ... In any observation statement the cloven hoof print of theory can readily be detected ...'

"The inductivist view forces us into a vicious circle. A theory often compels us to see the world in its light and support. Yet we think we see objectively and therefore interpret each new datum as an independent confirmation of our theory. Although our theory may be wrong, we cannot confute it. To extract ourselves from this dilemma, we must bring in a more adequate theory; it will not arise from facts collected in the old way."¹⁷⁵⁶

In other words, Gould is merely using his theory, which may be wrong, to attack Velikovsky's catastrophic evidence, to interpret the sedimentation and fossilization record in the Old Red Sandstone because his viewpoint will not allow him to confute this gradualist interpretation. Yet that is the *modus vivendi* and *operandi* of his criticism. Even though he is well aware of this "party line" approach to these phenomena, he simply cannot extricate himself from this vicious cycle. "I know my uniformitarian theory is

¹⁷⁵⁵Stephen Jay Gould (H), "Integrity and Mr. Rifkin," *op. cit.*, pp. 236-237.

¹⁷⁵⁶Stephen Jay Gould, Niles Eldredge (F), "Punctuated Equilibria: An Alternative to Phyletic Gradualism," "Appendix"; in Niles Eldredge, *Time Frames*, (New York, 1985), pp. 194-195.

right, ergo Velikovsky's catastrophic evidence must be wrong." That is the reason I believe Thomas Kuhn claimed that, across the boundary between established scientific theory and a revolutionary scientific theory, there can be no communication of thought and evidence. Gould's criticism is an ideal delineation of this process expressed by Kuhn. The established theory has become a straitjacket.

In an interview, Gould was asked by sociologist of science, William Glen:

"How much [scientific] resistance comes simply because novel ideas impinge on long-established prevailing "truths" . . . other than catastrophism, in which the [scientific] community has a large stake—intellectual, personal, and otherwise?"

"Gould: [replied] That's a key issue. It's even worse for paleontologists because of *à priori* gradualism . . . [they] didn't even realize that gradualism was an empirical viewpoint with plausible and methodologically sound alternatives. They thought [gradualism] was part of the definition of what it meant to be scientific. Because Lyell had virtually gotten away with a fast one in arguing that because uniformity had methodological meanings that all science must accept then therefore he could sweep under the rug of acceptance the empirical meaning that had such a totally different status."¹⁷⁵⁷

As anyone can plainly see, it is Gould who is unable to extricate himself, like his other paleontologist colleagues, from his and their *à priori* gradualist understanding of the fossilization of fish in the Old Red Sandstone.

Like most paleontologists, Gould does not or may not realize that *his* gradualist interpretation is an empirical viewpoint with plausible and methodologically sound catastrophic alternatives. His gradualist viewpoint on these phenomena was part and parcel of his definition of what it means to be scientific. In this case, it is Gould, once again, who has virtually almost gotten away with a fast one in arguing that because his uniformity has methodological meaning, all science must accept that fossil formation in bodies of water occurred gradually; therefore, he can sweep under the rug (or under the sediment) of acceptance the empirical catastrophist meaning that has such a totally different status. That is the essence of Gould's evidence and argument with Velikovsky —his uniformitarian bias. He admits that he has "predisposing biases,"¹⁷⁵⁸ but has carefully managed, in this essay on Velikovsky, to keep his slant or spin on the information from his readers. And it is, ironically, Gould himself who rails against just such a dogmatic approach to science.

"If we equate uniformity with truth and relegate the empirical claims of catastrophism to the hush-hush unthinkable of theology [which is what Gould did in Bible smearing Velikovsky], then we enshrine one narrow version of geological process as true, *à priori*, and we lose the possibility of weighing reasonable alternatives. If we buy the simplistic idea that uniformity triumphed by fieldwork, [which Gould denies] then we will never understand

¹⁷⁵⁷William Glen, "On the Mass-Extinction Debates: An Interview with Stephen Jay Gould," *op. cit.*, p. 258.

¹⁷⁵⁸*Ibid.*, p. 257.

how fact and theory interact . . . and we will never grasp the biases in our own thinking (for we will simply designate our cherished beliefs as true by nature dictates).¹⁷⁵⁹

Ludwik Fleck remarked on how this bias works in his 1979 book, *The Genesis and Development of a Scientific Fact*, that what was real or true was already considered to be within the fabric of science: He stated,

"What we are faced with here is not so much simple passivity or mistrust of new ideas as an active approach which can be divided into several stages:

(1) A contradiction to the system appears unthinkable.

(2) What does not fit the system remains unseen;

(3) alternatively, if it is noticed, either it is kept secret, or

(4) laborious efforts are made to explain an exception in terms that do not contradict the system.

(5) Despite the legitimate claims of contradictory views, one tends to see, describe, or even illustrate those circumstances which corroborate current views and thereby give them substance.¹⁷⁶⁰

What does not fit Gould's concepts regarding the fossil fish in the Old Red Sandstone, he systematically excluded and forced outside the perimeter of his consciousness so it could have no existence.

(1) That the fossilization of these fish could have been sudden and catastrophic for him was unthinkable; (2) Since it did not fit his concepts, it remained unseen. (3) When it was noticed that the fossilization of so many fish by slow burial, made no sense because the fish would rot and be devoured by scavengers; (4) The process was still interpreted as slow burial so it would not contradict Gould's system of thought. (5) Despite the legitimate claims of Digby McLaren that there was an extinction in the Devonian which was caused by a celestial catastrophe that made the world's waters quite cloudy, and Hallam's legitimate claim that sedimentation is frequently a short-lived, episodic process which would create the Old Red Sandstone quickly, Gould refuses to see or examine those circumstances but still maintains only those circumstances which corroborate current views. He never realizes this and is aware of none of this! Why? Gould simply cannot extricate himself from his paradigm of science.

Put into historical and philosophical terms, Thomas Kuhn showed in his *The Structure of Scientific Revolutions* that the older scientists, associated by their work to established paradigms, are unable to

¹⁷⁵⁹Gould (D), *op. cit.*, pp. 115-214.

¹⁷⁶⁰Ron Westrum's "The Blind Eye of Science," *Whole Earth Review* (Fall 1986) p.38.

communicate with upholders of the new paradigms and are never converted by any evidence to a new theory. What they do, as does Gould, is interpret all evidence in terms of the only paradigm acceptable to them, that is, the establishment paradigm. As Max Planck, the originator of quantum theory, explains: "An important scientific innovation rarely makes its way by gradually winning over and converting its opponents: . . . what does happen is that its opponents gradually die out and that the growing generation is familiar with the idea from the beginning."¹⁷⁶¹ The stubborn, scientific resistant Gould apparently has reached the point where he will not deal with the anomalous nature of the Old Red Sandstone. According to Kuhn and Planck, other generations must grow up who are more familiar and comfortable with the rejected paradigm and not loaded with the psychological baggage of commitment to the established theory. They have gone beyond the biases of the older generations.

For example, Dorothy Stimson, in her classic book, *The Gradual Acceptance of the Copernican Theory of the Universe*, shows that even in England, long after Newton and Newtonian mechanics had scientifically and rigorously been installed as late as 1753, Pike wrote, "[m]any Common Christians to this day firmly believe that the earth really stands still and that the sun moves all around the Earth once a day."¹⁷⁶² Or as Paul Feyerabend states of Copernican theory,

"Compared with those facts, theories and standards the idea of the motion of the earth was as absurd as were Velikovsky's ideas when compared with the facts, theories and standards of the fifties."¹⁷⁶³

It took almost 200 years for the proponents of an Earth-centered universe to give up the ghost of their "cherished beliefs as true by their explanation of nature's dictates." To suggest, as some do, that there has been anything resembling a balanced review of the evidence regarding Velikovsky by Gould on these matter is sheer hypocrisy.

For example, Gould argues Velikovsky was wrong because "[h]e begins with a working hypothesis . . . then attempts to find some physical explanation, however bizarre, that would render all these stories both mutually consistent and true. Most scientists would do exactly the opposite in using the limits of physical possibility Velikovsky then sought to . . . generalize his physics by extending it throughout geological time."¹⁷⁶⁴ One asks: How did Charles Darwin formulate his theory of evolution as delineated in his *Origin of Species*?

Darwin's theory was based on certain assumptions that ran counter to the scientific evidence of his day. He based his theory on the evidence of the fossil record, breeding experiments, the age of the Earth and the nature of heredity, although he derived his concepts from other areas of research. In fact, the early chapters of *Origin* are devoted entirely to this breeding hypothesis. If Darwin had followed Gould's injunction, he would have rejected his hypothesis because it most definitely went beyond "the limits of physical possibility," known to all scientific breeders. In fact, Darwin admitted as much, "[t]hat a limit to variation does exist in nature is assumed by most authors [breeders] though I am unable to discover a single fact on which this belief is grounded."¹⁷⁶⁵ The single fact Darwin did not find was grounded in the knowledge that no breeder had ever created a new species, nor had anyone observed a new species

¹⁷⁶¹Max Planck, *The Philosophy of Physics*, (London, Eng., 1936), p. 90.

¹⁷⁶²Dorothy Stimson, *The Gradual Acceptance of the Copernican Theory of the Universe*, (Gloucester, 1972), p. 94.

¹⁷⁶³Paul Feyerabend, *Against Method*, (revised ed.), (New York, 1988), pp. 132-133.

¹⁷⁶⁴Gould (A), p. 154.

¹⁷⁶⁵Loren Eiseley, *The Immense Journey*, (New York, 1958), p. 186.

developed from another in nature, nor had there ever been found any series of large animal fossils in the earth showing gradual changes of fossils going from one species to another to another to another, and so on.

Luther Burbank, one of the most able breeders of that era, knew from breeding experience that there appeared to be a limitation of evolutionary change which he stated as a "law," guarding against Darwin's "physical explanation" of "natural selection."

"There is a law . . . of the Revision to the Average. I know from my experience that I can develop a plum half an inch long or one 2½ inches long, with every possible length in between, but I am willing to admit that it is hopeless to try to get a plum the size of a small pea, or one as big as a grapefruit In short, there are limits to the development possible, and these limits follow a law. But what law and why?

"It is the law that I have referred to above. *Experiments carried on extensively have given us scientific proof of what we had already guessed by observation; namely, that plants and animals all tend to revert in successive generations, towards a given mean or average In short, there is undoubtedly a pull towards the mean which keeps all things within some more or less fixed limitation.*"¹⁷⁶⁶ (Emphasis added)

This "law" has been confirmed repeatedly by innumerable experiments again and again up until today. Now a *law* of science which has never been contradicted by any form of breeding (for that is what natural selection is, *i.e.* a form of natural breeding) is fundamental.

As Loren Eiseley, a world renowned anthropologist, stated,

"It would appear that careful domestic breeding, whatever it may do to improve the quality of race horses or cabbages, is not actually in itself the road to the endless biological deviation which is evolution. *There is great irony in this situation, for more than almost any other single factor, domestic breeding has been used as an argument for the reality of evolution.*"¹⁷⁶⁷ (Emphasis added)

What scientists do not do, according to Gould, is refuse to accept the limitations of physical possibility. But Darwin absolutely rejected this limitation and failed to conform to Gould's injunction and criticism of Velikovsky on this matter. In spite of this "law" or "barrier to change," Darwin stated in the *Origin of Species*,

"Slow though the process of selection may be, if feeble man can do so much by his power of artificial selection [breeding] I can see no limit to the amount of

¹⁷⁶⁶Norman Macbeth, *Darwin Retried*, (Gambit, Ipswich, Mass., 1971), p. 36.

¹⁷⁶⁷Norman Macbeth, *op. cit.*, pp 35-36.

change, to the beauty and infinite complexity of the co-adaptation between all organic beings, one with another and with their physical conditions of life, which may be effected in the long course of time."¹⁷⁶⁸

Darwin first acknowledged that expert scientific evidence by scientific breeders and breeding experiments showed "that a limit to variation does exist in nature." This had been accepted by those scientists working in the field. Yet in spite of this admission and scientific testimony that there is a limit to change—a law—a barrier to change—Darwin denied the validity of this evidence and attempted to find some physical explanation that would render his working hypothesis consistent and true when, according to Gould, most scientists would do exactly the opposite. In the most basic terms, Darwin, contrary to Gould's injunction and criticism of Velikovsky, created his hypothesis of evolution using breeding suppositions which went beyond the "limit of physical possibilities" of breeding law and "sought to generalize his [bio]physics by extending it throughout geologic time."

In fact, one year after publication of *The Origin of Species*, Thomas Huxley wrote, "[b]ut there is no positive evidence, at present, that any group of animals has, by variation and selective breeding, given rise to another group which was even in the least degree, infertile with the first."¹⁷⁶⁹ And Darwin privately admitted Huxley's reservations as well in a letter to George Bentham: "The belief in natural selection must at present be grounded on general considerations When we descend to details . . . we cannot prove that a single species has changed, nor can we prove that the supposed changes are beneficial, which is the groundwork of the theory."¹⁷⁷⁰ Darwin admitted his view on breeding is only a "general consideration" and is only an assumption that neither he nor anyone else could prove!

And ultimately, even Huxley was compelled to admit this major impediment to the theory at the end of the 19th century. "I remain of the opinion . . . that until selective breeding is definitely proved to give rise to varieties infertile with one another, the logical foundation of the theory of natural selection is incomplete."¹⁷⁷¹

If such a method of doing science by going beyond the possibilities is so very wrong for Velikovsky, why is it so very right for Darwin?

Furthermore, Darwin's working hypothesis needed immense periods of time for his natural selection mechanism to work. Again, if Darwin had followed Gould's injunction and criticism of Velikovsky, he would not have done so because the laws of physical science known in Darwin's time contradicted this "working hypothesis." Sir William Thompson, better known as Lord Kelvin, had,

". . . in 1854 suggested 20 million years for the age of the Solar System, which he based upon the rate of cooling of the Sun and the Earth. For the next 40 years his estimates varied, and were commonly less than 100 million years. His last suggestion, in 1881 was that the limits were between 20 and 50 million years."¹⁷⁷²

¹⁷⁶⁸Charles Darwin, *The Origin of Species*, (London, Eng., 1859), p. 109.

¹⁷⁶⁹*Earth in Upheaval, op. cit.*, p. 244; see Thomas Huxley, "The Origin of Species" (1860), reprinted in *Darwiniana, Collective Essays*, (1883), Vol. II, p. 74.

¹⁷⁷⁰*Ibid.*, p. 245, see Darwin, *Life and Letters*, ed., Francis Darwin, Vol. II, p. 210.

¹⁷⁷¹*Ibid.*, see Huxley, *Darwiniana Collective Essays*, (1893), Vol. II, Preface.

¹⁷⁷²Digby J. McLaren, "Impacts and Extinctions," *The Mass Extinction Debates: op. cit.*, pp. 123-124.

According to Derek Gjertsen:

"As is well-known, Lord Kelvin, the leading British physicist of his day, dismissed Darwin's work on the ground that it violated the principles of thermodynamics. The sun could be no more than 100 million years old; evolution demanded a much longer period in which to operate; therefore evolution must be rejected. Kelvin wasted no time pursuing the minutiae of the geological and paleontological evidence on which evolution was based. Physics in the guise of thermodynamics had spoken clearly and whatever failed to fit into its scheme had to be rejected."¹⁷⁷³

According to Francis C. Haber, Kelvin, in his paper "The Doctrine of Uniformity in Geology Briefly Refuted," in *Popular Letters and Addresses* (London and New York, 1894), "accused the uniformitarians of adopting an endless cycle of time in accord with the principle of perpetual motion, and of disregarding the effects of tidal friction and the dissipation of energy."¹⁷⁷⁴ Darwin, aware of this problem, wrote to James Croll: "I am greatly troubled at the short duration of the world according to Sir W. Thompson, for I require for my theoretical views a very long period *before* the Cambrian formation."¹⁷⁷⁵ Darwin was fully aware that his theory of long, almost endless, time was fully contradicted by Kelvin's scientific evidence.

This time restraint was based on the only known physics of that time of the proper age of the Earth and sun which clearly precluded Darwin's "working hypothesis" which required ages, almost without limit, for natural selection to work. As Gould states, "the physics of his [Kelvin's] day included no force capable of generating new heat [in the earth] . . . he had admitted his crucial dependence upon no new source of energy . . ."¹⁷⁷⁶ Darwin claimed the gradualist interpretation of geology, that sedimentary layers required larger periods of time for erosion to form sediments and thus should overrule clear cut physical laws: thus Darwin ignored this further fundamental contradiction to his "working hypothesis and attempted to find some physical explanation however bizarre, *at that time*, that would render his hypothesis consistent and true when scientists would do exactly the opposite in using the limits of physical possibility." Darwin then sought to generalize his cosmic time theory by extending it throughout an invented and elongated geologic time. But Darwin is Gould's hero and Velikovsky is the villain so what Gould applauds in Darwin he condemns in Velikovsky.

In fact, Gould admits that Darwin began "with a fanciful theory" and not by "scientific induction." In his essay, "Darwin's Middle Road," in *The Panda's Thumb*, Gould reminds us that although it has often been presented that Darwin derived his theory from the study of finches and turtles inhabiting the Galapagos Islands, Darwin, himself, contradicted this concept when he wrote from South America in 1834. "I have not one clear idea about cleavage, stratification, lines of upheaval. I have no books which tell me much and what they do I cannot apply to what I see. In consequence I draw my own conclusions, and most gloriously ridiculous ones they are."¹⁷⁷⁷

¹⁷⁷³Derek Gjertsen, *Science and Philosophy*, (London, 1989), pp. 163-164.

¹⁷⁷⁴Francis C. Haber, *The Age of the World*, (Baltimore, 1959) p. 289.

¹⁷⁷⁵*Ibid.*, p. 290.

¹⁷⁷⁶Stephen Jay Gould (G), *The Flamingo's Smile*, (New York, 1983), pp. 136-137.

¹⁷⁷⁷Stephen Jay Gould (I), "Darwin's Middle Road," *The Panda's Thumb*, (New York, 1980), p. 63.

According to Gould, "[h]e [Darwin] began with a fanciful theory involving the idea that new species arise with a prefixed life span, and then worked his way gradually, if fitfully, towards an idea of extinction by competition and world struggle."¹⁷⁷⁸ (Emphasis added) Thus, Gould criticizes Velikovsky for beginning "with a working hypothesis," while Darwin is to be lauded who "began with a fanciful theory." And of course, while Darwin admitted that his views were "gloriously ridiculous," Velikovsky goes down "gloriously." At this point, I do not think it amiss to suggest to Gould that he sit down calmly and reread his own writing, which he so engagingly contradicts almost every time he opens his mouth to criticize Velikovsky.

Furthermore, Darwin's theory was based on the fossil record. If his theory was correct and species gradually changed over the long ages, as Darwin had proposed, based on Lyell's concepts, then there should have been found innumerable lineages of fossils gradually leading from one form to another and another and so on. He had literally wagered his theory on finding long, gradual lineages in the fossil record. "The geological [record] is extremely imperfect and this fact will, to a large extent, explain why we do not find interminable varieties [transitions] connecting together all the extinct and existing forms of life by the finest graduated steps. He who rejects these views on the nature of the geological record will rightly reject my whole theory."¹⁷⁷⁹ But these untold lineages were not found.

As David Raup, curator of the Field Museum in Chicago, where 20 percent of all known fossils are kept, states,

"Most people assume that fossils provide a very important part of the general argument made in favor of Darwinian interpretations of the history of life Well, we are now about 120 years after Darwin, and knowledge of the fossil record has been greatly expanded Ironically, we have *even fewer examples* of evolutionary transition than we had in Darwin's time."¹⁷⁸⁰

Even Gould admits that there never was a clear set of lineages showing gradual change in the fossil record. "Phyletic gradualism [gradual evolution] . . . was never seen in the rocks."¹⁷⁸¹ Elsewhere, Gould writes, "we never see the process [gradual evolution in the fossil record] we profess to study."¹⁷⁸² In *The Panda's Thumb*, Gould further states on page 181 that "The extreme rarity of transitional forms in the fossil record persists as the trade secret of paleontology Gradualism . . . was never 'seen' in the rocks." On page 188 Gould adds, "The fossil record with its abrupt transitions offers no support for [Darwinian] gradual change." Based on Gould's injunction and criticisms of Velikovsky, there was never a scientific basis in the fossil record to support Darwin's claim of gradual evolution. Even Darwin admitted "[g]eology assuredly does not reveal any such finely graduated organic change [in the fossil record of intermediate forms]; and this, perhaps, is the most obvious and gravest objection which can be urged against any theory [of evolution]."¹⁷⁸³ The fossil record of that time did not show the gradual evolutionary transformation of one organism changing into another, to another, and so on. This precluded Darwin's "working hypothesis." But did this deter Darwin as Gould's injunction requires of Velikovsky? No, indeed! Darwin claimed the fossil record was incomplete and distorted and overruled this missing fossil record and then sought to generalize

¹⁷⁷⁸*Ibid.*, p. 65.

¹⁷⁷⁹*Origin*, (1959), p. 342.

¹⁷⁸⁰Rifkin, *Algeny*, *op. cit.*, p. 125.

¹⁷⁸¹Jeremy Rifkin, *Algeny*, (New York, 1983), p. 130.

¹⁷⁸²Stephen Jay Gould, "Evolution's Erratic Pace," *Natural History*, (May 1977), p. 14.

¹⁷⁸³Rifkin, *op. cit.*, p. 124.

his interpretation of the fossil record throughout geological time as true when scientists would do just the opposite!

Finally, there was the view current in the time Darwin formulated his theory as outlined by Steven M. Stanley, showing Darwin had a

". . . thorny problem with heredity. In the *Origin* (p.13) he lamented '[t]he laws governing inheritance are quite unknown.' Throughout his life, Darwin was saddled with the contemporary notion of blending inheritance . . . the idea that two parents' traits were averaged to form the genetic endowment of their offspring. Obvious departures were assumed to reflect environmental influences . . . It was argued, in particular, by the engineer, Fleeming Jenkin in 1867, that blending inheritance posed fatal difficulties for natural selection. Any new structure of some value would pass into oblivion [or as breeders knew, returns to the average] through generation-after-generation dilution with other structures, that because they were present earlier, prevailed numerically within the population."¹⁷⁸⁴

If a useful chance variation arose in a group of organisms, its offspring would inherit at best 50 percent of this feature, the next generation 25 percent, the next generation 12.5 percent and in a few more generations the useful variation would disappear into the overall makeup of the population.

Darwin responded with a thoroughly *ad hoc* new theory of inheritance, "pangeneses." This theory holds that all the cells of an organism throw off very minute gemmules or pangens which circulate through the body to the sex cells and develop buds or germ cells which have the power of reproduction and contain the units of heredity which is then transmitted to offspring. This theory was quickly put to the test and was found to be incorrect!

"At this point, Darwin, according to the rules of prevailing methodology, should have acknowledged the weakness of his position and abandoned his position."¹⁷⁸⁵ The experiment carried out in 1871 by Francis Galton to test Darwin's theory proved his inheritance concept worthless.¹⁷⁸⁶

According to the only heredity consideration widely known in Darwin's time, the development of a new species was impossible. Thus, based on Gould's injunction, and criticism of Velikovsky, this was a fundamental restraint on Darwin's hypothesis which ran counter to what was then currently held as inheritance. Darwin was fully aware of this impediment admitting in *Origin*,

"It may be doubted whether sudden and considerable deviations of structure, such as we occasionally see in our domestic productions, more especially with plants, are ever permanently propagated in a state of nature . . . They would also, during the first and succeeding generations, cross with the ordinary form and thus their abnormal character would almost inevitably be lost."¹⁷⁸⁷ [In the

¹⁷⁸⁴Steven M. Stanley, *The New Evolutionary Timetable*, (New York, 1981), pp. 61-62.

¹⁷⁸⁵Derek Gjertsen, *Science and Philosophy*, (London, 1989), p. 107.

¹⁷⁸⁶D. W. Forest, *Francis Galton*, (London, 1974), pp. 102-109.

¹⁷⁸⁷Stanley, *op. cit.*, p. 136.

fifth edition of *Origins*, Darwin wrote on page 125] "the new variation would generally be lost by subsequent intercrossing with ordinary individuals."

Did this heredity problem compel Darwin to abandon his hypothesis, as Gjertsen says, which ran against the accepted scientific view of the blended genealogy of species? No, of course not! He claimed gradual evolution occurred when scientists would do just the opposite, and then claimed his heredity hypothesis should be envisaged as running through all past biological ages!

Darwin rejected the best physics estimating the age of the earth. He rejected the biological laws of breeding research; he rejected the fossil record, all of which were negative to his theory, and he rejected the concepts of heredity of his day. If Darwin's rejection of these scientific fundamentals makes him a great scientist, then Gould's injunction is simply fraudulent if it is applied to Velikovsky, because Gould lauds Darwin for conceiving these concepts and upholding them in the face of negative evidence on each of them.

Gjertsen fully acknowledges that what Gould describes as the proper form of science is a form of anti-science.

"The method of science consists in taking a hypothesis such as natural selection and pursuing its implications and consequences wherever they lead *despite* any unfavorable predictions or explanatory failures. It could even be said that it is precisely this feature which identifies the creative scientist. Obsessively he will seize upon an idea and develop it against all opposition and against all counter-evidence, for the rest of his life."¹⁷⁸⁸

And for all the rest of Darwin's life, not one of these four major phenomena, breeding, fossils, the age of the earth, and heredity, ever gave support to his theory. But Darwin tenaciously held on to his theory, whereas if he had followed Gould's criticism as it applied to Velikovsky, he should have dropped it. But even on this point Gould is disingenuous because he does not believe at all that one must begin by working within the established concepts and laws of science. And he says so directly. In discussing David Raup's concept of mass extinction based on impact catastrophes, he once again, in dealing with one of his colleagues as opposed to his dealings with Velikovsky, states the following: "If Dave has a motto, it can only be: Think the unthinkable (and then make a mathematical model to show how it might work); take an outrageous idea with a limited sphere of [scientific] validity and see if it might not be extendable to explain everything [His] book [on extinction] is a wonderful exposition of this potentially valid iconoclasm."¹⁷⁸⁹ Neither Darwin nor Velikovsky made a mathematical model but in all other respects they did follow the "wonderful exposition" that Gould calls valid and lauds. Who can trust such a man who attacks Velikovsky for what he applauds in Raup? Gould has given the impression that this injunction of his regarding the proper approach to scientific discovery is actually followed by real scientists, but that is sheer nonsense, since he fully admits that this is not what occurs. "But scientists who make discoveries rarely follow this optimal pathway of . . . logical reconstruction. Scientists reach their conclusions for the damndest of reasons: intuition, guesses, redirections after wild goose chases, all combined with a dollop of rigorous observation and logical reasoning"¹⁷⁹⁰ That is, he lambastes Velikovsky for things he fully admits play little role in scientific discoveries: hypocrisy, once again.

¹⁷⁸⁸Gjertsen, *op. cit.*, p. 108.

¹⁷⁸⁹Stephen Jay Gould, "Introduction," in David Raup's *Extinction*, (New York, 1991), p. XVI.

¹⁷⁹⁰Stephen Jay Gould (J), "In the Mind of Beholder," *Dinosaur in a Haystack*, (New York, 1995), p. 96.

Gould's criticism, as I see it, is not logical, that is, based on a rational consideration of standards; rather it is visceral, based on psychological and sociological standards, which should have no place in such debates.

As proof of this visceral standard, I cite Peter James who also dealt with Gould's criticism of Velikovsky:

"Gould's final comment is his most extraordinary: 'But all these criticisms pale to insignificance before the most conclusive refutation of Velikovsky's examples—their explanation as consequences of continental drift and plate tectonics' (Gould, 'Velikovsky in Collision,' page 157). Roughly translated, this means: 'But the real reason I believe he is wrong is because I happen to subscribe to a different theory.' Strangely enough, the theory of continental drift was a heresy when *Earth in Upheaval* was written, and Velikovsky rejected it, a 'victim,' Gould thinks, 'to this great revolution in scientific thought.' In reply, one could warn against Gould's heavy-handed and reductionist use of continental drift and plate tectonics—certainly very popular theories at present and widely used as 'catch-all' explanations for a wide range of geological and paleontological problems—by pointing out that there is still dissent from the consensus, both on the reality of continental drift and the description of the mechanism behind it in terms of plate tectonics and sea-floor spreading. But Gould sounds the warning himself. *A mere eight pages* after his 'most conclusive' refutation of Velikovsky's example Gould discusses a classic case illustrating how things are distorted by the wearing of continental drift glasses and admits: 'The new orthodoxy colors our vision of all data; there are no "pure facts" in our complex world . . . Many readers may be disturbed by my argument for the primacy of theory. Does it not lead to dogmatism and disrespect for fact? It can, of course, but it need not. The lesson of the history holds that theories are overthrown by rival theories, not that orthodoxies are unshakable. In the meantime I am not distressed by the crusading zeal of plate tectonics, for two reasons. My intuition, culturally bound to be sure, tells me that it is basically true. My guts tell me that it is damned exciting—more than enough to show that conventional science can be twice as interesting as anything invented by all the von Danikens and all the Bermuda triangles of this and previous ages of gullibility.'

"At this point I had better allay the reader's disbelief by assuring him that these words *are* directly quoted from Gould's book. Let us follow the train of thought again—catastrophism is completely disproved by the fact that the evidence supporting it can also be interpreted by theories of continental drift; Gould appreciates that these theories—the currently popular paradigm—could be misinterpreting data; but he knows intuitively that the theories are true, while they are more exciting than theories of extraterrestrial visitations. *i.e.*, interpreting everything in terms of continental drift is right because it feels

so, and is more exciting than von Daniken; ergo Velikovsky is conclusively refuted."¹⁷⁹¹

James then goes on to show the utter irony of Gould who attacks Velikovsky's catastrophic view but then acts oblivious to the concept of catastrophes associated with the great extinctions of the Permian, or the extinctions of the dinosaurs at the end of the Cretaceous. Now Gould, it must be pointed out, champions the concept that extraterrestrial agents, either comets, asteroids, etc., were responsible for these mass extinctions. His and Eldredge's theory of Punctuated Equilibria is based, in part, on just such extinction episodes.

Since Gould maintains that plate tectonics is the ultimate refutation of Velikovsky's extraterrestrial catastrophic scenario, isn't plate tectonics the ultimate refutation of these other extraterrestrial catastrophic theories?

Heaven forbid! And how does Gould know that plate tectonics disproves Velikovsky's theory but not the extraterrestrial theories for these other mass extinctions? His intuition (bias) and guts tell him so. Gould, of course, has berated Velikovsky for believing everything in ancient myths, especially the Bible, as true. What Gould has chosen to replace these criteria are his intuition and guts, quite novel scientific criteria, to say the least. In the ancient world, priests would disembowel animals and examine their entrails or livers to understand the truth about the outcome of the future. Gould, has apparently refined their methods for "scientific investigation" by discovering which theories cause his own entrails to become inflamed, which means whatever theory does so is *ipso facto* true. Therefore, what scientists might do to validate their theories is to send Gould a copy to read. After reading it, the truth will be known; if Gould's guts remain lethargic the theory is invalid; if however, Gould's bowels go into an uproar the scientific world has the glorious new scientific truth. But all this aside, what I said earlier that Gould's refutation of Velikovsky is visceral is only related to his biases and not to the evidence which he rejects on clearly *à priori*, biased theoretical grounds from start to finish.

Along these same lines, Gould argues that Velikovsky draws an "inference [that] worldwide [catastrophic] events from *local* catastrophes occur by flooding, earthquake, or volcanic eruption."¹⁷⁹² But if local catastrophes explain away Velikovsky's world wide catastrophes, don't they also explain away other forms of celestially visited catastrophes to the earth caused by comets, asteroids, etc., as Gould supports? Heaven forbid! And how does Gould know *his* extraterrestrial type catastrophes, are not local catastrophes, while Velikovsky type catastrophes are? His guts tell him so, or more accurately his biases do!

This precisely is the reason Darwin claimed that *Origins* would be rejected by the Goulds, Sagans and others for his own generation. Himmelfarb explains,

"When Darwin predicted that his book would find more favor with intelligent layman than with professional scientists, it was because he thought the scientists were too committed to the old conception of species to admit new ideas on the subject."¹⁷⁹³

Gould is, in essence, absolutely no different than the critics of Darwin, whom he has often castigated.

¹⁷⁹¹Peter James, "Ever Since Darwin," A Review, *KRONOS*, Vol. VII, No. 4, (Summer 1982), pp. 29-30.

¹⁷⁹²Gould (A), *op. cit.*, p. 156.

¹⁷⁹³Gertrude Himmelfarb, *Darwin and the Darwinian Revolution*, (New York, 1962), p. 280.

However there is one final point that must be made regarding Gould and his theory of Punctuated Equilibria which is undoubtedly related to Velikovsky's book, *Earth in Upheaval*. The clearest, short exposition of the catastrophic aspect of Gould and Eldredge's theory that I have read is that presented by Jeremy Rifkin in his book, *Algeny*. Though Gould deplores this essay, it does, in fact, accurately delineate this aspect of Gould's evolutionary theory.

"Uneasy over the fact that the fossil record shows little or no evidence of intermediary forms, a new generation of paleontologists, led by Harvard's Stephen Jay Gould and by Niles Eldredge of the American Museum of Natural History, have advanced the thesis of 'punctuated equilibria' as an alternative to the orthodox view of gradual evolutionary development. According to this new theory, species change little if at all over vast stretches of time. Stasis, they argue, is the dominant mode in nature. However, occasionally that stasis is interrupted for a very brief moment of time. Suddenly, a small population from a parent species becomes isolated geographically and begins to evolve rapidly into an entirely new species. This rapid speciation might take place over 50,000 years, which in geologic terms is a mere speck of time. According to Gould and Eldredge, rapid speciation accounts for the absence of intermediary forms in the fossil record. The evolutionary change, they contend, takes place with such lightning speed that the geological record would hardly have time to record it [in the fossil record]. Gould and Eldredge's theory, then, rests on the assumption that the history of evolution shows long periods of homeostasis punctuated by short periods of very sudden speciation.

"But then the question arises as to what kinds of circumstances could have led to periodic isolation and rapid speciation. The new answer is sudden catastrophe. Unlike Darwin, who argued that the physical environment is relatively constant, a new generation of geologists is arguing that, in fact, the earth's history has been punctuated with severe catastrophic events, some of a global nature. These catastrophes, which probably included massive floods, plagues, earthquakes, volcanic eruptions, meteoric rainstorms, intergalactic disturbances were likely responsible for the periodic isolation of offspring from parent stock and the rapid speciation that ensued. The idea is that these catastrophic events spawned monstrous genetic mutations within existing species, most of which were lethal. A few of the mutations, however, managed to survive and became the precursors of a new species.

"Catastrophic theory and the theory of "punctuated equilibria" are transition arguments on the road to a comprehensive temporal theory of evolution. After all, what are catastrophic occurrences but sudden and dramatic shifts in the external periodicities or rhythms? Catastrophes introduce massive, overwhelming novelty into the environment. The sudden speed-up of external periodicities is far greater than what can be anticipated and absorbed by existing organisms. The biological clocks of existing species are disrupted and thrown off track. In the wake of these catastrophic crises, new organisms

emerge that are able effectively to incorporate the new external rhythms. These organisms embody entirely new temporalities. They are, in other words, new species."¹⁷⁹⁴

But what has all this to do with Velikovsky? Velikovsky has presented almost exactly the same catastrophic evolutionary theory as that just presented. In the book, *Earth in Upheaval*, that Gould has chosen to indict, Velikovsky presents this theory in his chapter "Cataclysmic Evolution," which contains the following account of his evolutionary theory written in 1955, several years prior to Gould and Eldredge's punctuated equilibria concept.

"[W]e are led to the belief [writes Velikovsky] that evolution is a process initiated by catastrophes. Numerous catastrophes or bursts of effective radiation must have taken place in the geological past in order to change so radically the living forms on Earth, as the record of fossils embedded in . . . sediments bears witness.

"How could this understanding of evolution meet the facts, and especially those facts that always appeared to be in discord with the theory of natural selection? . . .

"The fact that the geological record shows a sudden emergence of many new forms at the beginning of each geological age does not require the artificial explanation that the [fossil] records are always defective; the geological records truly reflect the changes, in the animal and plant world from one period of geological time to the next. Many of the new species evolved in the wake of a global catastrophe, at the beginning of a new age, were entombed in a subsequent paroxysm of nature at the end of the age.

"The fact that in many cases the intermediary links between present-day species are missing, as well as those between various species of the geological record, a vexing problem, is understandable in the light of sudden and multiple variations that gave rise to new species. . . .

"The fact that at several stages of the past many animals of various species and many species in toto were rather suddenly exterminated, in conflict with the idea of slow extinction in natural selection, conforms with the theory of cataclysmic evolution. . . .

"Great catastrophes of the past accompanied by electrical discharges and followed by radioactivity could have produced sudden and multiple mutations on an immense scale."¹⁷⁹⁵

¹⁷⁹⁴Jeremy Rifkin, *Algeny, op. cit.*, pp. 192-194.

¹⁷⁹⁵Velikovsky, *Earth in Upheaval, op. cit.*, pp. 257-259.

There are definite differences between Gould and Eldredge's theory and that of Velikovsky. Gould and Eldredge posit both small and large changes due to internal geological and terrestrial forces, as well as catastrophic extraterrestrial impacts. Further from the evolutionary standpoint, they require changes over tens of thousands of years, while Velikovsky envisaged massive mutations creating major species changes from one form to totally new ones much more rapidly. In all other respects, especially with regard to the catastrophic aspects, both theories are quite similar. To quote William James, the American psychologist, with respect to how new theories are handled by scientific establishment advocates, he wrote that these theories are "[f]irst . . . attacked as absurd; then it is admitted to be true but obviously insignificant; finally it is seen to be so important that its adversaries claimed that they themselves discovered it."¹⁷⁹⁶

It becomes self-evident that if Velikovsky's theory of "Cataclysmic Evolution," as expounded in *Earth in Upheaval*, "goes down gloriously," then, so too, does that catastrophic aspect of Gould and Eldredge's theory of Punctuated Equilibria! If extraterrestrial cataclysms cannot produce evolution in terms of Velikovsky's theory, then they cannot produce evolution in terms of Gould and Eldredge's theory! Naturally, Gould never, ever mentions in his criticism the similarity of these two catastrophic evolutionary concepts because it would expose the glaring inconsistency of his criticism. And that is the fundamental problem with Gould's critique. On almost every point or area in contention, Gould is disingenuously inconsistent: caricaturing another's theory is wrong, but Gould does not hesitate to caricature Velikovsky's theory. Bible baiting catastrophist theory is a "vulgar misrepresentation," but Gould Bible baits Velikovsky. Misrepresenting history is wrong, yet Gould misrepresents Velikovsky for supposedly using "exclusively outdated sources." Being dogmatic about one's uniformitarian theories to exclude thinking along catastrophic lines based on "à priori gradualism" when other empirical, methodologically sound alternatives exist, is unscientific, but Gould interprets all the evidence to criticize Velikovsky only on the basis of "à priori gradualism" equating it with scientific "truth." Velikovsky "begins with a working hypothesis" which is counter to what most scientists would do, yet Gould still lauds Darwin who "began with a fanciful theory," and David Raup for "thinking the unthinkable" and employing "an outrageous idea with a limited sphere of validity [to] see if it might not be extendable to explain everything." Plate tectonics is a definitive "refutation" of Velikovsky's extraterrestrial catastrophes, yet Gould is a leading exponent of extraterrestrial catastrophes to support this aspect of his and Niles Eldredge's evolutionary theory.

All in all, what can one say about Gould's criticism? Gould has accused Velikovsky of "carelessness, inaccuracy and slight of hand" [in] *Earth in Upheaval*.¹⁷⁹⁷ But, Gould is the one who was careless, inaccurate and pulled several "fast ones." It is not, as Gould states, that Velikovsky devised a cataclysmic theory of human history . . . [and] then sought to generalize his physics by extending it through geologic time, it is rather that Velikovsky devised a cataclysmic theory of evolution which he extended throughout geologic time prior to, and in one major aspect, quite similar to, that later introduced to the scientific world by Gould that may be so *gut wrenching* to his critic. As Gould states,

"People ask me all the time what I was thinking when Niles Eldredge and I first developed the theory of punctuated equilibrium in the early 1970's [over fifteen years after Velikovsky presented his theory of "Cataclysmic Evolution" in *Earth in Upheaval*]. I tell them to read the original paper, for I don't remember (or at least cannot find those memories amidst the jumble of my subsequent life."¹⁷⁹⁸

¹⁷⁹⁶Marcia Bartusiak, *Thursday's Universe*, (New York, 1986), pp. 170-171.

¹⁷⁹⁷Gould (A), *op. cit.*, p. 157.

¹⁷⁹⁸Stephen Jay Gould, *Wonderful Life, the Burgess Shale and the Nature of History*, (New York, 1989), p. 81 note.

Perhaps reading this material may jog Gould's memory from his amnesia, and he may even use "up-to-date sources" such as Velikovsky's *Earth in Upheaval* to explain this apparent lapse of memory. I believe, that Gould does not inform his audience of this striking similarity between his and Velikovsky's theories, especially of catastrophic evolution, because it would alert his readers to the fact that there is something much deeper and much more disturbing than "carelessness, inaccuracies and slight of hand" operating in his critique. But Gould has nothing to worry about since it's quite clear that his colleagues, at worst, will find his criticisms of Velikovsky "foolish" and not "evil." How could they? After all, he is attacking Velikovsky, and any attacks on Velikovsky, no matter how flawed or dishonest, are a good deed for science, while people like Gordon Atwater, who are fired from doing their scientific jobs, and are subsequently blacklisted from ever working in the field of science again, are the ones who are the baddies of science?

And I conclude by presenting an altered version of Gould's own reaction to a book he claims lacks integrity and which he deplures.

I will state my conclusion – bald and harsh. I regard "Velikovsky in Collision" by Stephen Jay Gould as a cleverly constructed tract of anti-intellectual propaganda masquerading as scholarship. Among essays promoted as serious intellectual statements by important thinkers, I don't think I have ever read a shoddier work. Damned shame, too, because the deep, issue is troubling and I do not disagree with Gould's basic plea in his other writings for respecting the integrity of cosmic catastrophism. But devious means compromise good ends and we shall have to save Gould's catastrophic views and conclusions from his own lamentable tactics. His arguments lack integrity. This we deplore!¹⁷⁹⁹ All that Gould has done in order to reject Velikovsky's ideas is argue from his own rigid preconceptions of what he construes to be science. But again, his own words show this is nothing but hypocrisy. "Nothing is more dangerous than a dogmatic world view – nothing more constraining, more blinding to innovation, more destructive to openness to novelty."¹⁸⁰⁰

Tragically, I must in my own words conclude, that the history of the second generation of Velikovsky's critics will be ignored by those "glorious" doyens of the establishment in science, academia and the scientific press. And the "Velikovsky Affair," with all the abuse and disregard for the truth, will continue down the long and dirty road as far as I can see. Have these individuals, who so misrepresented Velikovsky, no shred of decency or integrity? How in the name of honest scholarship could they have perpetrated so much mean spirited behavior for so long and not been exposed by their colleagues?

Each and every one of Velikovsky's critics described in this book are either highly respected members of the scientific community or scientific journalistic community. And yet these critics did not refrain from misrepresenting Velikovsky's evidence, concepts, motivations, or methodology. While perpetuating all this venality, they professed, at the same time, to follow the highest ideals of scientific discourse. The esteemed position of these critics did not stop them from attempting to emasculate Velikovsky's work and methodology nor from attempting to emasculate Velikovsky, himself (crank, crackpot, pseudoscientist) in order to leave him not a shred of dignity. And all this was done in the name of humanistic science and rationalism. Each of the critics claimed that they were not out to brutalize the man they criticized, but then they brought forth all the techniques of slander and misrepresentation as weapons against Velikovsky and his ideas. The use of such weapons are never the modalities of humanistic science or rationalism; they are the weapons of propagandists. It has been and will continue to be, I believe, dehumanizing modalities of behavior that will be employed to answer Velikovsky.

If at times this author has appeared to respond in anger to such emasculating, dehumanizing criticism of Velikovsky and his ideas, it is what this kind of criticism often evokes, and for which I apologize. Scientists and critics of Velikovsky and his work who resorted to this level of behavior have only demonstrated their own emotional insecurities about the nature of their scientific beliefs. The crusade

¹⁷⁹⁹See Gould (H), *op. cit.*, p. 230-239.

¹⁸⁰⁰Gould (J), *op. cit.*, p. 96.

against Velikovskianism by the scientific establishment, whether or not Velikovsky is ever vindicated, is still one of the meanest, ugliest, and nastiest episodes in the history of modern science. The lies told in the name of scientific criticism by these individuals have been circulating among the scientific and journalistic community and the public. They were presented to destroy Velikovsky and Velikovskianism. But like those lies presented by the first generation, they have failed. Velikovskianism will continue to grow in spite of these critics and their filthy methods.

Galileo, after his own humiliation at the hands of other men, wrote,

". . . the wrongs and injustices which envy and malice have engineered against me did not and do not trouble me. Rather . . . the magnitude of their injuries has tended to console me . . . and the infamy rebounds upon those who are deceitful and those with the highest degree of ignorance—mother of malice, envy, rage and all other wicked and stupid vices and sins

"Unhappy is our land, in which reigns a fixed resolve to exterminate anything new, especially in the sciences—as if everything worth knowing were already known."¹⁸⁰¹

Deceit and lies were used to destroy Galileo, as they were used to destroy Velikovsky, by the second generation of his critics, just as with the first.

But too much evidence fits his theory uniquely as no other. This evidence, in part, discussed above and elsewhere, still indicates Velikovsky's views are well supported and well worth perusing in spite of the calumny displayed against him and his ideas by Gould and the others exposed in this volume.

The house of science should be built not only on foundations of evidence, experiments and other methods of proof, but it should also be built on foundations of ethics, truthfulness and justice. These foundations come before all others. But the critics of Velikovsky have displayed total indifference to this second foundation and, therefore, have built an edifice that can only be described as a latrine. As Thomas E. Phipps, Jr. stated on page 1 of "An Unsolicited Essay in Science Criticism," submitted to *Aperion*:

"By using brick and mortar to build a latrine one can make it a very solid and enduring structure; and by decorating its interior with French wallpaper one can make it a thing of beauty, admired for generations. But it is still a latrine, as can be verified by invoking the sense of smell."

Anyone who has read this volume to this point and has a sense of ethics, of truthfulness and justice, will realize that the house of criticism of Velikovsky, erected by the second generation of his critics, is nothing less than a highly ornate, cleverly constructed facade of an edifice they deemed to be good science. In reality, with its facade exposed by the evidence presented in this book, the foulness of their moral corruptibility permeates everything and leaves one with the unmistakable impression that all that these critics built was nothing more than a heap of trash and falsehood!

This is what they have constructed and offered to the world in the sacred name of science. Nevertheless, they know that they can rest assured that none of their colleagues in science, nor the journals of

¹⁸⁰¹Ludovico, Geymont, *Galileo Galilei*, transl. Stillman Drake, (New York, 1965), pp. 160-161.

science, nor the journalists of science will ever say their work on Velikovsky is trash. But that is all it ever was. It will soon be evident that even this book will be ignored, misrepresented, and suppressed by the scientific establishment!

To quote William Warren Bartley, III, speaking of another heretic is apropos to the treatment meted out to Velikovsky.

"Many will not mention or cite him, yet scrupulously cite . . . [those] who publicly disagree with him . . . one can make a career as an opponent of . . . [his] ideas but not as a proponent of them. He is 'fair game': one may say anything about him — and steal any of his ideas . . . — with impunity."¹⁸⁰²

Like any stateless person, Velikovsky is "fair game" and may be persecuted by *establishment types* who feel the need to act out with impunity any and all levels of intellectual mistreatment upon him and his ideas. That this kind of behavior could continue to be perpetrated for fifty years by scientists, science writers, science journalists and the others speaks eloquently to the fact that the veil of dignity of science, academia and the press was removed to allow actions against Velikovsky that are shameful.

Some of those responsible for the continuing Velikovsky Affair are tremendously respected. This book has removed their "cloak of propriety," but I believe it will change nothing. After fifty years of massive pathological behavior toward Velikovsky and his ideas on the part of the scientific establishment, to avoid responsibility for this behavior requires massive pathological denial of what they did and what their colleagues have allowed. To admit that horrendous behavior was perpetrated on such an immense scale is simply too much to deal with, too difficult to face squarely, and thus, I believe the establishment will deny the Velikovsky Affair with deafening, neurotic silence.

* * * * *

A RAGE TO DENY: THE ROOTS OF THE VELIKOVSKY AFFAIR, By Irving Wolfe

To anyone who has read the chapters of this book, it is painfully evident that the Velikovsky Affair and the Continuing Velikovsky Affair are very real and sad events in the history of modern and especially American science. Together, they constitute a virulent syndrome that seems to run unabated from its eruption in 1946 to the present day and which can, therefore, be analyzed as a coherent, ever-present, seething phenomenon in American ideology. That is what this book has demonstrated up to now, but it is not all, for I believe that the Velikovsky Affair, even in its totality, is merely part of a much bigger and older phenomenon, one that appeared hundreds of years ago or more, as we shall see. What I am saying is that the Velikovsky Affair of the past 50 years, however unique and excessive it seems, is indicative not only of its specific period but at the same time of a perennial, universal characteristic of our culture, and it is only in this larger sense that it can be fully understood. In this chapter, therefore, I am going to do several things. First, I will analyze the original phase of the Affair (the 1950's) in considerable depth and offer a precise model to

¹⁸⁰²William Warren Bartley, III, *Unfathomed Knowledge Unmeasured Wealth*, (LaSalle, Ill., 1990), p. 193.

explain its occurrence. That done, I will then step back from the present to the past half millennium or more and locate the Velikovsky Affair inside a stream of like events which spans many centuries, meaning that is typical of, (if not endemic to), our modern, rational, scientific culture throughout its span. I will try to show that it is precisely because our culture since the Renaissance thinks of itself as objective, empirical and reasonable, that something like the Velikovsky Affair has to keep occurring. Lastly I will argue that its origins can be traced back to the very beginnings of rationalism in classical Greece. With these essential backgrounds established, I will then be ready to look at the Affair in its most recent phases, from the 60's to the 90's, and to describe the state of catastrophism today. The subject, as you will see, deserves a book. I will try to do it justice in a chapter.

ONE: THE 1950'S

There can, of course, be many possible reasons for the original Affair, concurrent or separate, and we will explore a number of them. First, perhaps, is the naked desire to be known as the one who destroyed Velikovsky. Critic after critic has taken his best shots at Velikovsky in what I call the king-of-the-castle syndrome, which refers to the child's game in which the boy who pushes his rival off the mound and replaces him declares "I'm the King of the castle and you're a dirty rascal" (usually pronounced "rassle.") In this game, each temporary victor rascalizes his predecessor and enthrones himself, and this is just what has occurred with Velikovsky's critics. The original Harvard group praised itself but was castigated by Asimov, who was replaced and castigated by Sagan, who was replaced and castigated by Jastrow, who was replaced and castigated by Bauer. Each one had the same desire, which was to denounce his predecessors as unsuccessful and make himself known as the one who could do it right, Harlow-the-Velikovsky-Killer or Isaac-the-Velikovsky-Killer or Carl-the-Velikovsky-Killer. As this book has demonstrated, none of these critics succeeded. To be more precise, almost every one has made a fool of himself, (a point to be explored later), while Velikovsky has remained an unconquered summit, but the lure persists, and this attraction is, therefore, one possible reason.

Group Loyalty

Second might be the pressure exerted by the group to which Velikovsky's detractors belong. Two things determine the coherence of a gang—the loyalty of its members and the turf it controls. It has to exist *for* something and to be constituted *of* something, and it has to function on mutual blind support. No loyalty, no gang and no turf, no gang. Cornell and Cambridge historian of science Steven Shapin, in his excellent 1994 book, *A Social History of Truth*, explains how the gang system in science works. In the words of fellow Cambridge historian Keith Thomas, Shapin's "book has proved that all scientific knowledge rests on social conventions ensuring trust and truthfulness."¹⁸⁰³ That is to say, science is essentially a club calling for loyalty and adherence, and "in every field there are specialized sub-groups, sometimes composing no more than a dozen or twenty people who know each other," and it is these groups which define and establish knowledge, not on rigorous evaluation of every statement, but rather "on mutual trust."¹⁸⁰⁴ This makes science a kinship phenomenon, a social artifact, and within this unit concepts are rarely double-checked because faith in an individual counts for more than proof. Science is a gang in which support without question is the price of adherence.

¹⁸⁰³Thomas Keith, "Gentle Boyle," *London Review of Books*, (September 22, 1994), pp. 14-15.

¹⁸⁰⁴*Ibid.*

There was, of course, a very important and coherent sub-group in American science in 1950, the astronomical club. America had by then become the center of world astronomy, Harvard was the center of American astronomy and Harlow Shapley was the heart of Harvard astronomy. When Velikovsky launched what was felt to be an assault on this bastion, Shapley, therefore, is the one who leaped to its defense and his club fell dutifully in behind him, which explains why the nucleus of the first outraged, hysterical attack on Velikovsky contains the names of Harvardites and Shapleyites like Donald Menzel, Cecilia Payne-Gaposchkin, Rupert Wildt, Walter Adams, Otto Struve and I. Bernard Cohen. These people worked for Shapley and/or spoke at conferences which Shapley helped organize and/or appeared in anthologies which Shapley helped edit and/or obtained research grants from committees on which Shapley sat. Now that Shapin has led us to appreciate the very close ties within each scientific sub-group as a unit, we can better perceive that the paranoid attacks upon Velikovsky in the early 1950's were not a set of unconnected acts by individual scientists acting individually, but an organized response orchestrated by a gang whose members reacted to stimulus with knee-jerk loyalty. (We will call this club the American astronomical sub-group, or ASG). The gang structure inspires mutual trust, belief and support, but its corollaries sadly are distrust of the outsider, disbelief of the outsider and rejection of the outsider. That is to say, because what one member believed was accepted by the rest, therefore what one member disbelieved was scorned, hated, mocked and rejected by the rest. As even mainstream psychologist Norman Storer perceived 20 years ago,¹⁸⁰⁵ "once the scientific community was committed to opposing Velikovsky's assertion it became a sign of disloyalty for a scientist to support Velikovsky The forces of group loyalty took precedence." Ergo, virtually the same arguments against Velikovsky by many people, (most of them non-arguments), the same put-downs in almost the same words by different people (the book is easy to demolish, but one doesn't have the time), the same anger, the same sense of insult. This is more than coincidence or serendipity, it is the result of concerted manipulation. One must be careful when raising the specter of conspiracy, but the bulk of the evidence indicates that the original Velikovsky Affair was not a simple, natural uprising by insulted individuals. After Shapin we have to see it as a function of group dynamics, of mutual reinforcement and confirmation; for all indoctrinated zealots will manifest similar reactions in similar language under similar or common circumstances.

Turf Protection

Group loyalty is, therefore, a possible reason; but like the first, it is only a surface answer. There is more, and we get closer to it when we ask—Loyalty to what? Obviously to the group's substance. What was that? To its given role (actually self-given, but accepted by society as correct), which was to find the truth about the cosmos. Here we have the third cause, protection of turf, which derives directly from the second. Harvard astronomy felt without question that it and it alone had the correct tools, the correct methods, the correct knowledge accumulated in a tradition and the correctly accredited practitioners to perform its task. This was its correct territory, which it supremely deserved, not by bribe or gift or criminality, but merit. It alone had earned the right to poke about in the cosmos and to slowly but steadily unearth its secrets (as it knew it could) better than any rival institution. This was its turf, and outsiders or pretenders must be vigorously driven off, not for greed or pride, but *because the interlopers were undeserving* while the Harvardists were all-deserving and *would do the best for humanity*.

¹⁸⁰⁵Norman Storer, "The Sociological Context of the Velikovsky Controversy," *Scientists Confront Velikovsky*, (Ithaca, N.Y., 1977), p. 37, (henceforth, SCV).

Invariance

In my opinion, turf protection also is a possible reason, but it, too, is still only a surface answer. There is more, and we approach it nearer when we ask to what fundamentally is the group being loyal (*i.e.*, what is its real turf)? Analysis of the cosmos? I think not. That is still only touching the surface. In my opinion, when we look deeper, we will find that the real turf of the Harvard astronomy sub-group was not merely to investigate the cosmos, but to analyze it such that what was found would contribute to the shared group belief that the world is a huge, orderly, precise mechanism incapable of disorder today. Nothing less than that was the agenda of the club, and it is ultimately to this dogma of invariance that the loyalty of the group was directed. The prize that was at stake was not simply the freedom to question the heavens but the Truth which the ASG had discovered about the heavens, that they were unchanging. *That* was its turf, its sacred mission, and, when Velikovsky questioned it by presenting a rival vision, he was attacked as one would a poisonous, insidious, evil heretic.

To understand the depth and religiosity of the Harvard belief in heavenly invariance, we need turn only to the writings of the Harvard group at the time *Worlds in Collision* appeared. To Harlow Shapley, its doyen, the human mind certainly has the ability "to comprehend the nature of the universe, its laws and reasons,"¹⁸⁰⁶ and what it finds is that "Nature is reasonably benign,"¹⁸⁰⁷ that "planetary orbits are completely stable,"¹⁸⁰⁸ and that man can survive anything likely to come along from nature "if he remains moderately intelligent."¹⁸⁰⁹ We are being told that nature is orderly, that we can discern that order and that our survival depends only on our human abilities, not on nature, for nature is not excessively violent, as Shapley says in a book appropriately entitled *Flights from Chaos*. He tells us there that "Not many catastrophes happen to the Earth, except those of its own making," that everything is predictable and "We roll along, in fact, so smoothly and quietly in the vacuum . . . keeping so well isolated from major bodies . . . and so well insulated from the speeding gases . . . that nothing much happens cosmically."¹⁸¹⁰ To him the entire cosmos exhibits "quiet and predictable behavior"¹⁸¹¹ and all we need to understand that is the discoveries contributed by "the vigorous march of astronomical science"¹⁸¹² which will eventually explain everything. This smug humanistic attitude, common enough in its time, is echoed by his protégé, Cecilia Payne-Gaposchkin, who considers the solar system "a gigantic, rotating body, isolated in space" and the universe around it is "found to be predictable" and, therefore, "the world can be analyzed, and some phenomena controlled."¹⁸¹³ To these people, the world—or the solar system—is stable, predictable and safe and we will soon learn all its secrets, and there is no doubt that every member of the Harvard club believed it. It was a blissful and confident vision.

That, however, is not the full measure of the Harvard euphoria. To perceive its totality, we have to turn to the words of a second Shapley associate, the astronomer Forest Ray Moulton. In his article "The Orderly Universe," included in *A Treasury of Science* edited by Shapley and retained through four editions, we can find the all-dressed, fully-orchestrated version of the dogma. To better appreciate its wishful one-sidedness, I will first introduce as a contrast some very different views of the cosmos put forth by British

¹⁸⁰⁶Harlow Shapley, *The Stars*, (Chicago, 1927) p. 12, (henceforth, *The Stars*).

¹⁸⁰⁷Harlow Shapley, *The New Treasury of Science*, 3 ed., (New York, 1958) p. 2, (henceforth, *New Treasury*).

¹⁸⁰⁸*Ibid*, p. 153.

¹⁸⁰⁹*Ibid*, p. 154.

¹⁸¹⁰Harlow Shapley, *Flight from Chaos*, (New York, 1930), pp. 56-57, (henceforth, *Flight from Chaos*).

¹⁸¹¹*Ibid*, p. 57.

¹⁸¹²Shapley, 1966, ix.

¹⁸¹³Cecilia Payne-Gaposchkin, *Introduction to Astronomy*, (New York, 1954), pp. 1-2.

astronomer Fred Hoyle who, in 1971, wrote that "space is not the dead place it was supposed to be a few years ago." To Hoyle, there is "distinctive evidence"¹⁸¹⁴ that huge explosions occur not merely in intergalactic regions but in our galaxy too, that cosmic ray energy is as abundant and powerful as all the nuclear energy, that "violent events occur" in nearby regions¹⁸¹⁵ and that outer space contains "a veritable multitude of intense centers of activity."¹⁸¹⁶

In contrast, here is what Moulton wrote only 20 years before (and what all the members of the Harvard group believed): that "the most important, the supreme discovery of science . . . [has been] the orderliness of the universe."¹⁸¹⁷ Heavenly motion is a perfection which "the running of no clock ever approached in precision" and this "majestic order prevails universally."¹⁸¹⁸ Every moving body in space falls under this scheme – "The theory of the motion of the moon is so perfect that its position can be computed for every instant," gravity acts "With extraordinary exactness" and even comets were placed by Brahe into "the orderly domains of celestial bodies," permitting scientists to compute the path of Halley's Comet "with perfect certainty."¹⁸¹⁹ So perfect is this knowledge, being founded on "an infallible basis," that "Mere words cannot do justice to it." "If we are entitled to conclude that we understand anything whatever,"¹⁸²⁰ it is the stable and precise motion of the heavenly bodies which astronomical science has perceived – "No machine ever ran with such accuracy."¹⁸²¹

To read this sort of thing brings us much closer to the deepest cause of the Velikovsky Affair, for Moulton, with the approval of Shapley, encapsulates the view which was current in science in 1950 and presents it to the public in Shapley's own compendium *as the deepest core of astronomical truth*—that the universe is totally and eternally orderly and measurable and predictable, and, of course, knowable. Everything is gradual and regular, there are no surprises and nothing will not be deciphered in time. I will call this the Perfect Cosmic Principle, (hereafter, the PCP), with apologies to Bondi and Gold's Perfect Cosmological Principle. The PCP is a vision of incorruptible evenness, regularity and precision. We might almost call it a religion, for it is what the American astronomical sub-group believed to be wholly true as an article of faith. To be more precise, Moulton is telling us that the PCP was *why* astronomical science had come into existence, and that it was the joy and honor of the American astronomical sub-group (the ASG) to offer this treasure to the world, to reveal it in all its unquestionable splendor. The apparent miraculousness of the ASG was simply its ability to perceive the real miracle of the ordered universe, a great and holy Truth which it as middleman for Nature presented to the world.

We can, therefore, quite legitimately view the members of the ASG as similar to the adherents of a religion, who never question their basic principles but spend their time analyzing and criticizing everything else in terms of their unquestioned principles, as all committed initiates of any religion do. The PCP gives them their right to exist and, therefore, they do not exist to find the truth about it because, of course, *that has already been found*. They exist only to prove it ever more and more with new data. Such people do not dig new holes, as Edward de Bono pointed out in *Lateral Thinking*; they only dig existing holes deeper. In this sense the ASG behaved precisely like a religion, and its laboratories functioned like the workrooms of a medieval monastery where the tonsured monks, (oops, accredited Harvardites), convinced of their Truth, fitted every datum into it like a bed of Procrustes. The hosannas and encomia of Moulton, his euphoric overstatements about the grand accomplishment of astronomical science, were, therefore, merely a bland

¹⁸¹⁴Sir Fred Hoyle, *The New Face of Science*, (New York, 1971), p. 81.

¹⁸¹⁵*Ibid.*, p. 82.

¹⁸¹⁶*Ibid.*, p. 96.

¹⁸¹⁷Forest Ray Moulton, "The Orderly Universe" – in *New Treasury*, *op. cit.* p. 62.

¹⁸¹⁸*Ibid.*, p. 63.

¹⁸¹⁹*Ibid.*, p. 69.

¹⁸²⁰*Ibid.*, p. 64.

¹⁸²¹*Ibid.*

summation of the ASG creed, and it is finally to this Faith that the loyalty of the ASG was most wholeheartedly directed. This, in my opinion, is the real turf which that loyalty group had banded together to uphold, advance and protect, and that is ultimately how science functions—science is what the major figures in the loyalty group allow the rest to believe.

We can now construct a model for the Velikovsky Affair. When Velikovsky appeared to question the group's faith in the traditional history of the Solar System and the geological history of the Earth and even the hegemony of Newtonian gravitation, the reaction of the ASG (fury, panic, astonishment, insult, destructiveness) had nothing to do with whether or not Velikovsky was a qualified astronomer or did or did not supply precise numbers to his predictions. It had perhaps a little to do with defense of the ASG's role as astronomers, but it had everything to do with defense of the PCP as a religious dogma, as the cherished and envied possession and mission of the ASG. When Velikovsky attacked that, (as they knew in their hearts he did), it was a thrust at the very life of the group, and its reaction, precisely like the medieval Church or Stalin's Russia or Nazi Germany, (or any rabid individual whose self-image has been perilously threatened), was animal-like. The threatened organism, whose deepest impulses are self-preservative and territorial, responded emotionally, instinctively, viscerally, with its claws extended and its lips drawn back from its teeth, and what followed was the Velikovsky Affair, which seems unexplainable if analyzed within a rational structure, but becomes inevitable if located as a tribal phenomenon, as unthinking, outraged group behavior. In my childhood the Canadian pennies had etched on them the many different titles of the British monarch, which included "ind. imp.," (in English, Emperor of India) and "fid. def." (in English, defender of the faith), and that I think well characterizes Shapley and his ASG. He was the emperor of American astronomy and defender of the PCP, and the first stage of the Velikovsky Affair, (which unfortunately succeeded in branding him a crank for two decades), was a tribal plot, a coordinated campaign of vituperation and character assassination functioning purely on non-reasonable group psychology. If as Shapin argues science is what one's trusted friends believe, then what one's friends reject is fully worthy of being destroyed.

The Postwar World

There is one more important element to be considered, and that is the period in which *Worlds in Collision* appeared. As Vorhees correctly says:

" . . . the timing of its initial release played a crucial role in his condemnation by the scholarly community at large. The year was 1950."¹⁸²²

Vorhees is referring to a convergence of tense political events that would have made America especially edgy at that time—a Russian A-bomb, China overrun by Communists and the demon of internal treason raised by McCarthyism. In addition, the realm of ideas was beset by the pseudoscience of Lysenko, Adamski and Hubbard. There was, therefore, in American science a very great distrust of best-selling, seemingly scientific books, especially those that cast doubt on the certainties of mainstream science; for as R. E. McAulay wrote in his sociological analysis of Shapley's actions, science must demonstrate that it "is indispensable to political and governmental concerns" and this cannot be maintained when there are "competing perspectives—especially those which project a somewhat capricious and at times mysteriously changing universe beyond human control."¹⁸²³ That is to say, Velikovsky threatened not only the ethos of American science but its status, its value to society, *i.e.*, its funding. The Affair could then be interpreted as simple self-defense.

¹⁸²²Duane Vorhees, "Velikovsky in America," *AEON*, Vol. 3, No. 4, (1993), p. 48.

¹⁸²³*Ibid.*, pp. 57-58.

This is true as far as it goes, but it gains deeper relevance when seen in the light of the thesis I have been developing. When we consider the history of the world in the 20 years before 1950, we see above all else a battle between two opposing political values, democracy and fascism, with, for example, England, the United States and France on one side, squared off against Germany, Italy, Spain and Japan. It was a fight for world allegiance and much was pegged to the outcome, for *the winner would become the country which had the correct ideas*. We might see this today as silly, for we do not believe that military victory is tied to ideological superiority, but not then. Each side, if it were to contend, had to show its preeminence in all areas of life – better artists, better scientists, better athletes, as if world records or Nobel Prizes meant that that country's values were best. Perhaps what lies behind this conception is the unspoken, primitive, almost pre-literate feeling that heaven will endow its chosen nation with the best runners and the best philosophers, who will be produced naturally by that nation because it has the correct political beliefs. However ridiculous that may appear, it did seem to infest Nazi propaganda in the 1930's, not to mention the ideological war between Russia and the U.S. in the 1950's.

Now consider 1940. The Germans flooded over Europe and the Japanese over East Asia, but, more specifically, the fascist attacks came most memorably *from the sky*. The Japanese dive-bombers in China and the German dive-bombers in Europe brought destruction screaming straight down from the heavens, and then, in an imitation of the mythical feats of a Marduk or Venus, waves of cataclysmic destruction in the form of armies and tanks and bayonets coursed across the land. It was pure Velikovsky, and, because at first it succeeded so rapidly, fascism seemed to be the Chosen Path. Towards the end of the war, however, the situation had been reversed, for the allies had gained almost total air control over Europe and had reduced the Japanese mainly to *Kamikaze* suicide attacks from the air, implying that democracy and not fascism was God's True Way. Then, as if air power were a giant definitive world poker game coming to a close in 1945, the U.S. played its ace in the hole, its A-bomb, and took the pot. Game over and the biggest gun in town had won.

This final Allied victory in the air meant more than political victory, it meant that democracy had proved that its ideas were the right ones and, with fascism buried like a dragon deep in the ground, it could now relax and build a new democratic world to imitate the heavens, a world without sudden violent fascistic change. The earth was now safe and *above all, the sky was safe*. The U.S. had seen to it that no more death would rain down from heaven. If the experience of terrifying heaven-induced catastrophes is part of our collective memory, and if we unconsciously fear them in the form of death from the sky, then the U.S. in 1950 was like Jove on Olympus, possessing the biggest weapon (the thunderbolt) and with it keeping the bickering rabble of lesser gods (*i.e.*, nations) in order, or like the God of the Old Testament who "with a strong hand and outstretched arm" suppressed evil nations and brought stability to the Earth. There was a quasi-religious feeling that the world had been cleansed of manmade catastrophe.

Then, as Vorhees says, things rapidly changed for the worse as we approached 1950—suddenly, more than one country had the Bomb. Europe, instead of gaining peace, had turned into two armed camps; eastern Europe and much of Asia had fled to a rival ideology and become Communist; the Korean War was approaching in Asia and at home, America felt that it had become infested with Red spies. Certainly, these political events were disquieting, but Russia could be countered by nuclear deterrents, early-warning systems, protective political alliances, negotiation and subversion, and it was felt that in these ways the horror of nuclear war, of total destruction, could be kept at bay. All of these defenses were initiated, but then, at just that point, when equilibrium or even stalemate seemed to have been achieved, along comes Velikovsky, not a clean-cut American, not "one of us," (the victors), but a European and a Jew with a thick guttural accent, presenting a hodge-podge of permissible scientific ideas mixed with impermissible myth and religion, and threatening to resurrect the entire pre-1800 pre-scientific past of religion and folklore which America had just destroyed, of fascistic suddenness and violence, even after Shapley had gloated that

"As rational practitioners . . . we deplore superstition—the last stronghold of the irrational. But, thanks to man's reasoning, never before has hampering superstition been in retreat on so wide a front."¹⁸²⁴

Velikovsky seemed to be defying not only Newton and Democracy and Truth, (and Harvard), but, even worse, to be saying that the sky was not safe and, worst of all, that the danger it posed was not in any way stoppable by us. After all, how does one deter a meteor or set up alliances against a comet or subvert a fireball or negotiate with an asteroid?

Therefore, Velikovsky had to be denied and destroyed before his lie could pollute the clean, new, shining world which U.S. democracy had created. In this sense, the Affair of the early 1950's was not merely formalized American science versus the upsetting, unformalized grab-bag ideas of a maverick outsider, nor even the 19th century classical myth of science versus something very medieval, but American-Science-as-Uniformitarian-Truth versus danger from the sky, and, with Russia looming and McCarthy seeing Communists under every bed, every backslider from this One Dogma became a heretic and a traitor. Under McCarthyism, people were fired for their politics, books were blackballed, free expression was blocked and a climate of tolerant listening did not exist. There was in 1950 a Cold War for the Truth, and the weapons were innuendo, influence, pressure, misrepresentation and character destruction. When therefore Velikovsky preached a universe against which not even U.S. democracy (God's own choice) could have any effect, the Affair was not merely understandable but inevitable. He had defied America in its very essence, its (hard-won) role as Jupiterian regulator of scientific knowledge and earthly behavior, as the bringer and keeper of the most lasting peace, and it, therefore, became them or him, all or nothing. After that, catastrophist psychology explains the specific events best. If he had maligned not only America-the-good, but its science, its rod of divine peace and justice, he deserved to be eliminated.

Interestingly, confirmation of American science's gang behavior towards Velikovsky is to be found in *Stargazers and Gravediggers*, which Velikovsky pointedly subtitled *Memoirs to Worlds in Collision*. In the first segment of File II, he describes being called to the office of George Brett, owner and publisher of the powerful MacMillan Company, in May of 1950. It was Brett's difficult task to tell Velikovsky that MacMillan was going to give up its best-selling book to a rival publisher:

"Tremendous pressure is being exerted against our company by a group of scientists . . . [The new publisher] has no textbook department and cannot be hurt."¹⁸²⁵

In response to Velikovsky's surprise and dismay, Brett tried to explain:

"Here Mr. Brett picked up a pencil and drew some circles. 'Academic circles are not isolated groups' And he drew larger circles' In this way the academic pressure may become widespread."¹⁸²⁶

¹⁸²⁴Shapley, *The Stars*, *op. cit.*, p. 157.

¹⁸²⁵Immanuel Velikovsky, *Stargazers & Gravediggers*, (New York, 1983), p. 131, (henceforth, *Stargazers*).

¹⁸²⁶*Ibid.*, p. 132.

During the course of the vigorous debate that followed, Brett resorted to the image once more.

"Brett . . . was definitely committed to his decision to free his house of a book that was arousing wrath among the powerful of the textbook world, and he again began to draw a pattern of circles to show me how the scientific groups are interlocked, how they are centered, and how they can damage a publishing house."¹⁸²⁷

Brett was not a theorist but a businessman who understood the pressures of commerce, the give-and-take of reputation, and in his experience American science was constituted of interlocked, overlapping, mutually supportive spheres of influence. He saw very clearly that the antagonists of Velikovsky's book were not independent individuals but members of interrelated gangs acting in unison, a situation which in his innocence he portrayed as patterns of circles. One could hardly ask for a better naive symbol to represent the loyalty groups of American science and their dense, close-minded, inward-looking connectedness. Given this situation, the Velikovsky Affair was a foregone conclusion.

TWO: THE HISTORICAL BACKGROUND

Up to here, I have restricted my discussion to what we may call the original Velikovsky Affair, c. 1945 to 1960. I had said that in its details it is a unique and ugly phenomenon in American science, but I had also said that at the same time it is part of a larger, trans-American phenomenon which extends over a span of hundreds of years, *i.e.*, from the Renaissance. In this section, I will explore the nature of that ancestry to show that the very deepest issue which characterizes the Velikovsky Affair of our century has remained at the forefront of our culture's preoccupations for some 24 centuries, not merely six, and that it continues unfortunately to this day.

Darwin

To appreciate this, we have to step back 100 years before the Velikovsky Affair and turn to the story of Charles Darwin and the reason why he wrote *The Origin of Species*. I said in the opening chapter of this book that the Velikovsky Affair is the darkest blot in the history of ideas since Galileo and Bruno four centuries ago. This may seem like hyperbole, for it leaves out the Darwin Affair, which in its day was just as hysterical and one-sided and filled with a desire to destroy. That is true, but there are also some significant differences in the sagas of Darwin and Velikovsky, for Darwin in three decades went from pariah to prophet, from outcast to hero accepted by most of society, professional and non-professional, whereas, as this book has shown, Velikovsky is still hated, resented and loathed half a century after his ideas first appeared. Members of the various science gangs still refuse to look honestly at his ideas or to permit their appearance in learned journals and at conferences or to have them debated by qualified specialists on both sides, or even to carry advertisements for them in club-controlled publications. The problem, therefore, is this—even though both men mounted frontal attacks on the club creeds of their times, Darwin has become the center of club-

¹⁸²⁷*Ibid.*, p. 134.

controlled science while catastrophism is still met with unabated anger, dishonesty and unprofessionalism. That has to be explained.

To discover the reason, we will have to take a much closer look at Darwin in relation to Velikovsky than usual. In my opinion, he did not write *The Origin of Species* primarily to offer his theory of evolution. What I mean by this surprising assertion is that that theory was merely the specific proof for a much larger hypothesis whose outline we can glimpse behind every word in the book. The clues are most abundant and direct in the concluding chapter, where one would expect the author to finally say what is most important to him. It turns out that this is just what Darwin does, and he states the true creed there three times to make sure the reader does not miss it. Amidst all the specific references to evolution, amidst the generalized recapitulation of his themes, amidst the modest assertions and mild demurrers, we find this:

"There are, however, some who still think that species have suddenly given birth . . . but, as I have attempted to show, weighty evidence can be opposed to the admission of great and abrupt modifications."¹⁸²⁸

This becomes the underlying purpose of his entire theory, to show that "little advantage is gained by believing that new forms are suddenly developed in an inexplicable manner."¹⁸²⁹ We suddenly see that the book is an anti-catastrophist polemic whose major purpose is to convince the reader, as he states only a few pages later, that "the most important of all causes of organic change is one which is almost independent of altered and *perhaps suddenly altered* physical conditions."¹⁸³⁰ (Emphasis added) This, in my opinion, is the true bottom line, the real purpose of Darwin's book, and the peak of this crescendo of uniformitarian propaganda occurs in the penultimate paragraph, where it is stated that, if one accepts the premise of evolution, then

". . . we may feel certain that the ordinary succession by generation has never once been broken," [i.e., the world has always evolved slowly and peacefully], "and that no cataclysm has desolated the whole world."¹⁸³¹

This is Darwin's hidden agenda, as it is the agenda of the entire 19th century, and in particular of the secular British mentality of Hutton, Playfair and Lyell and their fellow archaeologists and cosmologists. It is a total denial of the possibility of catastrophism. That is why Darwin was accepted so quickly by his scientific community after the original furor. It was not because he rejected the Bible and special creation, not because he upheld reason over folklore or common sense over authority, but *because he told us the world was essentially safe*. That was the deepest layer of his message, the one which finally won him general support. If evolution is true, it was felt, then catastrophism isn't, wasn't and cannot be.

Darwin is, therefore, able to conclude the paragraph with an astonishingly romantic, idealistic and unscientific sentiment which he nevertheless tags onto his theory as its scientific culmination:

¹⁸²⁸Charles Darwin, "Conclusion," *Origin of Species*. In *Great Essays in Science*, Martin Gardner ed., (New York, 1957), p. 11.

¹⁸²⁹*Ibid.*, p. 7.

¹⁸³⁰*Ibid.*, p. 11.

¹⁸³¹*Ibid.*, p. 13.

"Hence we may look forward with some confidence to a secure future of great length."¹⁸³²

I quote from the second edition, after Darwin had had time to reconsider what he had written earlier. Certain things had been changed from 1859 to 1861, but this thought remained as the cornerstone of his thinking. This in my opinion is the true attraction of Darwin and why he was soon accepted by most of the general population—he offered a world view which was only reassuring and satisfying, and he felt so confident of it that he went on to incorporate that romantic dream into a vision which governed the future of all organic existence.

"And as natural selection works solely by and for the good of each being, all corporal and mental endowments will tend to progress toward perfection."¹⁸³³

By this point Darwin's hallucinogenic raptures about the utter providentiality of Nature should have boggled any well-balanced reader's mind, but few have ever noticed (much less rejected) these teleological, Aristotelian, anthropocentric fantasies, perhaps because they agree with the delirious Darwin, who, carried away by the beauty of it all, concludes with the smug observation

"There is grandeur in this view of life . . . whilst this planet has gone cycling on according to the fixed law of gravity."¹⁸³⁴

There you have it all, the entire Victorian fairy tale—the planet cycles regularly forever, life goes on regularly forever and it all moves "toward perfection." It is a dream-world of wishful fantasy projected onto nature. One wonders how anyone could have taken this myth seriously, for its origins lie much more in Greek and Scholastic philosophy (and in British chauvinism) than in reason, but of course we all know the place Darwin holds in modern science.

To understand the theory's true source, let us consider a trifle more objectively than Shapley, Asimov and Sagan what Darwin has done. In a word, he has presented to the reader a vision of life which combines perfect human progress and a perfectly stable physical world in one comforting English picture. This has to lead us to suspect that Darwinian evolution is an Englishman's version of paradise, for natural selection is democratic, not autocratic, treating everyone alike and rewarding only merit, like the British parliament, and, even more, the British public school system. It works in a reasonable, not capricious or tyrannical manner, like English life at its rustic best, as opposed to the horrid excesses of France or Germany, its essence is progress (a liberal, common-sensical, empiricist British desire) and its aim is human perfection as exemplified by the English Victorian gentleman of good birth and education. It will go forever forward and upward, like the daydream of a properly-indoctrinated British public school boy, to finally produce a human model in which everyone will be like Darwin and his Oxford classmates, (*i.e.*, perfect). When we read this fantasy, this wishful dream, our conclusion has to be that Darwin is not a naturalist or a scientist but a romanticist, an enlightened hopeful Englishman who seeks in pure benevolence to extend the benefits of British culture to the benighted rest of the world. Newton the Oxbridge gentleman had showed 200 years earlier that the

¹⁸³²*Ibid.*

¹⁸³³*Ibid.*, p. 16.

¹⁸³⁴*Ibid.*

physical universe is ultimately stable and kindly, and Darwin, the Oxbridge gentleman, now showed that human life in the long run is equally steady and goodly. That is what Darwin wants for the whole world (what benevolent 19th century English gentleman wouldn't?) and he therefore tries his best to prove that that is what the world is like, that the forces of Life no less than the forces of the Cosmos will surely guide the world to perfect (British) peace and happiness.

Darwin in this sense is an exemplar of the 19th century view of life, because essentially almost all scientific theory since 1800 forbade disorder in nature. Laplace claimed to prove it about the sky, Darwin about organic life and Lyell about geology. In all of existence it was believed there is a discernible pattern of order and regularity. There are no catastrophes, no sudden appearances of new cosmic objects or new living organisms or new large earthly features. Change is very slow and steady, and above that it is progressive, it ascends to stability. The cosmos, the Earth and life move steadily toward less and less violent states in a vision of placidity, of eternally improving beneficence, that became the major scientific myth of the 19th century. The improved research of catastrophists as well as of traditional scientists since 1950, however, (as we shall see later), has shown ever-increasingly that this view derives not from the data but from human desire, that it comes not from the world but from us, which has to make us suspect that it is projected by us onto the world because that is desperately how we want to see it. When we contend that the cosmos was hot balls of fire and explosion but now it is settling down to a perfect clockwork, that life was reptiles and dinosaurs and giant animals but now it is settling down, that the earth experienced impacts and ice ages and vast extinctions but now it is settling down, we can see that this is merely a myth which we have created and which we then blindly extend to civilization (people were savage and wild and primitive but now they are settling down) or to religious values (societies were pantheistic, sacrificial and polytheistic, but now the true religion is perceived), etc. In every way, we say, and every day, every area of existence is settling down and getting better. This is obviously a fairy tale, (especially after Hitler, Stalin, Cambodia and Rwanda), but it was the ethos of the 19th century and it does continue unfortunately to exercise a hold over the mind of the 20th century. It is the unspoken religion of our time, and Darwin is one of its pillars.

In this light, the contrast between Darwin and Velikovsky could hardly be more stark. Velikovsky set forth an almost totally opposite view. He not only said that speciation can be caused by mutation, (attacking Darwin's surface agenda), but (getting at Darwin's deeper agenda) that the world in which mutations appear is unpredictable and sometimes violent and that these events are unstoppable and occur independent of consideration for humankind. The world's history (and, therefore, future) are governed by forces devoid of moral purpose, whose behavior is determined by brute chance and whose future acts do not necessarily lead to goodness or progress or stability or order (or the British gentleman's way of life), and could lie totally outside our control. It is wholly unlike an Oxbridge gentleman's country estate where the turbulence of nature is kept in check by well-inclined gardeners and the landscape is not left to chance but designed by aesthetes and the products of the seasons arrive in stately succession without too much violence or interruption. That may have been the model for most of British 19th century science and politics, but Velikovsky says the world can sometimes be chaotic and random and senseless to us, that not only can we not know what its long-term purpose is, (if any), but we cannot even foresee what will happen to it in the short run.

Imagine then the gauntlet Velikovsky threw down in *Worlds in Collision*, how much of a slap in the face it was to American astronomy enrobed in its self-given but publicly-seconded role as understander, explainer and master of nature, discovering its secrets, discerning its lawfulness, perceiving that it is orderly, finding out how to control it when it becomes (only momentarily) disorderly and even learning how to manipulate it more and more. To perceive what Velikovsky set up in contrast, we will turn to the last chapter of *Worlds in Collision*, as we did with Darwin, for it is there that we should expect to find Velikovsky's grand view of the universe, which stands behind all of his argumentation, as we did Darwin's.

Velikovsky's chapter heading is a quote from the *Visuddhi-Magga* which at once sets the tone—"This world will be destroyed; also the mighty ocean will dry up; and this broad earth will be burnt up."¹⁸³⁵ After this, Velikovsky then states his theme bluntly:

"The solar system is not a structure that has remained unchanged for billions of years Catastrophes have repeatedly reduced civilization on this earth to ruins [and] . . . if events of this kind happened in the past, they may happen again in the future, with perhaps a different—fatal—result."¹⁸³⁶

It is a statement with no holds barred, and it culminates in the firm assertion that "Cosmic collisions are not divergent phenomena . . . they are more in the nature of occurrences implicit in the dynamics of the universe."¹⁸³⁷ They are what astronomy would call "convergent phenomena,"¹⁸³⁸ events that arise explosively out of multiple causes in an explainable and direct manner. That is *his* vision of the cosmos.

Moving to the end, we find that the very last paragraphs of Velikovsky's chapter, like Darwin's, contain the most generalized formulations of the author's vision. In Darwin, as we just saw, these had been idyllic, unscientific wish-projections, but in Velikovsky they are a set of grim dire forebodings quoted from classical sources:

". . . seas, and lands, and sky . . . one single day shall hurl to ruin; and massive form and fabric of the world held up for many years, shall fall headlong. (Lucretius)

". . . the whole firmament shall fall on the divine earth and on the sea; and then shall flow a ceaseless cataract of raging fire . . . and the firmament of heaven and the stars and creation itself it shall cast into one molten mass and clean dissolve. (The Sybilline Oracles)

"A single day will see the burial of all mankind . . . all will descend into one abyss, will be overthrown in one hour." (Seneca)¹⁸³⁹

That is how Velikovsky ends his book, (aside from the 'Epilogue'), and a more thundering contrast with Darwin and 19th century, fairy tale science could hardly be imagined. This is the magnitude of Velikovsky's new science, that the cosmos and our solar system were not and are not forever stable, that there have been destructive events in the past and there may be again and we cannot predict them or stop them, which is to say that everything that traditional astronomy has built up (both its concepts and its tools) is pitifully weak and vulnerable. If Velikovsky is right, we do not control or understand the Earth nor our future on it, and science cannot see this or save us from it and consequently the whole myth of Science the Savior is helpless

¹⁸³⁵Immanuel Velikovsky, *Worlds in Collision*, (New York, 1950), p. 373, (henceforth, *WIC*).

¹⁸³⁶*Ibid.*

¹⁸³⁷*Ibid.*, pp. 375-376.

¹⁸³⁸*Ibid.*, p. 376.

¹⁸³⁹*Ibid.*

beside this greater truth. This is the key to the origin and continuation of the Velikovsky Affair, and why he is still hated – we cannot bear to know what Velikovsky urges and we flee in panic to its opposite.

Lyell

Now to go deeper. I had said that, in order to understand Velikovsky, we would have to step back a hundred years to Darwin. Now I say that, to understand Darwin, we will have to take a further step backward to the previous hundred years before him and survey the history of the scientific debates in geology, paleontology and astronomy from 1770 to 1870, especially in England. What we shall find is that the same irresistible antagonism to unruliness in nature that we came across in the Velikovsky and Darwin affairs also determined the outcome of the major scientific debates in the century before Darwin's time, and always in favor of order, purpose and restraint.

The first half of this period was marked by an intense antagonism between the neptunists, who attributed most of the Earth's rocks and sediments to the precipitative aftereffects of an immense flood, and the vulcanists and plutonists who saw the cause as volcanic action arising from great heat coming from deep in the Earth. The neptunists, who included Buffon, Leibnitz, Halley, Whiston, Cuvier and especially the very influential A.G. Werner, developed a model of global floods and immense rare catastrophes followed by successive rock and mineral deposition out of the receding world ocean. The vulcanists, whose ranks (at different times) included Demarest, de Dolomieu, Hutton, Playfair, Conybeare, Buckland and Sedgwick, argued that slow uplift from frequent volcanic activity of the sort seen now and subsequent slow erosion and denudation were adequate to account for all of the Earth's present geology. The battle took perhaps 40 years, and by the 1820's Wernerian diluvialism had been largely defeated in England and on the continent.

What we must perceive is that the point at issue was not merely an opposition between rival mechanisms for producing today's Earth, but between our old friends, violence versus placidity. That is to say, even though it is said that both views were "teleological and anthropocentric" and that both "involved substantial generalization from limited observations,"¹⁸⁴⁰ they were fundamentally different in vision. The neptunists were seen as upholders of the Mosaic chronology, catastrophists who argued that our world of high mountains and deep valleys and ocean basins was produced by the rapid and violent action of "immensely powerful forces at some time in the past, undetected in the modern world,"¹⁸⁴¹ forces which produced "exceptionally violent change in former times."¹⁸⁴² In contrast, the vulcanists insisted that the slow but continuous process of uplift-erosion-consolidation-denudation was enough to produce the present "condition of dynamic equilibrium"¹⁸⁴³ without recourse to catastrophe. Hutton, in particular, objected to "the widely accepted Mosaic chronology." He believed that the moment we would become "free of the mental shackles of rigid adherence to biblical doctrines" we would be enabled to perceive that "the operations of nature are equable and steady."¹⁸⁴⁴ When his mechanistic view was finally accepted in the 1830's, our notion of what our world is like shifted "from a planet in decay to one which was essentially stable and ordered."¹⁸⁴⁵ It is the same story that we had seen with Darwin and Velikovsky.

The battle went back and forth for about 30 years, but it is significant that the sides were not always rigidly drawn. The neptunists, for example, who were accused of being catastrophists, said that the various different rocks and metals precipitated slowly, not quickly, out of the universal ocean, while vulcanists like

¹⁸⁴⁰Andrew Hallam, *Great Geological Controversies*, 2 ed. (Oxford, England, 1986), pp. 1-2.

¹⁸⁴¹*Ibid.*, p. 30.

¹⁸⁴²*Ibid.*, p. 31.

¹⁸⁴³*Ibid.*

¹⁸⁴⁴*Ibid.*

¹⁸⁴⁵*Ibid.*, p. 2.

Hutton, who believed in slow erosion, also believed in rapid uplift. What were more important were the issues of cause (natural or divine) and process (passive or active, fast or slow). Behind these lay the question of the Earth's history – was it cyclical and repetitive or filled with sudden unique devastations. Hutton, who felt that the present world was built upon the ruins of former worlds, nevertheless supported "a cyclic steady-state model" in that he believed the processes of change recurred over large periods of time and, therefore, from this long point of view, one could not separate the present from the past. Earth history was an overall equilibrium achieved by "a constant interplay of forces."¹⁸⁴⁶ In contrast, the catastrophist Cuvier said that Recent was not like Ancient, that geology and especially paleontology gave evidence of sudden cataclysms, "frightful events" produced by "sudden and violent causes,"¹⁸⁴⁷ and, therefore, there were different processes in the past. To Cuvier "the march of nature is changed" for the work of nature indicates that "none of the agents that she now employs were sufficient" for the creation of the large, deformed geological structures which she exhibits.¹⁸⁴⁸

A third alternative, of course, was possible, as elucidated by Elie de Beaumont – "Long periods of quiescence were interrupted suddenly by relatively short-lasting upheavals."¹⁸⁴⁹ Most of the major figures located themselves somewhere among a combination drawn from these three choices until the appearance of Lyell, "the high priest of a new doctrine, uniformitarianism,"¹⁸⁵⁰ who seemed to resolve the matter once and for all. His ideas were formed in the 1820's and expressed in his three-volume *Principles of Geology*, 1830-1833. The book, we are told, is *not* a scientific report as most people mistakenly think, but "a treatise devoted to the presentation and defense of a new system." Lyell will have none of catastrophism or alternativism, but only "a steady-state, uniformitarian system"¹⁸⁵¹ at all times. He upholds "the permanency of the laws of nature" as if they led only to his view, attacks "Mosaic and Catastrophist approaches to geology," rejects any sort of "paroxysmal convulsions"¹⁸⁵² and declares as his creed "that no causes whatever have . . . acted but those now acting and that they never acted with different degrees of energy."¹⁸⁵³ Everything, he insists, has always been "slow and gradual," nothing ever occurred with greater intensity in the past and all rare events, if seen over a sufficiently long time, are merely a "balance about a mean."¹⁸⁵⁴ Lyell was a barrister, an orator and tactician, and he engaged all the leading geologists in debate. Within a few years most of them had capitulated (Sedgwick recanted in public) and uniformitarianism carried the day in England. Lyell had achieved the "change in public opinion" which he wanted¹⁸⁵⁵ and his "extreme uniformitarianism,"¹⁸⁵⁶ cleansed of any taint of the Bible or divine intervention or unique upheavals, become the new dogma in every field of science.

What must be understood is that this was much more a religious battle than a scientific one. Both the vulcanists and the neptunists could be considered deists (God created the universe) and theists (there is a divine purpose to the universe), but debated whether that purpose worked peacefully or catastrophically. Hutton may have spoken for both sides when he expressed "his belief of a grand design or purpose in nature," but there was sharp disagreement as to how this design functioned, particularly with respect to mankind. The neptunists followed the Biblical code that humankind will be held responsible for its sins by God, Who will intervene and inflict punishment when it is deserved, (*i.e.*, there will be catastrophes), whereas

¹⁸⁴⁶*Ibid.*, p. 25.

¹⁸⁴⁷*Ibid.*, p. 30.

¹⁸⁴⁸*Ibid.*, p. 38.

¹⁸⁴⁹*Ibid.*, p. 40.

¹⁸⁵⁰*Ibid.*

¹⁸⁵¹*Ibid.*, p. 49.

¹⁸⁵²*Ibid.*, p. 47.

¹⁸⁵³*Ibid.*, p. 49.

¹⁸⁵⁴*Ibid.*, p. 50.

¹⁸⁵⁵*Ibid.*, p. 52.

¹⁸⁵⁶*Ibid.*, p. 54.

the vulcanists held that the purpose of nature was always benevolent, (*i.e.*, there is only gradualism), and it is here, in their preference for a benign providence, that the 19th century British geologists display their religious (and very uniformitarian) bias most blatantly. As Hutton wrote:

"We perceive a fabric, erected in wisdom, to obtain a purpose This purpose is to maintain the earth as a 'habitable globe.' We 'are led to acknowledge an order, not unworthy of Divine wisdom.'"¹⁸⁵⁷

He saw the Earth as "in a state of perfection" and declared, "Therefore, there is no occasion for having recourse to any . . . destructive accident in nature . . . in explaining that which actually appears."¹⁸⁵⁸ The world, he said, is our nurse and shelter, and "the end of its intention" is "the preserving of animal life."¹⁸⁵⁹ Buckland wrote that geology shows "wise foresight and benevolent intention" and that its blessings are "proofs of the most exalted attitude of the Creator."¹⁸⁶⁰ Above all of these comforting concepts was that of the perfection of mankind, sitting at the top of the Great Scale of Being. "The intellect of man and his spiritual and moral nature," said Lyell, "are the highest marks of creative power known to us in the universe."¹⁸⁶¹ This masturbatory ecstasy, however, has nothing to do with science. The Hutton-Lyell-Darwin amalgam is a religious vision of cosmic benevolence, with nothing fearful having happened in the past and, therefore, nothing fearful to be expected in the future. All is slow and peaceful and all is for the best and always was, (shades of Dr. Pangloss).

The Triple Terror

It is hard to believe that this theological fairy tale became the foundation of science in the 19th century, but catastrophist theory explains it well. In 1992, I presented a paper in England at a meeting of the Society for Interdisciplinary Studies. Its title was "A Catastrophic Reading of Religious Systems,"¹⁸⁶² and its purpose was (if I may quote myself) to "Consider organized religion as a human product . . . an instinctive creation of the collective unconscious," interpretable as a response to the belief of catastrophism

". . . that global natural upheavals have occurred in the human past, caused by extra-terrestrial cataclysms, leaving behind in us huge unconscious collective terror."¹⁸⁶³

¹⁸⁵⁷*Ibid.*, p. 11.

¹⁸⁵⁸*Ibid.*, p. 32.

¹⁸⁵⁹*Ibid.*, p. 36.

¹⁸⁶⁰*Ibid.*, p. 42.

¹⁸⁶¹*Ibid.*, p. 56.

¹⁸⁶²Published in the Society's journal, *Chronology and Catastrophism Review*, Vol. XVI, 1994, pp. 2-10.

¹⁸⁶³Irving Wolfe, "A Catastrophic Reading of Western Cosmology," *Evidence that the Earth Has Suffered Catastrophes of Cosmic Origin in Historical Times. Proceeding of 1993 Cambridge Conference. Chronology and Catastrophism Review*, (London, England, 1994), p. 51.

I described that terror as consisting of three parts, the fear of individual death, the fear since the atom bomb of the death of the entire species and the fear (prompted by our unconscious racial memories of catastrophe) of the death of the world. This I called the Triple Terror, and I analyzed all of the world's extant religions to see if their deepest purpose was to placate this concern, which, if true, would go a long way to confirming catastrophism. To summarize the results in a very small nutshell, I was able to reduce all of the world's religions to two types of chains which, although different on the surface, "exhibit an overwhelming concern for the Triple Terror and can, therefore, be interpreted as different methods of escape from [the same] catastrophic fear."¹⁸⁶⁴ I then went on to argue that, at their deepest common ground, the world's religions are similar:

" . . . their common message is 'Don't worry.' In this way every religion lessens fear about life and the world by making chaos non-random and offering the believer eternal salvation from disaster. They are, therefore, interchangeable human constructs created not to serve God but our anxieties and needs Religion is, therefore, finally a single phenomenon explicable catastrophically as a universal human impulse (with local divergences) whose principal unconscious purpose is alleviation of the Triple Terror."¹⁸⁶⁵

In a companion paper entitled 'A Catastrophic Reading of Western Cosmology,' (presented at the SIS Conference at Cambridge University and published in the Proceedings), I interrogated each of the major cosmologies our culture has produced, from the pre-Socratics through Aristotle to Newton, and I found that they too, each and every one, seek above all else to assuage the Triple Terror. I will leave a more specific analysis of the points made there until I come in the following pages to analyze Greek, medieval and early modern thought about the universe, to show that what happened in those eras is the same as the Velikovsky Affair. For the moment we are still dealing with the 18th and 19th centuries, and with my contention that the geological debates of that period were essentially religious. I will, therefore, only quote what I said in the concluding paragraphs about our compulsion to construct theories that imply that the world is safe.

"It is as if we can discuss nothing else about the world, as if an obsession with the question of its survival has been the major concern of human thought in all our history from the first records to the present.

"There is a single overriding explanation that can be offered To see the history of Western cosmology as I have described it here, added to what we discovered about religion . . . is to recognize that all of our idea systems about the natural world, which is to say all of our very large intellectual constructs, appear to have a single purpose, which is to convey the bottom-line message 'Don't Worry.'¹⁸⁶⁶

¹⁸⁶⁴*Ibid.*, p. 52.

¹⁸⁶⁵*Ibid.*

¹⁸⁶⁶*Ibid.*, p. 64.

The "scientific" credo of Hutton, Buckland and Lyell is this sort of theology, and the "scientific" uniformitarianism of the 19th and even 20th centuries, (of Darwin to Shapley), is simply more of the same. They are related items which form part of the set of responses to the Triple Terror that our society has been creating over and over again since its origin.

Demonization

If we say, for the sake of argument, that the Hutton-Lyell-Darwin fairy tale was a religious faith, we should expect not only euphoric statements on its behalf but fanatical attacks on its opponents as the inevitable reverse side of the coin, and this is just what occurred. In the early 19th century, the enemies of uniformitarianism were pictured as little short of being servants of the Devil, and, like the maligning of Velikovsky in the 20th century, these attitudes persisted for a surprisingly long time. Hallam tells us that the neptunists, who were in the minds of their enemies associated "with the more extreme catastrophist ideas, with rigid adherence to the Mosaic account of history,"¹⁸⁶⁷ were "generally given a bad press."¹⁸⁶⁸ That is putting it mildly. They were called fantasists, supernaturalists, people who began with *à priori* concepts rather than evidence and who were guilty "of willfully forcing facts into line with subjective delusion,"¹⁸⁶⁹ (which, if anything, is far truer of Lyell, as we shall see later), while the uniformitarians were always presented (by their own supporters, of course,) as sober, sensible, empirical and intellectually sound. This is blatant propaganda by the winners and, to anyone not aware of what was at stake, the hysteria would seem extremely excessive. Hallam, for instance, wonders why the animosity to Werner "seems to have persisted for an unduly long time after the controversy was over" while "Demarest, Hutton, and their supporters have been, in contrast, treated reverently as paragons of the empirical, intuitive approach"¹⁸⁷⁰ and he is puzzled why uniformitarianism was pictured then as "a triumph for the forces of light . . . over the powers of darkness."¹⁸⁷¹ We however can understand that it was a terrible fear of catastrophe that underlay the demonization of the catastrophists then, (and still does), for we know, as Hallam guesses, that the truth about the past of the Earth is "more complicated" than the simplemindedness of uniformity.

Because the battle lines were firmly drawn, it led both sides into excess and error. Lyell, for instance, simply rejected any data that seemed to lead to the possibility of catastrophism, including, for a long time, indications of evolution in the fossil record, which he insisted were merely "additions" rising inevitably "from less to more complex and perfectly organized forms."¹⁸⁷² Similarly, the neptunist De Luc was forced to reject any indications of "the great antiquity of the world" in order to preserve the concept that there are strong signs of rapid and recent violence in geological structure, to the extent that "the memory of the revolutions which gave them birth must still be preserved among men."¹⁸⁷³ Neither side would concede that it was possible to accept both catastrophe and uniformity in one theory.

Lyell's most important opponent, however, was William Whewell, who preferred the word "science" over "natural philosophy." He agreed that the laws of nature were immutable, but felt that "secondary

¹⁸⁶⁷Hallam, *op cit.*, p. 23.

¹⁸⁶⁸*Ibid.*, p. 30.

¹⁸⁶⁹*Ibid.*, p. 37.

¹⁸⁷⁰*Ibid.*, p. 23.

¹⁸⁷¹*Ibid.*

¹⁸⁷²*Ibid.*, p. 59.

¹⁸⁷³*Ibid.*, p. 36.

combinations" of primary causes may be much more violent than their origins and, therefore, that the history of the Earth was characterized by long repose "interrupted suddenly by brief cataclysmic events,"¹⁸⁷⁴ which of course is far more sensible. To him, therefore, our geological past "includes catastrophes and convulsions of a very extensive and intense kind."¹⁸⁷⁵ Most important, however, was his attack upon the uniformitarian use of time. This had been Hutton's and Lyell's escape route, for they argued that, given enough time, every geological structure could be explained by means of a very slowly changing, nonviolent process. Time removed the necessity for invoking catastrophe. Whewell ferociously attacked their opposing time to force and said that, when the uniformitarian theorists relied on time alone, "to call in one, to protect us from the other, is . . . presumptuous" and to choose either alone is "superstition."¹⁸⁷⁶

This is the critical observation, that time, gradual and ordinary, was invoked "to protect us from" (to shield us from having to acknowledge) force, for this would have been an especially crucial issue to the English gentry in the hundred years before Darwin. By far the most important political event of that epoch was the French Revolution, the conquest of Europe by Napoleon, his defeat and the emergence of England as No. 1. To Britain, it exemplified the quintessential opposition between suddenness and gradualism, or between randomness and purpose, and it was applied directly to science. A hundred and fifty years later, here is Stephen Jay Gould's analysis of the political nature of that "scientific" debate:

". . . scientists began to see change as a normal part of universal order, not as aberrant and exceptional. Scholars then transferred to nature the liberal program of slow and orderly change that they advocated for social transformation."¹⁸⁷⁷

Gould did not emphasize that it was mainly British scholars who did this, not French, but it is easy for us to see why. To put it crudely, the British elite had a good thing going—a profitable economy, a self-glorifying social dogma and a docile lower class. It was for them a quite convenient social model in which all was gradual, peaceful, relaxed and led by design. If, however, the French model became the norm (having one's head chopped off is sudden and violent and intense and purposeless) then it might infect English society, and who knows where that could have led? On the theory that, consciously or unconsciously, every nation tries to discern what the pattern of heaven is and to model its politics on that, (below being a mirror of above), the British elite wasn't bloody going to have catastrophe in politics or in the heavens, and they saw to it that the British values of moderation, gradualism and smallness won in every sphere of thought. Every area of intellectual research was forced into that one mold.

The English geographer Hallam, who is reluctant to embrace this line of explanation, nevertheless begins his 1990 book with this statement:

"We have come a long way from acceptance of the conventional Victorian belief in the disinterested scientist, engaged in the objective pursuit of Truth, to a less lofty but more realistic one which takes account of the existence of a whole range of social interactions within the scientific community as determinants of scientific theory Schools of thought related to individuals,

¹⁸⁷⁴*Ibid.*, p. 53.

¹⁸⁷⁵*Ibid.*, p. 54.

¹⁸⁷⁶*Ibid.*

¹⁸⁷⁷Stephen Jay Gould, *Time's Arrow, Time's Cycle*, (Cambridge, Mass., 1987), p. 217.

or even nations, can play a role, and prejudice . . . may frequently complicate the straightforward interpretation of data."¹⁸⁷⁸

This is good advice, and we heed it when we describe the geological debate of 1780 to 1860 as a *religious* battle about whose view of the world would triumph. Europe had to adjust after the new cosmology of Newton and Laplace, the new geology after Hutton and the new politics after the rise and demise of Napoleon. It had to find a new myth to absorb these spectacular revolutions, and the escape from suddenness in all of these fields was the slowness of great lengths of time. Given the framework of enough time, there was no need to perceive paroxysmal processes anywhere, and this line of thought made it possible for the British to continue to believe that the heavens were still perfect, as was the Earth, as were the animals, with man at their head, at the top of which was the moderate English gentleman. The British came up with a new narcotic to suit the new data and made politics as impervious to catastrophe *in the long run* as was the Solar System. From a catastrophist viewpoint, we can easily interpret this emphasis upon gradualism as prior desire forced onto new observation to give the same general result as Darwin and Shapley—all is ordered and gradual, it always was and will be, and consequently the world is safe. The surface message became: the political world should be *kept as it is*, we don't want revolutions. Underlying message—we don't want catastrophes. To summarize, what occurred in science in the late 18th and early 19th centuries was a process of myth-making, of ideological coercion, as we had already seen with Darwin and Shapley. We always squirm in the face of new ideas until we find a way to make them part of a comforting model. We then support it and attack its opposers (as was done with Velikovsky) to make ourselves believe that the collective delusion we have created (which removes the fear of catastrophe) is the Truth. It was the same old ugly story.

Lest the reader fear that I am placing too great a stress on the time, the ideological framework within which these allegedly objective debates occurred, (as I insisted on the relevance of the postwar cold war period in which *Worlds in Collision* appeared), I again quote Hallam on science. The accumulation and interpretation of evidence by science, he says, is not innocent.

". . . observations are theory-laden States of innocence are reserved for the Garden of Eden."¹⁸⁷⁹

The castigation of opponents, therefore, is no more objective in science than it is in politics or religion, which is how Gould explained the vicious desire by uniformitarian science to put down, defame and erase the catastrophists of the 17th and 18th centuries.

"To many scientists, natural cataclysm seemed as threatening as the reign of terror that had taken their great colleague Lavoisier."¹⁸⁸⁰

Both consequently had to be *forbidden*, political and cosmic catastrophe, and what followed, therefore, in the early 19th century was the expectable but sad process of glorification of what one wants and demonization of what one fears, (just as it occurred with Velikovsky in 1950), with the same long-lasting effects:

¹⁸⁷⁸*Ibid.*, p. viii.

¹⁸⁷⁹Hallam, *op. cit.*, p. 223.

¹⁸⁸⁰*Ibid.*, P. 217.

"What is extraordinary about the self-proclaimed founders of modern geology . . . is that until very recently standard historiography has . . . accepted their contemptuous dismissal of early thinkers about the earth as undisciplined speculators."¹⁸⁸¹

Every totalitarian elite, political or religious, (even one as seemingly democratic as 19th century Parliamentary England), has always sustained its energy by presenting itself as Light and its opponents as Darkness. That is as true of Inquisitional Spain or Nazi Germany as of Lyell. We may summarize by saying that the uniformitarianism of the 19th century, "which evolved in opposition to an episodic, catastrophist view of change epitomized by Cuvier and his followers,"¹⁸⁸² and which Shapley a hundred years later still implicitly believed, is a human creation, not The Truth. It goes in the direction *we* want, not that dictated by the evidence, and it is elevated into dogma by group pressure, as it was also in 1950. As I will show in the next section, it is what we have always done, just as we have always demonized the opposition. The Harvardist defense of the PCP against Velikovsky, (which I described earlier), was merely the most recent in this series of repugnant instances.

Aristotle

Now to Aristotle and Newton. I had said that, to understand Shapley and the ASG we must go back to Darwin, and to understand Darwin we must go back to Lyell. This has allowed us to reconstruct the wishful origin of uniformitarianism. Now, to understand Lyell, Darwin and Shapley together, we shall have to look at no less than the 2100 years before them, for in my opinion they are not only uniformitarianism's legitimate but immediate background. I am referring here to the astronomical ideas of Aristotle, Aquinas and Newton, who between them form what I feel is an unbroken thread of cosmological theory, a unified chain of concepts which functioned not merely as the foundation for science during that time, but became the moral and political underpinning of Western culture, (since life copied the skies). I will argue that the large views which these three men produced (or which evolved about them) stand in a direct line leading to the PCP. Each appeared after a period of intense debate about the nature of the world, each advocates virtually the same fairy tale of cosmic order, and each thereby became for its time the most powerful suppression of doubt about the benevolence of the universe. It is the same pattern which we found underlying the three more recent periods of ideological combat I have already discussed, which must lead us to suspect that whenever, in our entire cultural history, suspicion has arisen about the total kindness of nature, we have always created a new dogma to confirm it.

To begin with Aristotle and work forward to Newton, he must be understood in relation to the many and very divergent theories of the universe which abounded and clashed in the three centuries before him. As I outlined in my aforementioned article on cosmology,¹⁸⁸³ at least three major types had appeared in Greece, ranging from the (probably early) Homeric dish universe floating on water between and being oppressed by heaven and hell, to the (probably subsequent) materialist dynamic models of the pre-Socratics, whose universes (whatever their particular choice of fundamental matter was) were stable and solid, even if impersonal and without purpose, which was much better than the Homeric picture of a cosmos filled with

¹⁸⁸¹*Ibid.*, p. 1.

¹⁸⁸²*Ibid.*, p. 217.

¹⁸⁸³ "A Catastrophist Reading of Western Cosmology," in *Proceedings of the 1993 Cambridge Conference*, publ. Society for Interdisciplinary Studies, London, England, 1993.

turbulent, selfish, insensitive gods. This might have been followed by the Platonic-Pythagorean concept of a world functioning in perfect order according to Number and Form, constituted of various geometrical shapes and held together in musical harmony by a World Soul. While this is far more comforting than the previous concepts, I pointed out, however, that the World Soul is moral and one may anger it by transgression.

"There is, therefore, in the Platonic-Pythagorean universe no absolute kindness to Earth and mankind, or, if it is eternal, it is not continuous but may be interrupted by the occasional spanking The overall concept, therefore, while it is comforting, is not perfectly so."¹⁸⁸⁴

It is against these predecessors that Aristotle's success in the ideological-theological-cosmological marketplace must be understood. Consider what he did—he presented a world of perfectly interlocked spheres which moved in perfect circles guided by a Prime Mover who is completely good, with the Earth at the center. This means not only that the universe is perfect and eternal, nor even that this perfection is controlled by a divine force which is omniscient and omnipotent, but that this whole structure cares for humankind, and, therefore, *heavenly catastrophes are not allowed*. As Lynn Rose perceptively observed in *Mankind in Amnesia*

"Aristotle's system . . . not only has been the most influential of all cosmological theories, but also is the most excessive of all such theories in its astronomical uniformitarianism. His views are at the farthest possible extreme from those of Velikovsky; indeed, Aristotle's entire system seems specifically designed to eliminate the very possibility of worlds in collision. That has also been the reason for its enduring popularity and appeal."¹⁸⁸⁵

This element in Aristotle's cosmology is the very core of his vision, and Rose is perfectly correct in giving it the prime emphasis. The Earth is presented as being "immovable,"¹⁸⁸⁶ and all sublunary change is "cyclical," while the heavenly "spheres are unchangeable and impenetrable: the only "activity" that they are allowed is rotation."¹⁸⁸⁷ To make sure that only this occurs, each sphere has its own "guardian angel . . . to keep it moving at an absolutely uniform rate" and each planet "is set . . . on the equator of one of the spheres."¹⁸⁸⁸ More than that, as Rose said

"Each of these planet-bearing spheres is enclosed within layers of other, 'unplaneted' spheres. Thus the planets cannot get near each other, any more than they can get near us Aristotle has not only removed the planets two

¹⁸⁸⁴Wolfe, (1993), *op. cit.*, p. 55.

¹⁸⁸⁵*Ibid.*, pp. 52-53.

¹⁸⁸⁶*Ibid.*, p. 53.

¹⁸⁸⁷*Ibid.*, p. 54.

¹⁸⁸⁸*Ibid.*

stages from any originating source of motion, but has ensured that the source of motion will be rational rather than blindly irrational."¹⁸⁸⁹

Rose speculates that this vision was created "to soothe Aristotle's deep-seated fear of planetary catastrophes."¹⁸⁹⁰ Using the fruits of the extensive historical research carried out by different catastrophist scholars since that chapter was written, (including Rose), we can be a little more precise. Velikovsky in his "revised chronology" moved things closer to our time by up to 800 years. I think, however, that "revised" is too neutral and gentlemanly a word, and I prefer "shortened." Velikovsky's is a shortened chronology which in the past ten years has been shortened even further by the work of Gunnar Heinsohn and Lynn Rose himself. If we locate Aristotle within what we might call the Very Shortened Chronology of Rose and Heinsohn, then the Greek concern with the nature of the world takes on a very different hue, for *the last of the catastrophes might have ended not more than 300 years before him*. This is perhaps too long for the events to have been kept alive in living memory, but they could have persisted in oral historical narrative, or the process of repression by collective amnesia (but retention in disguise in myth and art) could have begun.

Whatever the state of the memories of the catastrophes was, their recurrence, as people like Jung and Freud have pointed out, would have been feared consciously or unconsciously. The Greeks, therefore,

". . . would desperately want to believe the opposite, and, if Aristotle emerged as the winner, then . . . his view won among the many contenders because he gave the people of his time the best reason to believe the opposite. Aristotle is a much better escape from that fear than any of his predecessors This in my opinion is the key to Aristotle's success Aristotle's moral and physical concepts provided an extremely comforting statement about the three elements of the Triple Terror: humankind is special, we as a race will survive and so will our abode, the Earth."¹⁸⁹¹

Aristotle's message, therefore, is the same one which we have encountered from Shapley to Darwin to Lyell. It says, "Don't worry." The world is safe, it always has been, and (of course) there will be no catastrophes. As Lynn Rose put it,

"His denial of what had happened ... went to such extremes that he created a system in which interplanetary near-collision not only did not happen, but could not possibly happen.... No one, in the entire history of recorded thought, did more than Aristotle in an effort to de-legitimize catastrophism."¹⁸⁹²

To summarize, if we are correct that late classical Greece was still in a state of traumatized aftershock from giant catastrophes, then what would have been fervently desired was a myth in which the world would be controlled by a force which would never allow catastrophe, and in this framework

¹⁸⁸⁹*Ibid.*

¹⁸⁹⁰Lynn Rose in Immanuel Velikovsky, *Mankind in Manesia*, (New York, 1982), p. 53, (henceforth, *MIA*).

¹⁸⁹¹Wolfe, (1993), *op. cit.*, p. 55.

¹⁸⁹²*MIA*, *op. cit.*, p. 53.

"Aristotle may be seen as collective unconscious desire run wild . . . reason would not be involved, except as the servant of desire. Fear would have led, and the intellect of Aristotle would have done what fear wanted."¹⁸⁹³

The Scholastics

Next come the Middle Ages. It is known to everyone that the cosmological model of Aristotle, as adopted by Rome and later adapted by the medieval Church, constituted the official cosmology of Europe for close to 2000 years. We shall turn now to look at this medieval system, which I shall argue is an even more outrageous attempt to flee from the Triple Terror than its classical predecessor. It is perhaps best if we see medieval cosmology as occurring in two phases: the first, a modest improvement on Aristotle, while the second rises to an ecstasy of collective self-delusion. Scholasticism, which is the label most often applied to Church thought of the Dark and Early Middle Ages, evolved a model founded on Aristotle's "in which everything the Church desired . . . could find its most desirable place."¹⁸⁹⁴ All of then-current medicine, biology, cosmology and psychology was put into a

". . . vast structure . . . ruled by God, who keeps the cosmos spinning through perfect love, acting through his agents, the choirs of angels, who are both the heavenly harmony of Plato . . . and the intelligences of Aristotle. Absolute perfection, wholly benevolent, exists in the vast cosmos above the Moon and keeps the terrors of Earthly existence in constant check."¹⁸⁹⁵

The Dark Ages, to a large extent, Christianized Aristotle, joining him to their emergent dogma that had begun to take form in the second half of the millennium, especially after the Council of Trent. As a result

". . . the whole of Christian Europe believed in a cosmic structure even better than Aristotle's, in that it propounded a universe not merely ruled by a well-intentioned Prime Mover . . . but created by God precisely *for* that purpose."¹⁸⁹⁶

Sounds good, does it not, but I then wondered rhetorically "Could one ask for more?" and I answered yes, because this early Scholastic theory still did nothing about the sublunary world where, despite the eternal pacificity of heaven above, any of the Triple Terrors could still recur on Earth below. How much better would it be to pacify this region too, and that I feel is what late Scholastic cosmology did. I am referring here to post-Aquinian Christian cosmology, which occupies a distinct rung on the ladder of fantasy leading from Aristotle to Newton (and, by extension, Shapley). The problem was that in the scheme of Aristotle everything is fixed forever, *i.e.*, to describe the present is thereby to describe the past and the future,

¹⁸⁹³Wolfe, (1993), *op. cit.*, p. 55.

¹⁸⁹⁴Irving Wolfe, *op. cit.*, p. 56.

¹⁸⁹⁵*Ibid.*

¹⁸⁹⁶*Ibid.*

(just like Laplace 2400 years later). Nothing fundamentally changes. The Christian Church, however, had a very different vision of both the past and the future, for it "was predicated upon a . . . drastic evolutionary scheme, an arrow of time moving toward the Millennium." The reconciliation which late Scholastic Christianity conceived between these two seemingly irreconcilable systems was to combine them into successive segments of a united belief

". . . that the universe possessed a stability at the moment . . . but that Christian eschatology was nevertheless correct to predict that even this . . . would one day be destroyed in a tumultuous upheaval [the Battle with the Antichrist] and be replaced by a new unmoving unchanging and thus more perfect permanence."¹⁸⁹⁷

It is very important to recognize what the Aquinians did. Aristotle had reasonably separated Above from Below and had accepted that Earthly life could be capricious and irregular, (because that could hardly be denied), but, in the Scholastic model

". . . when the Kingdom of God will be achieved, every single part of the physical cosmos will be orderly because Below will be just like Above. All will be Above. This will make the Earth as safe as the heavens and mankind will then not need to have any kind of fear, local or cosmic."¹⁸⁹⁸

What the medieval Church taught was that this new world, this paradise of security, was available to everyone who believed in its message, for these people would be resurrected into that new world and would

". . . then live forever in perfect peace and happiness. This is the vast narcotic which medieval Christianity sold It is the wildest dream-fulfillment of self-placation which our culture has developed and it dominated Western thought until Bruno and Galileo."¹⁸⁹⁹

In the light of the thesis I am developing, we can easily argue that deep catastrophic fear underlies this medieval vision, that it, just like all the rest of our cultural concepts we have examined, can be consistently interpreted as an hysterical, collective response to unconscious racial memory.

"It is easy to interpret this concept . . . as pure desire propelled by fear, just like its predecessors. The underlying message is that people do not have to worry [for] . . . to see things correctly, from the heavenly or large-scale point of view,

¹⁸⁹⁷*Ibid.*

¹⁸⁹⁸*Ibid.*

¹⁸⁹⁹*Ibid.*

is to perceive that . . . the universe is like a giant orchestra led by God, which will be forever in tune."¹⁹⁰⁰

What is more important, however, is that if we grant the desire-laden character of Scholastic eschatology, then we also have to recognize that the "philosophical" scheme of Aristotle and the "theological" concept of the Scholastics and the "scientific" models of Lyell and Darwin are merely different names *we give* to what are really the same recurring obsessive and very human phenomena. It is always the same!

Newton

This brings us to Newton, whose world view, if we take it back to Copernicus, has been our major dogma for more than 400 years. Most people take Newton to be completely true, (as was said like a catechism by Velikovsky's critics), and accept without much question the stability and knowability of the universe—it *can* be understood and it *is* stable, and every advance in science will only reveal this more incontrovertibly. This I call Newtonianism, a theology more than a cosmology; a mystical vision of heavenly order created by the manipulation of large-scale projection into a religion of the Universe. It has evolved over the past 250 years into our modern world's most powerful Truth, but I propose to look at it now in a very different way, "as an unconscious, collective response to the pressure of unconscious collective terror," which is to say "as a human construct founded on desire, like its predecessors."¹⁹⁰¹

The exposure of Newton will require two steps. First we will take a close look at his theory to show its weakness as science, and then we will look at it as religion, to show its strength as myth. Concerning Newton as a description of the physical world, we have to understand that those of Velikovsky's critics who blandly use Newton as a measure by which to utterly dismiss Velikovsky (which includes almost everyone from Shapley in the 1940's to Bauer in the 1980's) are wrong. They are simply betraying their ignorance of the implications for cosmology of quantum and relativist physics which have been available since the 1930's, as well as of the discoveries in astronomy in the last two decades. In contrast to them is the attitude of Jacob Bronowski in mid-century, who may serve as an example of the well-informed scientist. Compared to how Newton was accepted in his own time, (as summed up in Pope's poem "God said, Let Newton be! and all was light"), Bronowski at the present time is aware that the concept of "a world outside ourselves where we simply look on and observe," upon which Newton is predicated, is a myth.¹⁹⁰² He understands, (as Shapley and the ASG didn't), that science in the quantum age can have "nothing whatever to do with bold generalizations about the universal workings of cause and effect"¹⁹⁰³ because "there is no universal 'now' upon which to found one's *obiter dicta*, "there is only "here and now" for each observer."¹⁹⁰⁴ To be more precise, Planck had shown that the world is discontinuous and uncertain and Einstein had rejected absolute space and absolute time, which meant that there was no objective world for the Newtonianist to measure nor a Newtonian observer free of a frame of reference to measure it. There was only a continuum created by the interplay between science and nature. What this means is that Newtonian classical determinist science, which Shapley to Bauer believe in implicitly, cannot be done. As a result, said Bronowski, gravity, the great underpinning of classical science, was an illusion —

¹⁹⁰⁰*Ibid.*

¹⁹⁰¹*Ibid.*, pp. 57-58.

¹⁹⁰²Jacob Bronowski, *The Common Sense of Science*, (Harmondsworth, England, 1960), p. 82.

¹⁹⁰³*Ibid.*, p. 76.

¹⁹⁰⁴*Ibid.*, p. 72.

". . . the laws of gravitation have gone. There is no gravitation; there is no force at all; the whole model was wrong The true causes . . . have no resemblance to the causes in which we believed for nearly three hundred years. Gravity was a fiction. Something else was at work."¹⁹⁰⁵

Newton's theory is still very useful today if it is used merely as a tool to calculate motion, (as Ptolemy's was 2000 years ago), but, when it is evaluated as a purported portrait of what nature is actually like, then, like Ptolemy, it is simply not good science. To recognize its weakness as science, however, is not primary now. What is more important for our purposes is to perceive how well Newtonianism functions as myth, as a denial of the Triple Terror, like its predecessors. Notice the similarities—just like Aristotle, just like the Scholastics, he depicts celestial objects in perfect motion around an unmoving center, with heaven a region totally free of sudden or violent alteration which exists in peace eternally, and this immense universal world of safety out there renders the tiny local world of fear down here understandable, just like Aristotle and Aquinas. In all three systems, heaven and earth are very different but are ruled nevertheless by the same force, and in all three systems local irregularities like comets or tornadoes are insignificant in the long run, which is regular. These are the three consecutive victors in Western cosmology, and it should be easy for us by now to see that they all are attempts to deny the Triple Terror. Most important, the impulse behind the three cosmologies is the same—it is "Don't worry." Local disorder is temporary, while order is the true nature of the universe. Each system comforts our fear of local instability by opposing to it a picture of cosmic stability, which is, therefore, their common message.

"I had been taught as a schoolboy that the Newtonian world view was revolutionary, but it wasn't: it was just like Aristotle's Newton is simply Aristotle in modern dress."¹⁹⁰⁶

Consequently, whether the theory is phrased in medieval syllogistic or Greek dialectic or modern mathematics, these are mere alterations of clothing, but there is no change in content. The ideas have merely changed form.

"It is as if, when it comes to our largest and deepest thoughts about the Universe and us, humankind is unable to produce anything else."¹⁹⁰⁷

Seeing Newtonianism like this, as a myth identical to its predecessors, helps us to interpret it like its forebears as a product of desire stemming from a concern about the stability of our world, one that has persisted since our earliest histories. We want to think that the world runs like a clock, we want to think that one Force controls it all and we want to think that it will be that way forever. That is to say, its myth of order and perfection seems to be something we have to believe in. After every period when doubt arises, we have to evolve a creed which will renew our confidence that cosmic perfection exists, and that I feel was Newton's true function. He did for us what Aristotle and Aquinas had done before him, and for the same reasons. We may, therefore, say that his theory

¹⁹⁰⁵*Ibid.*, p. 71.

¹⁹⁰⁶Wolfe, *op. cit.*, p. 61.

¹⁹⁰⁷*Ibid.*

". . . has replaced Aristotle and Scholasticism not because it is truer . . . but only because it is a better medium for the same old message Ptolemy, Aquinas and Newton were successively canonized not for their increased precision or "correctness" but for their common pacifying picture of the world."¹⁹⁰⁸

¹⁹⁰⁸*Ibid.*, pp. 62-63.

THREE: THE PRESENT PHASE

Now that our historical review has been completed, it will allow us to come to the heart of our analysis, which is the subversive, fanatical manipulation of belief in our culture. Remember, the Velikovsky Affair was and is a battle between competing ideas—is the world orderly or not, is the world predictable or not, and, therefore, *is the world safe or not*. It isn't the mandate of this book to attempt to settle that question, for we are probing into the causes of the Affair, *i.e.*, the way in which science has conducted itself in the battle. Readers who want to become familiar with the catastrophic debate itself are advised to consult the Bibliography appended to the end of the book, which lists all the books, reviews, journal articles and conference proceedings in which the research by Velikovsky himself and by what are now two generations of catastrophists (not all of whom fully support Velikovsky) is presented. Half a century of scholarship is there, and it constitutes a formidable body of evidence in favor of the concept of catastrophism. There are also several books and more than a dozen articles by mainstream scientists which support some form of catastrophism, most of them having been written within the last two decades, indicating that support for catastrophism has grown rather than diminished since 1950. For those who enjoy sinking their teeth into a complex and juicy controversy, these texts will surely tantalize the intellectual appetite. More than that, I feel they will form the foundation for the academic study of catastrophism in future centuries from the point of view not merely of science itself but of the history, philosophy and above all sociology and psychology of science.

Science and Art

Our mandate here, however, is to cogitate about the Affair, which is to say about the consistently excessive misbehavior of scientists when they are confronted by catastrophism. It might be useful if we begin our quest by posing to ourselves certain pertinent questions about this misbehavior, the answers to which could take us a long way towards a solution. First, consider what this book's chapters have illustrated: scientist after scientist stumbling into error. Our first question, therefore, has to be why it is that almost all of Velikovsky's opponents have made fools of themselves regarding catastrophism, when they are generally not fools. Then, why have they degraded themselves in their attacks on Velikovsky, not in areas outside their competence, but within their very fields of specialization? Why have almost all of them betrayed the deepest principles of their disciplines when they wrote about Velikovsky, but not anywhere else? Why have most of them been guilty of despicable personal behavior when they debated Velikovsky, but not anywhere else? These are not merely irrational acts, they are non-typical of the individuals when catastrophism is not involved. It is obvious, therefore, that the common factor in all of these uncommon instances is the theory of catastrophism, which seems to provoke uncontrolled, immature, emotional responses in people whose lives apart from that have mostly been dedicated to controlled, mature and strictly intellectual pursuits, and that is what we have to explain. The answers to our questions will help lead us in the right direction, for they will shed light on the different roles of science in our society and enable us to perceive that, quite surprisingly, the greatest of

those roles (the cause of the Affair) is not merely unscientific but antiscientific, its object being not to find truth but to obscure it.

To explain, let us begin with the three astronomical concepts we have just analyzed. They span the entire history of Western cosmology, *but they all say the same thing*, in different languages, but an identical message. It is, therefore, much less important to consider what they tell us about the world than what they can tell us of humankind. What can it be in the mentality of our culture that it has produced these *and only these* conceptions of the universe? As the reader will easily guess by now, it is my belief that the history of Western cosmology is nothing but the eternally recurrent creation of a picture of cosmic stability, and that we have put this message of comfort from ourselves to ourselves into our largest ideologies to make ourselves

believe it is true. That is truly why we have created them, to keep saying to ourselves what we need to have said.

The consequences of this insight are vast. We in the rationalist West have been traditionally trained to believe that art, myth and religion are untrue (because they are subjectively created by humankind) whereas science is true because it is the objective product of nature. In contrast, I have been advocating for the past two decades a very different interpretation of art and its social function. As I have outlined in more than a dozen articles on drama, literature, films and soap opera, (published in the journals *Kronos*, *The Velikovskian*, *Aeon* and the *Society for Interdisciplinary Studies Review*), it is my theory that we have to look at all cultural objects as having an unconscious origin. Specifically, I feel that, if we as a species have been traumatized by the experience of several enormous cosmically induced catastrophes within the past few thousand years, we would tend to react much as a traumatized individual would. That is to say, we, like an individual, would try to develop methods to deny the trauma while creating opportunities of reliving it in disguise, both of which would help us to function in our daily lives. In an individual, it is understood that some part of him creates the delusions, (we know not which), while a different part creates the disguised reenactments. If we as a whole culture can behave like a traumatized individual, then it is my theory that the artist is that part of society which placates the trauma through the production of great art. In plays and novels and poems and opera and ballet and paintings, we can revisit the catastrophes in disguise, under control, with ourselves as the aggressors and with a happy and/or closed ending, which allows us to relive the horror in a much more satisfactory and bearable manner. It is an entirely subconscious transaction both ways, in which the artist unknowingly functions as that part of society who on behalf of the entire society creates the myths that will allow the memorial pressure of the suppressed catastrophic experiences to be lessened by symbolic reenactment of the trauma, in what Freud called the *repetition compulsion*. (This topic will get extended treatment later).

The obverse side of the coin, however, is that we also have to have a part of society which unknowingly produces our mechanisms of denial, and it is my theory that that function in our culture is fulfilled above all by science. That is to say, I will now try to show that science in its largest sense may not, as everyone thinks, be a tool to unearth truth, but a method to create a lie by which we subconsciously fool ourselves about the truth. In a paper I presented at a conference on catastrophism in Portland, Oregon in 1994, (published in the *Proceedings*, 1995), I divided science into five areas—scientific problems, methods, research, laws and theories. In general, it can be accepted that the first four, the set containing science's problems, methods, research and laws, does constitute a legitimately self-contained field of human endeavor which produces knowledge of a certain kind about what we call the physical universe. Its object *is* to find the truth. There are very deep doubts raised by philosophers of science about the ultimate validity of this knowledge, which I treat at length in a philosophical book I am writing on the limits of human knowledge, but in terms of this book, that is not relevant. As far as we are concerned here, we can take the fruits of the problem-to-law chain at face value.

That cannot be done, however, with the fifth section, the large generalized ultimate theories of science, for it is here that in my opinion science's unconscious social function as collective self-denial of the past occurs. The title of my talk just referred to was 'Big and Little Science,' and by Big Science I mean these Grand Theories, which I argue are not scientific because, for the most part, they can't be quantified, they cannot be tested and, therefore, they are not subject to disproof.¹⁹⁰⁹ They turn out to be a world of their own, often unrelated directly to what is done in Little Science, but, much more important than that, what they contain above all else is the religion of classical uniformitarianism, *i.e.*, statements of faith that the world is orderly and that our mainstream scientific theories discern it. That is what they dispense above all else in large gobs. If this is true, then, as I wrote, the function of Big Science is mainly

¹⁹⁰⁹ "Big and Little Science," in *Ancient Myth and Modern Science*, Kronia Publications, David Talbott, ed., 1995.

". . . ideological manipulation . . . we seem to have departed from the world of 'knowledge' and 'laws' and to have entered a realm of pure propaganda, where little science in its first four dimensions is being used in the fifth, the Alice-world of Big Science, to sell us a view of nature which does not follow from the evidence."¹⁹¹⁰

That view, of course, is uniformitarianism—the world is regular, the world is stable, and, therefore, catastrophes cannot have happened. The tactic is to use small achievements to lend credence to the Big Lie— if all of the local accomplishments of science are true, then uniformitarianism must be true too. That is just what Shapley said.

As a result, I contended that "Big Science is not a product of scientific data, but of human nature" and consequently that "What it upholds would, therefore, not be what physical nature requires us to believe about the world . . . but what human nature wants to believe about the world." More specifically, I surmised that, "if Big Science is a structure we and not the world have devised, what it advocates must be a denial of what we are afraid of." That, of course, would be non-uniformitarianism (or "episodes" of instability, or catastrophism), and, therefore, we may guess

". . . that, unconsciously and hysterically, our culture as a whole has collectively evolved or created classical rationalist uniformitarian Big Science not to find out the total Truth about nature but, in a pretense of doing just that, to use the success of small science about nature as the bait to get us to swallow the lie of Big Science about nature. The big lie works . . . because small science is true."¹⁹¹¹

The cause for this frantic behavior derives directly from catastrophic theory —

". . . if we are all racially terrified by unconscious inherited memories of immense global natural catastrophes, we have to deny them somehow, and to tie Big Science to the coat-tails of little science does the trick neatly."¹⁹¹²

This, in my opinion, is the subconscious use that our culture has made of Big Science—it is a vast lie that we have frantically created to deceive ourselves.

The corollary is perhaps even more revolutionary: we have been taught since Bacon that verifiable science is true and that everything unverifiable, like art, religion and myth, is an "idol" or falsehood. That is the largest truism in our culture. If, however, art and myth and religion contain a vast kernel of truth about the real history of our planet and ourselves, as Velikovsky said, and if mainstream science's largest social role is to market an untruth about the world, as Velikovsky said, and if as I have said the most urgent question we must put to the history of ideology is not to what extent a particular theory is true (Merton) nor how do ideologies change (Kuhn) but why, underneath each surface variation, "where it counts, where the life of

¹⁹¹⁰*Ibid.*

¹⁹¹¹*Ibid.*

¹⁹¹²*Ibid.*

each cosmology resides . . . there is neither revolution nor evolution, but sameness,¹⁹¹³ are we not faced with the astonishing possibility that, in the largest sense, art is true and science is not? (See my chapter above on Isaac Asimov). Plato, remember, would ban all artists from his Republic because he said they create what did not happen, and creative people from the Renaissance up to the nineteenth century have had to contend with the stigma that all art "feigns." From the point of view I am advancing, we can reverse that attitude and say that it is Science which ultimately feigns by leading us from small truths to the Big Lie, whereas Art leads us from small lies (artistic creations) to a Big Truth. We can then understand Plato's attitude, for he in his last works designed a perfectly ordered *polis* to reflect what he believed was a perfectly ordered universe, and certainly, in a system founded on this great an untruth about nature, (even if, as Lynn Rose insists, Plato accepted that nature *had been* unsafe in the past), the artist (who tells the truth about nature) would be unwelcome. (See ahead on Plato.) If, however, we accept the reverse, that catastrophism is true, that the world is *not* always perfectly stable, then it is art and myth and religion which should be our foundation, not desire-created classical philosophy or modern Big Science.

Flight from the Truth

It is within this framework that we can effectively describe the entire Affair, including its most recent items, for a distinct pattern emerges, which is a collective inability on our part to face the truth about the world. For example, in the two centuries before Aristotle and Plato, (as I explained in *Wolfe 1993*, p. 2), Greek natural philosophy was rife with contending views of the universe, many of them incorporating instability, but the view which won, the view which became the consensus, was a view which advocated not merely stability but perfect order. Similarly, in the fifteenth and sixteenth centuries, people like Nicolas of Cusa and Giordano Bruno proclaimed heavenly instability, different skies, a non-providential scheme in nature and (in Bruno's theory) multiple solar systems, while in the 17th century

"On the eve of the establishment of Newtonian cosmology, the speculation on cosmic cataclysms had become so commonplace that in 1672 Molière . . . could make a joke of it:

"(We have, Madam, while sleeping, had a narrow escape. A world has passed by us . . . and if it had on its way met our earth, it would have broken it into pieces like glass.)"¹⁹¹⁴

The view which won in the late 17th century, however, was not this one but Newton's, and "The notion that the solar system may have a history became, (in the name of the new religion of science) as sacrilegious as it had been for the scholastics."¹⁹¹⁵

It is what has always happened. We have always chosen order over disorder. For example, in the late 18th and early 19th centuries, as we have seen, concepts of heavenly instability and catastrophe were being espoused by many of the leading scientists of the day, but what won was the uniformitarianism of

¹⁹¹³Wolfe, (1993), *op. cit.*, p. 60.

¹⁹¹⁴Livio C. Stecchini, "The Inconstant Heavens," *The Velikovsky Affair: Scientism Versus Science*, (London, England, 1978), p. 87.

¹⁹¹⁵*Ibid.*

Hutton, Lyell and Darwin, while in the 20th century the re-emergence of catastrophism with Velikovsky was met by the intransigent conservatism of the ASG. As I wrote,

". . . there is a common thread which runs through all the metaphysics of Western culture, and it is this: disorder challenges order, chaos defies stability, but perfection wins . . . demonstrating that a profound concern about cosmic, earthly and human stability has persisted since the beginning of recorded history Ideologically, there may really be nothing new under the Sun."¹⁹¹⁶

Velikovsky, for instance, describes the early classical Greek philosophy of the Stoics, Pythagoreans and Homer as immersed in world conflagrations, cosmic disorder and gods battling, but

"The Aristotelean negation of the traumas of the past . . . became the rock on which the Alexandrian schools were built."

He then shrewdly observes,

"The teaching of uniformitarianism (Lyell, Darwin) is a nineteenth-century version of Aristoteleanism. And as much as the Church Scientific (Thomas Huxley's expression) still follows in the steps of Darwin, it is still Artistotelean And as much as the latter term is an equivalent of Scholasticism, the Middle Ages are not yet at their end."¹⁹¹⁷

We may carry this thought straight onward to Shapley, Sagan and Asimov and observe that if a devoted Artistotelean were to have fallen asleep in ancient Athens and to have awakened in modern America, he would perceive that the PCP is alive and well and flourishing in Harvard. As long as pure uniformitarianism is desired, the Middle Ages will not end because people simply cannot tolerate the opposite.

Velikovsky, for example, recounts Darwin's original reactions, as written in his *Journal*, to the evidence he found everywhere of numerous new species and mass extinctions without extensive change in land form. Darwin at first blush asks,

"What, then, has exterminated so many species and whole genera? The mind at first is irresistibly hurried into the belief of some great catastrophe; but thus to destroy animals . . . in southern Patagonia . . . up to Behring's Straits, we must shake the entire framework of the globe."¹⁹¹⁸

¹⁹¹⁶Wolfe, (1993), *op. cit.*, p. 63.

¹⁹¹⁷MIA, *op. cit.*, p. 52.

¹⁹¹⁸Charles Darwin, *Voyage of the Beagle*, (Appleton & Co.) pp. 169-170 in *Stargazers*, *op. cit.*, p. 305.

Yet, 20 years later, he denied catastrophism absolutely in the *Origin of Species*, as we saw, and Velikovsky concludes,

"The success of Darwin, the speedy acceptance of his theory into all things spiritual and material of the last hundred years was due to his assurance that the frame of this globe had never been shaken."¹⁹¹⁹

Like each age before him and since, he had peered into the abyss, recoiled in terror and run to denial.

Virtually the same reaction may be deduced from the writings of Plato 2200 years before Darwin, who at first speaks openly of catastrophic disasters but in old age forbids the very thought of them. Professor of theology Thomas Parry quotes from Plato's *The Statesman*:

"There is a time when God himself guides and helps to roll the world in its course; and there is a time, on the completion of a certain cycle, when he lets go, and the world . . . turns about and by an inherent necessity revolves in the opposite direction Hence there necessarily occurs a great destruction of (animals) which extends also to the life of man."¹⁹²⁰

Velikovsky from the same dialogue quotes Plato describing what vast alterations can occur to planetary and solar orbits:

". . . the sun and the other heavenly bodies, how in those times they used to set in the quarter where they now rise, and used to rise where they now set . . . of all the changes that take place in the heavens this reversal is the greatest and most complete."¹⁹²¹

The result of such reversals, says Plato, are enormous catastrophes.

"Plato describes in the *Timaeus* the effects of a collision of the earth 'overtaken by a tempest of winds,' with 'alien fire from without, or a solid lump of earth,' or waters of 'the immense flood which foamed in and streamed out': the terrestrial globe engages in all motions, 'forwards and backwards, and again to right and to left, and upwards and downwards, wandering in every way in all the six directions.'

¹⁹¹⁹WIC, *op. cit.*, p. 122.

¹⁹²⁰Thomas A. Parry, "The New Science of Immanuel Velikovsky," *KRONOS*, Vol. I, No. 1, (1975), p. 15.

¹⁹²¹WIC, *op. cit.*, p. 122.

"As a result of such a collision . . . there was a 'violent shaking of the revolutions of the Soul' . . . which produced all manner of twistings, and caused in their circles fractures and disruptions of every possible kind."¹⁹²²

This is pure catastrophism, but Parry observes that "it is a younger Plato who describes the effect of drastic change in the heavens upon the earth,"¹⁹²³ whereas an older Plato in *The Laws* would condemn to death those who believe it. To Parry, therefore,

"Plato . . . bridges the past recognition that there has been dramatic inconstancy in the past [and] the quest for a basis for a source of unquestioned stability Plato appears to stand at that time when men were of a strong disposition to *deny the oral and written memories* of what had once happened by creating rational systems of thought that would provide for them the sense of security and control that they could never have if they continued to believe that the earth was indeed subject to periodic destructive upheavals."¹⁹²⁴

Parry then quotes Giambattista Vico as having written in 1725, about humankind's inability to see the truth in myth, that "The fables in their origin were true and severe narrations But because for the most part they were originally monstrous, they were later misappropriated, then altered, subsequently became improbable, after that obscure, then scandalous, and finally incredible."¹⁹²⁵ It would seem that this collective mental progression is what may have occurred to Darwin and Plato individually – at first they openly recognized catastrophe, but they later had second thoughts and retreated in panic from the consequences of admitting catastrophe, leaping to the illusion of knowledge and order:

". . . it might be said that the penchant for control and predictability in the human race plays much the same role as the ego does in the individual: it provides a sense of security and mastery, and keeps at bay as many of the forces of chaos as possible."¹⁹²⁶

The reason for this compulsive need is not difficult for Parry to guess – "man's reluctance to be open and receptive to the world, and thereby vulnerable . . . [derives from] the terrors of cosmic catastrophes . . . in the face of which he was completely helpless and vulnerable."¹⁹²⁷ In a collective sense, the notion of catastrophe may be literally intolerable to us, and we have always fled from it the moment we realize its implications.

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¹⁹²²WIC, *op. cit.*, p. 124.

¹⁹²³Parry, *op. cit.*, p. 15.

¹⁹²⁴*Ibid.*

¹⁹²⁵*Ibid.*, p. 3.

¹⁹²⁶*Ibid.*, p. 17.

¹⁹²⁷*Ibid.*

I must at this point acknowledge a voice of dissent. My colleague Lynn Rose, one of the contributors to and editors of this book, does not agree with the emphasis which was placed upon the words of Plato above. Rose is a professor of philosophy, and in my opinion a very thorough, painstaking scholar who expresses his opinions with reserve, and only after much careful research. His recent discovery in ancient history, where he offers strong proof¹⁹²⁸ that the conventional date for the Egyptian 12th dynasty (C.-1800) *must be moved forward* almost 1500 years, (*i.e.*, to about -370), is I think a landmark in the revision of Egyptian chronology and will, along with the work of fellow catastrophist historians, propel the field of ancient history willy-nilly into a new paradigm which favors Velikovsky. That is to say, he demonstrates with meticulous evidence that what was considered unthinkable in 1950 (Velikovsky's drastic redating forward of ancient chronologies) is much more probable today than the received opinions of that time. What he says, therefore, deserves the most respectful attention.

It is Rose's belief that we must not simply take Plato's dialogues at face value as expressing his own beliefs. Plato, he says, is a dramatist and a poet, and one cannot attribute to an *author* every opinion voiced by any of his characters. I agree, for I have seen literary critics foolishly insist that the words of the poor Duke of Gloucester in *King Lear*:

"As flies to wanton boys are we to th' gods— They kill us for their sport."¹⁹²⁹

are the deepest thoughts of Shakespeare himself, whereas the play, *as a whole*, demonstrates a very different outlook.

Rose, in his book *Aristotle in Amnesia*, argues similarly that Plato "is almost as inaccessible."¹⁹³⁰ The dialogues, says Rose, are *plays* to be taken with a good deal of salt, for their purpose may not be to simply act as mouthpieces for Plato about a certain position, but perhaps "to provide the readers with a raging debate about that position"¹⁹³¹ in order "to get them to think for themselves."¹⁹³² There is consequently, says Rose, a second form of irony in the dialogues, beyond the well-known Socratic (self-deprecating) irony. Rose calls it Platonic irony, which occurs "in the form of the many positions and arguments that he presents for his readers' consideration, but that he might not really have wished to defend at all."¹⁹³³ It is therefore wrong, says Rose, to accept without question, for example, that characters like the Athenian Stranger in the *Laws* must be "spokesmen for the views of Plato."¹⁹³⁴ He does backtrack by admitting that "Perhaps *some* of what they say is Plato's own view; but . . . None of these things should be just taken for granted"¹⁹³⁵ because that is not the form in which they are presented. "If Plato had wanted all of his writings to be taken literally," says Rose, "then presumably he would have written essays and monographs, like Aristotle."¹⁹³⁶

¹⁹²⁸*Journal of Near Eastern Studies*, 53.4, pp. 237-261.

¹⁹²⁹William Shakespeare, *King Lear*, Act 4, Scene 1 in *The Riverside Shakespeare*, G. B. Evans, General ed., (Boston, Mass., 1974) pp. 37-38.

¹⁹³⁰Lynn E. Rose, *Aristotle in Amnesia*, (unpublished manuscript, p. 2).

¹⁹³¹*Ibid.*, pp. 3-4.

¹⁹³²*Ibid.*, p. 3.

¹⁹³³*Ibid.*, p. 4.

¹⁹³⁴*Ibid.*, p. 3.

¹⁹³⁵*Ibid.*

¹⁹³⁶*Ibid.*, p. 5.

With regard to the point at issue here, Rose feels that Stecchini may have misrepresented the *Laws* in *The Velikovsky Affair*, that Parry "repeats every one of Stecchini's errors"¹⁹³⁷ and that the *Epinomis* from which Stecchini quotes may not have been written by Plato but by his student Philip of Opus, who added it to the *Laws* after Plato's death. Rose's objective is to refute the belief that Plato was not a catastrophist but only a step "along the way to the explicit and unmistakable astronomical uniformitarianism of Aristotle."¹⁹³⁸ On the contrary, insists Rose, everything Plato wrote "is consistently along catastrophic lines."¹⁹³⁹

Charles Ginenthal, Lynn Rose and I have debated this matter several times among ourselves, especially at organizational meetings when we were planning the Velikovsky Centennial Conference, (held in New York, July, 1995). Ginenthal takes a position opposed to Rose's. Following the approach of historian of science George Sarton, he sees no inconsistency in the picture of Plato as a rigid and fearful elitist who would like to force the unruly populace to think what he wants them to think, at any cost, especially about the cosmos, for Ginenthal contends that a belief in the *present order* of the heavens is as essential to Plato's philosophy as it is to Aristotle's, no matter how much they may differ between themselves about the *past*. My professional training inclines me to acknowledge Rose's point in principle, but in practice I find that Plato displays ambivalent tendencies: he seeks a perfect universe of purpose, structure and motion while at the same time admitting that the heavens do occasionally change and cause enormous destruction. The issue, therefore, deserves further study, and it may require some modification of this section in the future.

Demonization

Now back to the point I was making, that we tend to flee from the idea that the cosmos may be disorderly. There is a darker side to this hysterical pattern which I have heretofore only pointed out in passing, but which we will look at more closely now because it explains the reaction from age to age not merely to the *idea* of catastrophism, but to the *individual* who upholds it. Cicero at the time of Caesar "denied the possibility of the planets changing their courses" and declared that this truth "is so manifest that I can scarcely deem one who denies it to be of sound mind."¹⁹⁴⁰ Lyell and his fellow British uniformitarians viciously attacked the catastrophists of their time, presenting their opponents as victims of malevolent delusion while the victory of their own theory was "a triumph for the forces of light . . . over the powers of darkness."¹⁹⁴¹ Newton, who at first had supported his pupil William Whiston's *New Theory of the Earth*, 1694, turned savagely on him a decade later, blocking his admission to the Royal Society and helping to have Whiston dismissed as a teacher at Cambridge. "Scholars," (Stecchini tells us), "have failed to notice that the refutation of Whiston's doctrine was of major concern to Newton"¹⁹⁴² because Whiston upheld cometary impacts and near approaches, the Deluge, a shorter year and random cosmic events, whereas Newton was concerned to prove that nature is providential and that "the world is stable and has remained unchanged since creation."¹⁹⁴³

What we see in each of these instances is a process of demonization in which the enemies of uniformity, the catastrophists, are said not merely to be wrong but to be demented and/or evil, and that leads directly to the treatment of Velikovsky by the ASG. It seems that almost every time someone influential advocates catastrophism, he must be not merely refuted but destroyed. As Princeton historian of science

¹⁹³⁷*Ibid.*, p. 8.

¹⁹³⁸*Ibid.*, p. 10.

¹⁹³⁹*Ibid.*, pp. 7-8.

¹⁹⁴⁰Quoted by Velikovsky, *MIA*, *op. cit.*, pp. 59-60.

¹⁹⁴¹Hallam, *op. cit.*, p. 23.

¹⁹⁴²Stecchini, *op. cit.*, p. 93.

¹⁹⁴³*Ibid.*

Livio Stecchini pointed out in *The Velikovsky Affair*, there were several times in our culture when the doctrine of the stability of the heavens was seriously questioned, but in each instance, by means of threat, suppression, misrepresentation or bodily destruction, a consensus was finally shaped to support the dogma and rule out the opposition. Nicolas of Cusa, he tells us, "denied the qualitative difference between heaven and earth" in the 15th century.

"He claimed that heavenly motions do not have stability as an inherent quality, and formulated the hypothesis that some statements of ancient writers may be explained by their having seen a sky different from what was seen in his time."¹⁹⁴⁴

Nicolas did not meet great opposition because the Church was firmly in control of thought, but the same was not true for Giordano Bruno, who

". . . denied the existence of a providential order in nature and hence of the stability of the solar system which is linked with the doctrine of circular movements."¹⁹⁴⁵

There is no absolute regularity of heavenly motion, he said, and no absolute time, and no proof that our solar system is the only set of worlds, for an infinite number may exist, of which ours is just one. By that time, the Church was under siege from the ideas of Copernicus, Galileo and Brahe and could not afford to be tolerant. Bruno was carted to the Vatican and, after seven years, when he would not recant, he was burned alive in Rome in 1600. If uniformitarianism had to see the world as unchanging, benevolent and moral, then those who opposed it *could* be demonized and destroyed because they supported (*i.e.*, were the servants of) an evil vision.

It is a pattern not wholly unfamiliar to the 20th century, when the Nazis with their preferred "Aryan" ideology felt they had a sacred duty to destroy both the hated Jews and their more hated books. Every reigning dogma has always wanted to annihilate its opposition, and this has been especially true of uniformitarianism. Three final anecdotes will serve to illustrate the historical extent and manic durability of this blind rage both at the idea and at the propounders of catastrophism. The first comes from the 20th century and the Velikovsky Affair, when the original publisher Macmillan was not only forced to give up publishing its best-selling book but was *told to destroy all its remaining copies on hand* which, of course, is what the Nazis had done to undesirable books less than 20 years before. The second comes from classical Greece, where "Plato declares that the most dangerous and subversive doctrinarians are those who deny the eternal regularity of the heavenly bodies."¹⁹⁴⁶ To Plato, morality cannot exist without heavenly order and it is wrong of people like Democritus to argue that all of existence has been created and runs by chance. For people like this

¹⁹⁴⁴*Ibid.*, p. 84.

¹⁹⁴⁵*Ibid.*, p. 85.

¹⁹⁴⁶*Ibid.*, p. 154.

"Plato recommends . . . that they be imprisoned for five years in a House of Better Judgement to be brainwashed and that, if they do not change their minds within that period, they be put to death."¹⁹⁴⁷

The third concerns Aristarchos of Samos, whose life was similarly placed in danger a century later for implying that the Solar System was not safe. When he dared to argue *that the Earth follows an orbit*, the Stoic Cleanthos of Assos responded that, "For moving the hearth of the universe," (*i.e.*, for denying that the Earth was the fixed center of cosmic motion), he ought to be charged with impiety, a crime punishable by death.¹⁹⁴⁸

Here is the naked impulse behind the Velikovsky Affair – whenever the feared, hated, horrible truth about the heavens is spoken, attack the idea, destroy the book and, if you cannot destroy the man, then ruin his reputation. Demonize him. It is vile and it is vicious, but it is a truth about humankind. We have run from the past and its message of catastrophe ever since we first undertook organized speculative philosophical thought about the heavens.

New Evidence

What seems to lie behind this continual demonization is a gross over-valuation of the effect upon nature of what we believe. We actually think that what we think determines reality. The universe, however, has a mind of its own and impudently does what Nature wants, and slowly mainstream cosmology will have to learn to see that, but it is not easy because there is a shield of desire-laden scientific myth to be penetrated. Knights in armor trying to breach that wall include the British astronomers Victor Clube and Bill Napier, who offer a different model from Velikovsky, but it is also a very different picture of our Solar System from the placid fairy tale of the ASG. Remember, the Shapley-Gaposchkin portrait (see earlier) speaks of a solar system almost perfectly isolated from all intrusion and change (just like Aristotle's structure of spheres). In contrast, Clube and Napier tell us in *The Cosmic Serpent* that "we are in a solar system that somehow evolves, and the solar system is itself immersed in an ever-changing galaxy,"¹⁹⁴⁹ and galaxies themselves do not remain static. They do not "hang forever in empty space just as we observe them In fact, the whole display is a maelstrom of motion,"¹⁹⁵⁰ and this continues to be true as far as we can see out into the distant universe. Nothing anywhere in the cosmos remains fixed, and consequently the "perfect cosmological principle" is untrue because the entire "universe is evolving . . . there is unending change on the largest scale."¹⁹⁵¹

This omnipresence of change involves constant cosmic violence, they say, for "both nearby and remote galaxies periodically deviate from quiet to brief energetic states,"¹⁹⁵² as in gravitationally-collapsing, rapidly-rotating, condensing stars or superstars. In addition, "temperamental" or "periodically violent" galactic nuclei can become "monsters" and blow up,¹⁹⁵³ and, as a result, there are a great number of "galaxies

¹⁹⁴⁷*Ibid.*, p. 155.

¹⁹⁴⁸George Sarton, *Hellenistic Science and Culture in the Last Three Centuries B.C.*, (Dover ed.) (New York, NY, 1987), p. 59.

¹⁹⁴⁹Victor Clube, Bill Napier, *The Cosmic Serpent: A Catastrophist View of Earth History*, (New York, 1982), p. 15.

¹⁹⁵⁰*Ibid.*, p. 16.

¹⁹⁵¹*Ibid.*, p. 18.

¹⁹⁵²*Ibid.*

¹⁹⁵³*Ibid.*, p. 23.

in whose centres very violent things are going on."¹⁹⁵⁴ Focussing nearer to us, we are told that the arms of spiral galaxies indicate "quite rapid if not violent evolutionary processes" and our Solar System in particular is very "close to a major irregularity" of this type.¹⁹⁵⁵ These spiral arms contain billions of interstellar comets, and if these structures near our Solar System are "rapidly evolving,"¹⁹⁵⁶ then it is inevitable that "the solar system interacts with such material."¹⁹⁵⁷ "Acting as a large gravitational scoop," the Solar System could capture many of these objects, which could lead to "episodes of planetary bombardment"¹⁹⁵⁸ and ignite catastrophic "terrestrial processes."¹⁹⁵⁹ In their opinion, therefore, the strong possibility of the presence of nearby temporary fluxes or clouds of comets and of subsequent capture or near-approach events means that our Solar System has not "been running smoothly" for 4.5 billion years as is traditionally believed,¹⁹⁶⁰ and that the "classical quiescent picture" which mainstream astronomy has advanced¹⁹⁶¹ is quite wrong, for the latest evidence indicates that "our Galaxy is in a recently disturbed state."¹⁹⁶² The reader must understand that Clube and Napier are not disputing the "long-lasting regularity and order" of the observable heavens, but, whereas the ASG interpreted this to mean that there can be no disorder in our Solar System, Clube and Napier insist that regularity over the long term is perfectly consistent with episodic short-term deviation. Catastrophes are commensurate with the evidence.

They then bring the realm of possible disorder firmly down to the Earth. Asteroids on Earth-crossing orbits constitute "a potential collision hazard," while the frequent cratering which has been discovered on every planet and moon speaks of continual collisions with comets, fireballs, meteorites and their fragments, producing impacts ranging from the very large cometary event that is said by Walter Alvarez to have ended the Cretaceous Period 65 million years ago, to the much smaller Tunguska explosion of a presumed meteorite fragment over Siberia in 1908. These impacts "cannot fail to have catastrophic consequences,"¹⁹⁶³ which lead Clube and Napier to "a neo-catastrophist view of Earth history"¹⁹⁶⁴ which is a continuation of what several astronomers before them had advanced, going back to Laplace in 1806, but it had always been ignored. They review the evidence for catastrophism on Earth (geological shifts, drifting of continents, unrecognized giant craters) which cannot be attributed to internal (*i.e.*, purely terrestrial) causes, for the record of the impacts (shock waves, heat, pressure, hot ejecta, ash, vapor, dust, earthquakes, tsunamis, extinctions and a depleted ozone layer leading to ultraviolet radiation exposure) cannot be passed off as the "usual" effects of "normal" local perturbations. Flipped geomagnetic fields, episodes of tectonic drift, sea floor spreading, rapid mountain building and severe climatic changes call for non-gentle, non-uniform causes.

To these two astronomers, catastrophes initiated by non-terrestrial intrusions are the only reasonable solution. "The fossil record is hardly Darwinian," they say, ¹⁹⁶⁵for evolution is erratic, containing "numerous brief episodes of mass extinction"¹⁹⁶⁶ and "rapid proliferation"¹⁹⁶⁷ which call for catastrophism "to explain

¹⁹⁵⁴*Ibid.*, p. 29.

¹⁹⁵⁵*Ibid.*, p. 32.

¹⁹⁵⁶*Ibid.*, p. 33.

¹⁹⁵⁷*Ibid.*, p. 40.

¹⁹⁵⁸*Ibid.*, p. 35.

¹⁹⁵⁹*Ibid.*, p. 33.

¹⁹⁶⁰*Ibid.*, p. 59.

¹⁹⁶¹*Ibid.*, p. 71.

¹⁹⁶²*Ibid.*, p. 41.

¹⁹⁶³*Ibid.*, p. 77.

¹⁹⁶⁴*Ibid.*, p. 92.

¹⁹⁶⁵*Ibid.*, p. 103.

¹⁹⁶⁶*Ibid.*

their suddenness, ubiquity and intensity." That is to say, if these events were abrupt and instantaneous, as the evidence indicates, then "catastrophic circumstances must be invoked."¹⁹⁶⁸ They, therefore, accept that Cuvier and Darwin are both correct, *i.e.*, that the Earth is most often stable but is occasionally disturbed, which means that strict uniformitarianism, the theory which allows only "familiar processes," is "a fallacy."¹⁹⁶⁹ As a result, they assert that it is against all the evidence to have "the evolution of the Earth discussed as if the planet existed in isolation,"¹⁹⁷⁰ as the ASG believed. What is required in its place is the recognition that the Earth "suffers drastic, sudden and irreversible upheaval from time to time"¹⁹⁷¹ and, therefore, that "catastrophism is uniformitarianism,"¹⁹⁷² which is what Velikovsky also emphasized in *Worlds in Collision*— "Cosmic collisions are . . . implicit in the dynamics of the universe." They are as normal as periods of quiet.

This is not to be considered a proof for catastrophism, but it is certainly a plea for its objective consideration, which Velikovsky deserves but has hardly ever had. If the continualist view of Shapley-Sagan-Asimov-Kurtz is incorrect, and if the periodicist concept of the catastrophists is closer to the truth, it means that catastrophes of some sort or magnitude are continually possible and, therefore, that there is no intrinsic reason why Velikovsky or any catastrophist should be wrong *à priori*. This does not mean to say that Velikovsky *must be right*, but certainly he or any catastrophist now *can* be right. Each separate catastrophic theory will, therefore, have to be evaluated on its merits, as it always should have been, and not by categorical *pronunciamentos* beforehand, as Velikovsky's was. The arrogant disallowing of catastrophe in advance, on absolutist grounds, without consideration of the evidence, is no longer possible.

Two recent books offer important confirmations of our approach to the topic. In *Time's Arrow, Time's Cycle*, (1987), Stephen Jay Gould analyzes the concealed psychological processes behind the victory of uniformitarianism in the 19th century. Gould is a professed anti-Velikovskian, yet his evaluation of the history of geology greatly aids Velikovsky. To start with, Gould is at pains to debunk the classical legend of "objective science" whereby "facts . . . lead by a sort of brute-force induction to grand theories," seeming to make science "the ultimate tale of progress."¹⁹⁷³ In opposition, he argues firstly that "scientists are not robotic inducing machines" but "human beings, immersed in culture" who can never get beyond it,¹⁹⁷⁴ "so we must make the best of our ineluctable embedding."¹⁹⁷⁵ Second, he insists that "great theories . . . are not simple inductions from observed facts,"¹⁹⁷⁶ but that all major scientists have personal visions of the natural world which "stand prior . . . to their attempts at empirical support."¹⁹⁷⁷ Science, therefore, is never "pure observation and applied logic, divorced from realities of human creativity and social context," but is always culturally conditioned and privately influenced, and, therefore, it will "only be harmed in the long run by its self-proclaimed separation as a priesthood guarding a sacred rite called *the* scientific method."¹⁹⁷⁸

Gould then interprets the vicissitudes of Charles Lyell's "scientific" opinions in this light. It might surprise readers to learn that Lyell, the "father of modern science," strenuously denied evolution, the "centerpiece of biological science," from the 1830's, when his *Principles of Geology* ruled the field, to the 1850's,

¹⁹⁶⁷*Ibid.*, p. 104.

¹⁹⁶⁸*Ibid.*, p. 105.

¹⁹⁶⁹*Ibid.*, p. 129.

¹⁹⁷⁰*Ibid.*, p. 92.

¹⁹⁷¹*Ibid.*, p. 129.

¹⁹⁷²*Ibid.*, p. 94.

¹⁹⁷³Gould, *op. cit.*, p. 5.

¹⁹⁷⁴*Ibid.*, p. 6.

¹⁹⁷⁵*Ibid.*, p. 7.

¹⁹⁷⁶*Ibid.*, p. 9.

¹⁹⁷⁷*Ibid.*, p. 10.

¹⁹⁷⁸*Ibid.*, p. 7.

when debates with Darwin forced him to modify his opinion. The ninth edition of his book, in 1853, contained no submission, but the tenth, delayed until 1866, was "the first to announce his retreat." Up to then, Lyell had resolutely rejected the evolution of humans, whom he said (how preposterous it sounds) were "God's addition of the last moment."¹⁹⁷⁹ He finally surrendered, "albeit with great reluctance" and in "a painful process,"¹⁹⁸⁰ and he naturally tried his best to minimize his "capitulation,"¹⁹⁸¹ but Gould insists that, when he gave in, it was a momentous defeat, for he "surrendered both his vision and all its sequelae."¹⁹⁸² What occurred in this moment of anguish was a spiritual negotiation, not a scientific decision, says Gould, in which the core of Lyell's belief, "his linchpin of nonprogressionism,"¹⁹⁸³ "this anchor of his central vision," was traded away "because it permitted him to preserve all other meanings of uniformity."¹⁹⁸⁴ That is to say, despite "his supreme reluctance to place human origins into nature's ordinary course," (how odd) and despite having to accept "(most distasteful of all) perhaps even periods of mass extinction with subsequent recreation,"¹⁹⁸⁵ Lyell gave in because "he viewed evolution as a more acceptable explanation . . . than old-fashioned progressionism," (*i.e.*, change of state). To Gould, therefore, evolution for Lyell was not a scientific advance dictated rigorously by new evidence, but merely "the fallback position of minimal retreat from the rest of uniformity,"¹⁹⁸⁶ by which he could still retain "uniformity of rate" and "uniformity of law" over variations in intensity, and actualism over progressionism and special First Causes, which permitted him to "relinquish only one of the uniformities . . . better one room than the entire edifice."¹⁹⁸⁷ The reason for Lyell's distress (and distaste) is not difficult for us to perceive. Gould says Lyell accepted evolution as "the most conservative intellectual option available to him," but we may put it more pointedly: even though it caused Lyell great "pain and trouble of mind,"¹⁹⁸⁸ *it continued to get rid of catastrophism*. There was nothing scientific about his choice, it was all preference.

Gould calls Lyell's book "the most important, the most influential, and surely the most beautifully-crafted work of nineteenth-century geology,"¹⁹⁸⁹ but, if we analyze this evaluation critically, we soon perceive that it was the book's craftsmanship rather than its evidence which made it important and influential. "I recognize a baleful influence of his rhetoric," says Gould, which he admits has had a very counter-productive effect on science, for, when geologists "succumb to Lyell's rhetorical device," the result too often is "reasonable hypotheses of catastrophic change, rejected out of hand by a false logic that brands them unscientific in principle."¹⁹⁹⁰ (Ironically, he does not notice that this is precisely how mainstream science used Lyell's ideas against Velikovsky). Lyell's fury at William Whiston for having "dared to promote comets" is offered as a specific "example of how Lyell's rhetorical confusion might stifle legitimate research."¹⁹⁹¹ What Gould is referring to here is the modern debate about the comet-impact explanation for the immense catastrophe which occurred at the boundary between the Cretaceous and Tertiary periods 65 million years ago. In his opinion "the catastrophists were right Lyell's gradualism had acted as a set of

¹⁹⁷⁹*Ibid.*, p. 168.

¹⁹⁸⁰*Ibid.*

¹⁹⁸¹*Ibid.*

¹⁹⁸²*Ibid.*, p. 109.

¹⁹⁸³*Ibid.*, p. 170.

¹⁹⁸⁴*Ibid.*

¹⁹⁸⁵*Ibid.*, p. 171.

¹⁹⁸⁶*Ibid.*, p. 172.

¹⁹⁸⁷*Ibid.*, p. 173.

¹⁹⁸⁸*Ibid.*

¹⁹⁸⁹*Ibid.*, p. 177.

¹⁹⁹⁰*Ibid.*, p. 176.

¹⁹⁹¹*Ibid.*

blindness, channeling hypotheses in one direction.¹⁹⁹² It is, therefore, Gould's estimate that "For more than a century, many geologists have been stifled . . . by an *à priori* commitment to gradual change,¹⁹⁹³ but "current views represent a pretty evenly shuffled deck between attitudes held by Lyell and the catastrophists" and, therefore, "our current allegiance does not mark Lyell's particular triumph"¹⁹⁹⁴ — "our modern understanding is not his."¹⁹⁹⁵ Due to his great persuasiveness, says Gould, (which was his major asset), Lyell's "bias" had placed "a conceptual lock . . . upon the science of paleontology,"¹⁹⁹⁶ "but we have been compelled to balance his dichotomy" and to recognize that catastrophism and uniformitarianism "both capture important aspects of reality."¹⁹⁹⁷

In recent years, however, the situation has begun to change, for in no major scientific field is pure absolute uniformitarianism upheld by everyone any longer. What is emerging, slowly but steadily, is *neocatastrophism*, which Henry Zemel and Bill Mullen prefer to call *cenocatastrophism* or the science of *recent* catastrophic events. That is particularly true in geology, as witnessed by such books as Derek Ager's *The New Catastrophism: The Role of Rare Events in Earth History*, (1993), Claude C. Albritton's *Catastrophic Episodes in Earth History*, (1989), and especially Richard Huggett's *Catastrophism and Earth History*, (1992). Huggett is by no means a Velikovsky supporter, but he is concerned to initiate "a fresh start on general issues about the history of the Earth"¹⁹⁹⁸ which will include catastrophism. He delineates many kinds of catastrophes, some which cause sudden change within a system, some which cause change to the system, some which exhibit gradual stages which lead to sudden change when the system is forced over a threshold, and some where the changes can be catastrophic but not violent. Unstable systems may dissolve, quasi-stable systems will resist up to a point and then flip and regain equilibrium, and catastrophes can be global, continental or local, but all systems are "subject to catastrophic change" for "catastrophes may occur at all scales."¹⁹⁹⁹ Huggett plays about with the various words different geologists have used to describe the phenomenon, (cataclysm, calamity, sudden turn, discontinuity, paroxysm, episodic, random, non-linear and punctuation), but he still prefers "catastrophe," although he dislikes the terms micro-, meso-, macro- and mega-catastrophes. Within this range, dozens and dozens of catastrophist theorists are mentioned, and among these Velikovsky is labelled an extreme catastrophist like Cuvier, in that he calls for sudden violent change to an entire ecological system, the Earth itself.

The problem facing catastrophism, as Huggett sees it, is that most geologists even today will not renounce uniformitarianism. That is to say, even though "many geoscientists now accept that extreme events of great magnitude do occur," few agree with the "suddenness and violence . . . envisaged by the old catastrophists."

"This disinclination to embrace truly catastrophic events arises more from misconceptions about, and an irrational and emotional antagonism towards, catastrophism, than from the impossibility of continental and global catastrophes having occurred. Anti-catastrophist feelings run deep."²⁰⁰⁰

¹⁹⁹²*Ibid.*

¹⁹⁹³*Ibid.*, p. 174.

¹⁹⁹⁴*Ibid.*, p. 172.

¹⁹⁹⁵*Ibid.*, p. 178.

¹⁹⁹⁶*Ibid.*, p. 179.

¹⁹⁹⁷*Ibid.*, p. 178.

¹⁹⁹⁸Richard Huggett, *Cataclysm and Earth History*, (Oxford, England, 1989), p. viii.

¹⁹⁹⁹*Ibid.*, p. 9.

²⁰⁰⁰*Ibid.*, p. 18.

He refers to "the antiquity and venerable nature of catastrophist discourse" which "has been the subject of lively debate . . . since the seventeenth century," yet, even though the disputes are now familiar only to "historians of geology," he cautions that "To view them as historical curiosities is incredibly short-sighted."²⁰⁰¹ To Huggett "the catastrophists have been given very short shrift by the modern, blinkered uniformitarians" whose depictions of them have been "scathing and erroneous," presenting the debate as "a battle between good and evil: catastrophism is black, uniformitarianism is white It is very narrow-minded."²⁰⁰² Catastrophic ideas "have not been paid the attention they deserve," he insists,²⁰⁰³ and the villain of course is well-known: "ever since Lyell insinuated uniformitarianism into geology . . . the old ideas have received grossly unfair treatment," which is "a very misleading portrayal."²⁰⁰⁴

Huggett cannot accept the old catastrophism in total, and says that the new catastrophism is lesser in extent, but "both claim that biological and geological rate and state are non-uniform . . . they both stand in antithesis to uniformitarian systems of Earth history."²⁰⁰⁵ He, therefore, calls for "a new hearing to provide a rationale for exploring a truly neocatastrophist methodology in studying the history of the inorganic and organic worlds."²⁰⁰⁶ In his opinion, however, this methodology will not produce an all-or-nothing winner in the combat between uniformity and catastrophe, for Huggett argues that, (as Velikovsky had said all along)

". . . there is no longer a clearly-defined distinction between them They have been replaced by the concept of an evolving universe in which erratic changes . . . take place at greatly fluctuating rates."²⁰⁰⁷

What is significant for us, therefore, is how Huggett's views indicate the extent to which Velikovsky's absolutist detractors have been disproved and he has been proved since the appearance of his first book. In 1950, the catastrophist ideas of Velikovsky were treated with ridicule, incredulity and scorn, but

"In 1990 catastrophism is no longer the dirty word that it has been for the last century and a half. You no longer run the risk of being labelled a crank and disowned by the scientific establishment on joining the Catastrophist Club."²⁰⁰⁸

I have especially stressed the changing attitudes to uniformitarianism because its (pernicious) influence has extended far beyond geology. When Lyell in the mid-18th century engaged in his vigorous proselytizing of the concept, he insisted even then that this was not merely a characteristic of geology but a universal principle, that all processes everywhere in nature were slow and minute. By 1835, he had convinced most people of it, especially in England, and, when seemingly contradictory data began to appear

²⁰⁰¹*Ibid.*, p. 19.

²⁰⁰²*Ibid.*, p. 20.

²⁰⁰³*Ibid.*, p. vii.

²⁰⁰⁴*Ibid.*, p. 19.

²⁰⁰⁵*Ibid.*, p. vii.

²⁰⁰⁶*Ibid.*, p. 21.

²⁰⁰⁷*Ibid.*

²⁰⁰⁸*Ibid.*, p. vii.

in paleontology and the fossil record and the theory of ice ages, even these were soon incorporated. By the 1870's, uniformitarianism had swallowed geology, astronomy, human history and meteorology into one dream of gradualist tranquility. It had its enemies, for the great physicist Lord Kelvin was an intractable opponent, and William Whewell, "a formidable antagonist of uniformitarian geology,"²⁰⁰⁹ wrote as late as 1872 that it was too soon to decide between catastrophism and uniformitarianism and actually called for the formation of "a subsience entitled 'geological knowledge of facts.'²⁰¹⁰ They, however, were, and unfortunately have remained, in the minority while the majority of geologists have stoutly approved catastrophism up to the present. "Few modern scholars," wrote American geologist G. G. Simpson in the 1930's, "profess to see much that was very good in catastrophist views of the eighteenth and nineteenth centuries."²⁰¹¹

The problem is that uniformitarianism by the early 19th century had become a faith which dictated in advance what could or could not be true. Geologist K. Prestwich wrote in 1895 that uniformitarianism had by then come to be seen as "an infallible faith,"²⁰¹² geologist W. Hubbert observed in the 1930's that the concept that "the laws of nature are invariant with time . . . has now become the common denominator of all science,"²⁰¹³ and Dutch geologist Reijer Hooykaas said in 1975 that "Some uniformitarians . . . made Uniformity into a kind of religious dogma."²⁰¹⁴ With this attitude came the normal appendages of any fanaticism, demonization of the opposition in conjunction with the assertion of one's own infallibility. Hooykaas dryly summarizes the negative dimension:

"In the 18th century darkness reigned until, through Hutton, suddenly all became light. In the beginning of the 19th century Cuvier, Buckland, c.s., fell back again upon deluges and catastrophes, until Lyell dispelled the clouds and definitively established uniformitarian orthodoxy."²⁰¹⁵

The self-glorifying dimension of uniformitarianism took the form of dictatorial and universalist *à priori* pronouncements upon what science was permitted to discover, where truth was decreed in accord with principle. Hooykaas tells us quite plainly that in the science of geology *ex cathedra* has reigned over evidence up to the present, that "Uniformitarianism tended to interpret data in conformity with the assumption of the immutability . . . of all geological causes"²⁰¹⁶ and as a result, even today "strict adherence to uniformitarian tenets tends to force past phenomena into a preconceived frame."²⁰¹⁷ Hooykaas' description of the catastrophist viewpoint, in contrast, is wholly non-demonic. He supports "actualistic catastrophism," which he considers "superior to Lyellian uniformitarianism because it proceeded from observation to theory without imposing limits on the rate of past changes *à priori*."²⁰¹⁸ One might consider it ironic that, in the allegedly objective domain of geological science, those who pleaded for objectivity over opinion—"Catastrophists held that the interpretation ought to be adapted to the data"—are the ones whose point of

²⁰⁰⁹Claude C. Albritton, Jr., ed., *Philosophy of Geological History*, (Strondbury, Penn., 1975), p. 8.

²⁰¹⁰*Ibid.*, p. 3.

²⁰¹¹*Ibid.*, p. 6.

²⁰¹²*Ibid.*, p. 353.

²⁰¹³*Ibid.*, p. 5.

²⁰¹⁴*Ibid.*, p. 348.

²⁰¹⁵*Ibid.*, p. 311.

²⁰¹⁶*Ibid.*, p. 356.

²⁰¹⁷*Ibid.*, p. 6.

²⁰¹⁸*Ibid.*, p. 6.

view has been regularly dismissed, but, when the plea is made on behalf of a concept which runs directly counter to the prevailing dogma, the voice of reason is unheard and the faith continues unabated. As an example of blind clinging to orthodoxy, we read in Huggett about American geologist Lionel Hawkes, who said as late as 1958 that anything which departs from uniformitarianism is not "proper scientific investigation"²⁰¹⁹ and should be forbidden, and who predicted in the 1970's that, by the year 2005, uniformitarianism would be so utterly proved that no one would be able to contest it.²⁰²⁰

We must remember that this blind faith was only about 75 years old in 1950, when Velikovsky appeared, and that it had sailed along up until then on a current of undisturbed support, with the old regime demonized and the new idolized, the earlier absolutely wrong and the new absolutely right, and it felt it was climbing to perfection. What had occurred was a total evasion of the evidence, which had been legislated away by a choice of mind, and no clouds lurked on the horizon. Newton had Laplace, Hutton had Lyell, Darwin had Huxley, and these "bulldogs," (Darwin's term), these hit men, had polished the fantasy and given it a cachet which seemed impregnable until Velikovsky raised the old hated fears to life again, stronger than ever and more dangerous because more popular. That is why it has taken an additional half century for catastrophism to begin to make an impact (pun unintended) on mainstream science, but it is happening. Huggett, for example, who is not an old-fashioned catastrophist and looks with suspicion upon adherents of things like rapid pole shifts, nevertheless dispassionately mentions de Grazia, Velikovsky and Clube and accords several pages to Warlow. He hates it that the views of the earlier catastrophists were "handed down to us in a very partial manner"²⁰²¹ and insists on "the recognition of truly catastrophic processes as agents of global geological and geomorphological change,"²⁰²² processes which have to include "episodicity" as well as "internal triggers" and "the recognition of external (extraterrestrial) triggers,"²⁰²³ for "the evidence for sudden and violent events having occurred during the course of Earth history is strong."²⁰²⁴ When we add the evidence from fields like astronomy, stratigraphy and ancient history, a strong cross-disciplinary argument for catastrophism appears, but it has only advanced to the fringes of specialized debate. That it should take this long for these counter-indications to uniformitarianism to come to the surface in mainstream science illustrates that that concept is not simply an oppression in geology but has been a pernicious drag upon freedom of thought in every field of science.

After all, everything that we see is "punctuated" —no marriage runs at a constant pace, nor do the fortunes of any government or sporting team or institution or industry. There are continual downs and ups, for that's what it is to exist, and we have learned that it is the same throughout the universe, where stars are always being born or dying, galaxies erupt or collide or perish, jets of matter are expelled at close to luminous speed and immense pulses of energy scream through space, which is unimaginably noisy with the violence. Yet mainstream science (the alleged source of truth) tells us that there is one region in the entire universe free of all these vicissitudes. It is not outer space beyond the Solar System, nor local space within the Solar System, but our little Solar System itself, which, we are informed, is a unique and anomalous haven of order in all this tumult, a quiet clock ticking regularly and for always, even though it is surrounded by constant change.

One does not need the latest discoveries of astronomy to tell us that this foolish and wishful concept is a myth, a religious faith and not science. It is a relic of the past, a dinosaur vision of the ASG and its descendants which is fear-driven, desire-laden and self-delusive, but it has been and continues to be believed, and the cause is what I call the Ostrich Attitude. Whether it is true or not, ostriches are believed to respond to the fearful by burying their heads in the ground, in the belief that, if they no longer see the source of fear, it is

²⁰¹⁹*Ibid.*, p. 120.

²⁰²⁰*Ibid.*, p. 115.

²⁰²¹Huggett, *op. cit.*, p. 20.

²⁰²²*Ibid.*, p. 115.

²⁰²³*Ibid.*, pp. 15, 16.

²⁰²⁴*Ibid.*, p. 194.

not there. That in my opinion is what mainstream science has done since Newton, Lyell and Darwin—buried its head in uniformitarianism and believed that catastrophes do not occur. The danger with putting one's head in the earth, however, is that it exposes one's backside to a swift kick, and that I think is what nature has done to classical science. Nature has rudely turned up powerful contrary indications while classical science was dreaming its dream of order, and catastrophism has equally rudely called attention to this evidence while classical science slept. That's where the battle lines are drawn. There are important discoveries which point to catastrophism, but getting them recognized by mainstream science is still slow and niggardly.

A powerful example of the new discoveries is Ralph H. Abraham's ruthless demolition of the fiction of eternal cosmic peace in his 1994 book *Chaos, Gaia, Eros*. Abraham is a mathematician, professor emeritus at U. Cal. Santa Cruz, who has made a lifelong study of the possibility of solar-system stability and has decided that *it cannot be proved*. To him it is merely a dogma that we favor because "its very comforting."

"This dogma is built into modern science, as it is in most religions. Under the name *the dogma of stability*, it has hounded celestial mechanics from Sir Isaac Newton to Immanuel Velikovsky."²⁰²⁵

He reviews the history of those isolated men who have upheld the doctrine of catastrophe induced by extra-terrestrial bodies, whom he calls "cometophiles." These include Nicholas Cusanus in the 15th century, who rejected perfect planetary orbits, and Giordano Bruno in the late 16th century, who insisted that comets have closely approached the Earth, causing fear and religion. Bruno spoke of an infinite universe indifferent to human providence or our desire for solar system stability, in which planetary motions are irregular and unpredictable. As a result, he found himself in trouble with the ecclesiastical authorities because "the eternal stability of the solar system became official church dogma." There was a battle between anxiety and peace of mind, Abraham says in an echo of Velikovsky, for what the Church dogma provided was "freedom from worry," while "Bruno's attack . . . induced in people the maximum of anxiety and fear."²⁰²⁶ Bruno was accordingly burned to death by the Church.

Next, Abraham describes the bitter controversy that arose a hundred years later between Newton and his protégée Whiston, an interaction whose outlines are distressingly similar to the combat between Bruno and the Catholic Church. It begins in 1681, when Halley's Comet passed very near to the Earth. "The world actually came close to destruction with its passage," says Abraham, and the result was widespread fear. "Many people worried that the comet would collide with the Earth."²⁰²⁷ Shortly afterwards, Newton in 1687 said he proved that it could not, but Whiston in 1694 argued "the chaotic role of comets in the creation of the solar system," and we are told that "Halley secretly shared this view."²⁰²⁸ Abraham feels that at first Newton did too, albeit secretly, but later Newton reversed his opinion about comets and irregular motions and persecuted Halley. Most people today are unaware of these internecine battles, and therefore do not know that Newton, despite his popular veneration, had presented a falsehood, for Abraham insists that Newton *was never able to prove planetary stability*.

²⁰²⁵Ralph H. Abraham, *Chaos, Gaia, Eros*, (New York, 1994), p. 178.

²⁰²⁶*Ibid.*, p. 180.

²⁰²⁷*Ibid.*, p. 182.

²⁰²⁸*Ibid.*, p. 183.

"Newton's mathematical model, so far as it intends to be a model for the clockwork regularity of the universe, may not have much to do with the real universe."²⁰²⁹

It was a dogma, just like that of the Church's a hundred years earlier.

Newton's contemporary Leibnitz ridiculed him for saying he had proved cosmic order and then for having to introduce "divine intervention to maintain the order of the heavens."²⁰³⁰ The curious result of the Newton episode was a bifurcated outlook about the universe, poised between irreconcilable and mutually contradictory visions. To the general population, Newton had established the truth of heavenly order, but to the scientists (and especially the mathematicians) he had failed. The problem of doing this consequently became the major puzzle in astronomical mathematics.

Laplace attempted it a century after Newton, but he did not succeed either, (even though popular myth believed he had), for Laplace, Abraham insists, was actually a crypto-catastrophist. He felt that collective anxiety "about the Sun rising tomorrow, and the possibility of a collision with a comet, were very important in the evolution of society and culture, and wrote extensively about it."²⁰³¹ This argument is also made by Stecchini in *The Velikovsky Affair*. Abraham feels that Laplace (like Newton himself) "was unable to prove stability as a mathematical result of Newton's model; he accepted it purely as an article of faith."²⁰³² Stecchini feels on the contrary that Laplace never fully accepted it.

"Laplace . . . was cited throughout the nineteenth century . . . as having provided the mathematical proof that the solar system, and hence nature, is built like a mechanical clock. But this is only one side of his total view."²⁰³³

Elsewhere, Stecchini tells us, Laplace urged "that mankind should learn to accept without excessive fear the likelihood that a comet may strike the Earth," and that "the motions of the Earth are not unalterable, being subject to several unpredictable forces, among which is the impact of meteorites."²⁰³⁴ He admitted that one *could* infer from his formulae that "nature has arranged everything in the sky to insure the permanence of the planetary system' . . . but added that such a conclusion was wrong."²⁰³⁵ Laplace actually described the consequences of a cometary collision in words very much like Velikovsky's, and even wondered if magnetic or electric forces could play a role.²⁰³⁶ His attitude is firm: *Le ciel même, malgré l'ordre de ses mouvements n'est pas inaltérable.* (The heaven itself, despite the order of its movements, is not unalterable.)

The problem is that "Scientific literature never mentions the Laplace statements listed above." Instead, it focusses only on those utterances which can lead one to believe that the heavens cannot change:

²⁰²⁹*Ibid.*, p. 180.

²⁰³⁰*Ibid.*, p. 186.

²⁰³¹*Ibid.*, pp. 191-192.

²⁰³²*Ibid.*

²⁰³³Stecchini, *op. cit.*, p. 101.

²⁰³⁴*Ibid.*

²⁰³⁵*Ibid.*

²⁰³⁶*Ibid.*, p. 103.

"He won immediate fame for having provided the mathematical proof of the stability of the solar system that was missing in Newton, despite the fact that he had specifically warned against such an interpretation."²⁰³⁷ Again, therefore, we have in the misguided worship of Laplace the imposition of virtually religious dicta (what Stecchini calls "the theological assumptions of Newton,")²⁰³⁸ onto the allegedly pure rationalism of science, and again in order to permit a belief in the eternal stability of the heavens. Desire had triumphed once more over reality, and the phenomenon recurred a century later, in 1899, when Sweden's King Oscar II offered a substantial prize to anyone who could prove mathematically that the solar system *had* to be stable, (indicating that *until then it had not been proved*). The award was given to Poincaré, but Abraham's analysis indicates that Poincaré "managed to win . . . without actually proving that the solar system was stable,"²⁰³⁹ and he adds emphatically that since then *it still has never been properly proved*.

Now here we are, a hundred years since Poincaré, and the issue has arisen once again. This time, however, it is not proof which has the upper hand, but disproof, and the reaction of mainstream science has accordingly been different: whereas in the earlier instances it had lionized Newton, Laplace and Poincaré even though their proofs of planetary stability were false, now it attacked Velikovsky, even though his argument for planetary *instability* fully deserved honest investigation. To Abraham the cause of the vitriolic abuse of Velikovsky is evident: we are terrified at the thought of planetary instability and will not allow it to be discussed.

"One could have predicted that the academic world would react to his thesis with a most unscholarly fury, even personal vindictiveness."²⁰⁴⁰

Abraham's attitude is different and exemplary. He does not begin with a need to believe in heavenly stability, but measures Velikovsky objectively as a man who is to be valued for seeing "what other scholars were unable to see, because he relied on evidence they had chosen to neglect," and who is to be taken seriously because "a number of fundamental discoveries predicted by Velikovsky demonstrated the value of his method." Mainstream astronomy continues to resist Velikovsky nevertheless, says Abraham, because of a frightened obstinacy.

"The record shows that astronomers hold rigidly to a peculiar dogma, not much advanced from Laplace . . . or the biblical story of creation: that the solar system has remained essentially unchanged since it was created eons ago. Their assumption has of necessity predetermined the views of geologists and historical biologists. This dogma, basically of theological and not scientific nature . . . is grounded on fear. The dogma is groundless but the fear is real, and was the principal reason for a prolonged emotional outburst against Velikovsky."²⁰⁴¹

²⁰³⁷*Ibid.*

²⁰³⁸*Ibid.*, pp. 104-105.

²⁰³⁹Abraham, *op. cit.*, p. 193.

²⁰⁴⁰Stecchini, "The Inconstant Heavens," *op. cit.*, p. 78.

²⁰⁴¹*Ibid.*, p. 194.

Abraham believes in contrast that the Earth *is* in danger, (however small the mathematical likelihood of an event right now), that "there is the totally independent possibility of a close encounter with a comet," that the dogma of stability is a myth which comes "from Christianity or from its successor religion scientism,"²⁰⁴² and that Velikovsky has not been refuted.

"Through mathematics alone, one cannot contradict Velikovsky. The dogma of stability remains just an article of faith."²⁰⁴³

We may add, as it has been since Aristotle. The whole of Western culture throughout history has elevated this desire into dogma.

The Future

Finally, we will attempt to look into the future. What the new evidence and the new books have shown is that catastrophism is now becoming respectable, that, contrary to what almost every mainstream scientist might have expected in 1950, the criticism of catastrophism has weakened in the last half century while its support has strengthened. This may sometimes occur indirectly, as in Ginenthal's account of the very anti-Velikovskian astronomer David Morrison arguing for catastrophism just a few years ago while all the time continuing to deny Velikovsky's role or importance; but nevertheless, catastrophism has become a legitimate topic of current scientific discussion. Will it, however, especially in its "extreme" or Velikovskian form, ever become part of mainstream thought or mainstream science? Alfred de Grazia, in the original *Velikovsky Affair* (1966), had hoped that, after the exposure of scientific misbehavior concerning Velikovsky up to then, the Affair would soon end; but as this book has painfully shown, it did not. Now, after the professional weakness and self-destructive bias exhibited by Velikovsky's critics, after the new evidence, after the support offered by people like Gould, Hallam, Huggett and Clube, after everyone has seen the Shoemaker-Levy comets bursting into our corner of the universe and actually hitting a neighboring planet, will it end?

Perhaps it will, but there is a distance to go, as is evident from the analysis of mainstream science offered by British sociologist of science Barry Barnes. He speaks of "the myth of rationalism" which underlies classical science, the belief that science progresses in "gradually increasing correspondence with the reality it describes."²⁰⁴⁴ According to this myth, it is felt that bias, passion and irrationality are eliminated in science's "cumulative development" toward truth, and this faith in science "is generally believed in the way a myth is believed."²⁰⁴⁵ Barnes, in contrast, looking at science sociologically, sees it primarily as an instance of group commitment to a dogma and, therefore, does not ask if science is right or wrong, but seeks "to know how scientists become committed to their paradigms, and how that commitment is maintained."²⁰⁴⁶

"The key is to regard scientists as participants in a tradition of research
Paradigms then appear as the inherited knowledge of scientists: they are

²⁰⁴²*Ibid.*, p. 197.

²⁰⁴³*Ibid.*, p. 196.

²⁰⁴⁴Barry Barnes ed., *Sociology of Science*, (Harmondsworth, England, 1972), pp. 85-86.

²⁰⁴⁵*Ibid.*, p. 87.

²⁰⁴⁶*Ibid.*, p. 89.

accepted from the ancestors as the basis for research, developed and elaborated in the course of that research, and passed on in their developed and elaborated form as the accepted knowledge of the next generation . . . scientists accept them and become committed to them as the result of training and socialization, and the commitment is maintained by a developed system of social control."²⁰⁴⁷

This rigid control is achieved by a carefully manipulated process of indoctrination and ordination in which

". . . the formidable authority of teacher and text, backed by laboratory demonstrations carefully designed for pedagogic effectiveness, not only encourages the acquisition of current procedures and current interpretations but demands an exclusive concentration upon them."²⁰⁴⁸

The same observations are made by most sociologists who study how science *functions*. W. O. Hagstrom, for instance, describes the strong relationship between conformity and reward:

"The prolonged and intensive socialization scientists experience is reinforced and complemented by their practice of the exchange of information for recognition. The socialization experience produces scientists who are strongly committed to the values of science."²⁰⁴⁹

The result of this Pavlovian conditioning is the powerful influence of dogma upon behavior.

"The desire to obtain social recognition induces the scientist to conform to social norms He will tend to select problems the solution of which will result in greater recognition, and he will tend to select methods that will make his work acceptable."²⁰⁵⁰

He will, of course, also tend to fear and hate anyone (especially an outsider) who tackles unapproved problems or who uses unapproved methods. Within this rigid world, moreover, form is as important as content.

²⁰⁴⁷*Ibid.*

²⁰⁴⁸*Ibid.*

²⁰⁴⁹*Ibid.*, p. 117.

²⁰⁵⁰*Ibid.*, pp. 110-111.

". . . in mathematics the style of a proof . . . is often considered as important to its merit as the truth of the theories proved. While there are technical reasons for this, there are distinctively social ones as well."²⁰⁵¹

Sanctions by the authorities in power keep the practitioners in line and also isolate them from undesired ideas—"works that deviate too far from the norm will be refused publication in scientific journals."²⁰⁵² The result is scientific ignorance and the loss of scientific freedom.

"Such an exercise of sanctions makes it impossible for the great mass of scientists to evaluate for themselves the importance and validity of the information presented. Delegating considerable power to a few authorities obviously infringes on the norms of independence."²⁰⁵³

This is precisely how Science in the form of Shapley and the Harvard led ASG behaved, and how the general body of scientists was kept uninformed of Velikovsky's merits and agitated about his deviance. Simpletons like Asimov have tried to downplay the Velikovsky Affair as an exaggeration or even a lie, but we see here that the evidence of sociology makes the Affair a certainty.

Michael Mulkey, to whom I also referred in the first chapter, makes very similar points about the coerciveness of scientific education. Before we look at what he says, however, it might be useful to detour for a moment to the more old-fashioned sociologist Robert Merton, whose description of science is (or was before Kuhn's work) the standard view in sociology. I do this because Mulkey will attempt to disprove Merton by reference to the Velikovsky Affair, but, curiously, Merton himself will give us data to validate The Affair. For instance, he speaks of science as not merely methods or knowledge but "a set of cultural values and mores governing the activities termed scientific"²⁰⁵⁴ (*i.e., science is an ethical activity*). These mores or norms constitute "The ethos of science"²⁰⁵⁵ which consists of "imperatives . . . reinforced by sanctions."²⁰⁵⁶ They are, in effect, a *faith* in which every committed scientist believes.

"The mores of science . . . are binding, not only because they are procedurally efficient, but because they are believed right and good. They are moral as well as technical prescriptions."²⁰⁵⁷

To deny these norms, says Merton, is, therefore, considered "a breach of faith,"²⁰⁵⁸ and he predicts that the result of such a desecration of "the scientific spirit," of "the moral consensus of scientists," would be righteous,

²⁰⁵¹*Ibid.*, pp. 110-113.

²⁰⁵²*Ibid.*, p. 113.

²⁰⁵³*Ibid.*

²⁰⁵⁴Michael Mulkey, "Cultural Growth in Science," in Barnes, *Sociology of Science, op. cit.*, p. 127.

²⁰⁵⁵*Ibid.*

²⁰⁵⁶*Ibid.*, p. 67.

²⁰⁵⁷*Ibid.*, pp. 67-68.

²⁰⁵⁸*Ibid.*, p. 68.

selfless, detached anger on the part of mainstream scientists, "a moral indignation directed toward contraventions of the ethos."²⁰⁵⁹ That is to say, Merton wrote before the Velikovsky Affair occurred, but he unwittingly foretells it: ". . . the indignant bystanders . . . respond with hostility and want to see . . . that behavior conforms to the rules." I, too, like Mulkay, feel that, in general, the Mertonian description of science is a myth, "that some of the Mertonian norms exist in science more as "institutional fictions,"²⁰⁶⁰ *i.e.*, that scientists do not *behave* as Merton says they do. Nevertheless, most scientists of 1950 *believed* in Merton, and, therefore, I feel that his picture of moral indignation is correct. If the norms of science are "right and good," (*i.e.*, what God wants), then betrayal of them, as Velikovsky was said by the powers-that-be to have done, would have elicited precisely the disinterested rage and insensitive brutality that has characterized the Velikovsky Affair. Merton makes it sociologically inevitable.

Now to Mulkay, who considers the outrageous response of science to Velikovsky to be a direct result of the education scientists receive, which does not inculcate openness to novelty, but intellectual narrowness.

"This rigidity becomes more intelligible when we recall the intensive educational process in science and that the *intended* goal of this process is primarily the attainment of familiarity with a body of established knowledge."²⁰⁶¹

Such an emphasis upon the primacy of received dogma, says Mulkay, "is likely to be particularly effective" in insulating the average scientist from tolerance for the revolutionary or controversial.

"First, it lasts well into adult life. Second, its severe demands . . . tend to isolate the participants from alternative vocational and intellectual interests. In the third place, science graduates tend to be particularly dependent upon their teachers for . . . eventual employment. Furthermore, Kuhn (1963) suggests that the science students within each specialty are educated by learning systematically just one consistent approach to the problems they will be faced with He concludes that 'nothing could be better calculated to produce 'mental sets' Hagstrom further suggests that 'the effects of scientific socialization are reinforced by a highly selective system of recruitment.'²⁰⁶²

To Mulkay, therefore, there is no doubt that this oppressive, patriarchal and narrowed formation of scientists accounts well "for the reception of Velikovsky's work."²⁰⁶³ It produces an extreme intolerance for disturbing new ideas, because what highly socialized scientists desire is *cognitive consensus*, whereas Velikovsky's radical ideas produced *cognitive dissonance*,²⁰⁶⁴ and to Mulkay, "the intensity of scientific reaction against Velikovsky

²⁰⁵⁹*Ibid.*, p. 67.

²⁰⁶⁰*Ibid.*, p. 131.

²⁰⁶¹*Ibid.*

²⁰⁶²*Ibid.*

²⁰⁶³*Ibid.*, p. 132.

²⁰⁶⁴*Ibid.*

is in accord with the magnitude of the dissonance."²⁰⁶⁵ Organized science as Mulkey describes it, is, therefore, much like a body of fanatically indoctrinated, medieval monks who simply could not tolerate the evil deviations of Velikovsky.

The result of their clone-like repugnance was the gross instances of misbehavior listed in the first chapter. I have quoted some of Mulkey's examples there, to which we can add several more.

". . . informal pressures were brought to bear upon those who suggested that these revolutionary ideas should at least be open for discussion."²⁰⁶⁶

"[The dismissal of] Gordon Atwater . . . is a specific example then, of scientists applying sanctions against one of their fellows for supporting Velikovsky, and thereby clearly disregarding the norm of scientific independence."²⁰⁶⁷

"It is also evident that this whole affair represents a marked rejection by sections of the scientific community of the value of original thought If originality . . . requires possible reformulation of paradigms which have attained the support of a quasi-moral commitment, then originality is likely to be rejected."²⁰⁶⁸

". . . what is particularly noticeable about the Velikovsky Affair is . . . the persistent tendency of scientists . . . to justify rejection of Velikovsky's claims simply by indicating the latter's departure from established beliefs."²⁰⁶⁹

". . . a new cognitive element was added by stressing that Velikovsky was not professionally qualified . . . and that, in addition, his integrity was suspect. As a result . . . it became unnecessary to take Velikovsky's claims seriously."²⁰⁷⁰

"But primarily scientific activity was directed toward . . . preventing the dissemination of Velikovsky's propositions throughout the scientific world and the wider community."²⁰⁷¹

²⁰⁶⁵*Ibid.*

²⁰⁶⁶*Ibid.*, p. 129.

²⁰⁶⁷*Ibid.*, pp. 129-130.

²⁰⁶⁸*Ibid.*, p. 129.

²⁰⁶⁹*Ibid.*, p. 130.

²⁰⁷⁰*Ibid.*, p. 131.

²⁰⁷¹*Ibid.*, p. 133.

None of these activities involves looking honestly at Velikovsky's ideas, and to Mulkay, the reason is evident. Given that Velikovsky encountered "a scientific community strongly committed to specific theoretical and methodological models which have been inculcated by an intensive socialization,"²⁰⁷² (which means a *band of fanatics*), Mulkay feels that the behavior of Science *could not have been different*, and in his study he concentrates on Shapley not because he is unique, but because his tyrannical behavior is merely "an extreme instance of the way in which certain scientists reacted to Velikovsky."²⁰⁷³ Sociologically, Mulkay is saying, Shapley is not aberrant. He is typical, only more so.

If this portrait of the formation of scientists is correct, then the popular myth of the scientist as supremely open-minded and eager for new ideas *cannot be true*. On the contrary, the average scientist *must* be closed-minded, intolerant of deviance, loyal only to his club and suspicious of innovation, and it is only the rare exception who will think or work independently. This, of course, will reduce to a minimum the number of scientists at any time who would be able to look at radical theories like Velikovsky's objectively, for mainstream scientific dogma must favor only *one* view at a time, (and how it was obtained and where it will lead), which leaves little room for the indoctrinated believer to assess without prejudice any competition or alternative. Ergo, the small number of scientists who asked that his work be evaluated on its merits, compared with the large number who merely parroted the party line in knee-jerk (and non-scientific) fashion. Mainstream science is not, alas, a place where daring concepts are welcomed with open arms.

The result, as far as our study of the Velikovsky Affair is concerned, is that, as a consequence of this history of tunnel vision, of professional blinders, nature to mainstream science today is not much changed from what it was to Lyell and Shapley 50 and 150 years ago, that is, uniform, linear and merely punctuated occasionally by moments of raised intensity which soon subside. To catastrophism of any sort, however, nature in *all* its dimensions is a non-linear, non-uniform, random sequence of episodes alternating expectedly between equilibrium and catastrophe. That is the essential battle, and it arises directly from (and will not be settled until there is a reconciliation between) the very different ethos or world view each side passionately upholds.

This is the first part of the problem, the inertia of indoctrination. Added to it is a second obstacle, which is the extreme amount of ego invested by scientists not only in their status in their profession, but in the status of the profession itself. (See earlier, Ginenthal's criticism.) There is consequently a great reluctance to risk change, or worse, to admit error, especially if it is pointed out by an outsider. The question then is, how is a new or revolutionary idea to break through these barriers of pride or this rigid kind of brainwashing, which is not merely narrow and dogmatic but cultist and elitist?

Mainstream Science as Desire

The third and greatest barrier, of course, is our absolute need to not believe in the possibility of celestial catastrophism. In my articles on catastrophism, I have argued since 1974 that every collective product of our culture, (e.g., literature, films, soap opera, comics), must serve that need for us as a whole, just as fantasies and neuroses would in an individual. They protect us from awareness of the trauma. Now, the catalogue of prejudice, distaste, preference, distortion and selection which I have outlined in mainstream science illustrates clearly in my opinion that what Lyell did to geology and Darwin to biology and Shapley to astronomy can be linked as parts of the same traumatized response, as symptoms of a syndrome we may call *catastrophobia*, which is the blind fear of disorder in the natural world. It seems to be the major hidden disease of our culture and, if scientists, too, are embedded in the culture, as Gould insists, then they too must serve the disease. Science may be described like the rest as a part of our cultural organism helping the whole to express its desire to not believe in catastrophism. Now, we have seen that Lyell constantly exercised *his*

²⁰⁷²*Ibid.*

²⁰⁷³*Ibid.*, pp. 128-129.

desire. Uniformity is what he wanted long before the evidence, and, when he retreats, it is not because of better evidence but is the result of a negotiation with what he wants. Desire, therefore, is the overriding force behind the entire uniformitarian debate and, when Lyell won it because of eloquence, not evidence, it indicates that we were co-conspirators. What I mean is that I think we acquiesced gratefully to his blandishments not because he had superior proof, but because he gave us what we wanted. It coincided with what he wanted, and the result was a marriage of mutual needs, his desire and ours, which we have called science. We have tied it like a ball and chain to our freedom of thought because we do not want to think in those areas forbidden by our uniformitarian fetters, and we, therefore, participate in the conspiracy to keep us from seeing the truth. Lyell could not have happened alone: it takes two to tango. He served us and we made him our jailer. His vision, led by racial memory and its effects, created a structure permitting denial of the memory, and this need matched ours, ergo his influence. It was a mesh of desires, and the result has been 150 years of ideological servitude. It is true that geology is slowly coming out from under it now, as are equally relevant disciplines like astronomy, ancient history, archeology and religious studies, but this casting off of the uniformitarian yoke, allowing new answers to appear, has been a long time coming. (Perhaps we are even approaching the time when Velikovsky will get a fair hearing.)

To those milk-fed on the myth of science which even Gould debunked a moment ago, it may seem repugnant to describe science as the product of desire. But certainly all of astronomy, until recently, as Velikovsky and the different scientists in our group have shown above, and all of ancient history, as the many catastrophist researchers working in history and chronology have shown, and every earth science, as even Gould himself has told us, have been struggling with the blindfold of uniformitarianism. We may say that all of modern science as Big Science is a creation constructed by desire, whose purpose is to get us to accept without contestation that the heavens are, were and always will be orderly, and that there have been no deluges, no cometary impacts, no violent destructions, no sudden creations of species. When we look at all of classical science from, let us say, 1750 to 1950 and perceive that the evidence in each field has been consistently (*i.e.*, compulsively) distorted, misinterpreted and selected to yield this result, we have to deduce that the Science of this period is an interlocked tissue of self-deceit which does what we want. That is to say, it is rampant desire, a human construct which we *have* to have. It subjected human thought for two centuries, and the Velikovsky Affair occurred precisely within this matrix of fear and desire.

The impulse behind it, not surprisingly, is very childish. I recall a short story about a young girl named Emily. When she became ill, her parents called the family doctor, but when he arrived Emily wanted to destroy him because, in her immature mind, she would not be sick until the doctor said she was, and, if she could prevent him from saying it, she would continue to be well. Kill the hated messenger and the event has not occurred. It was as if she believed that the doctor's words could determine reality, that whatever the doctor said would be true. Unfortunately, the large institutions in our adult society often seem to suffer from the same infantile delusion. Witness the propagandistic maneuvering of Lyell in the 1830's: he literally seems to have felt that, as long as he could coerce the Royal Society into denying catastrophes, *they could not occur, as long as no one said they could*. He was proud to have "carried the day" in the debates, but what did he "carry?" Merely the opinions of a certain group of men at a certain place, yet he felt that this petty upper-class human consensus would forever impose itself on nature. It has to have been the same for Shapley, Asimov, Sagan, the AAAS and Kurtz—frantic, bloody-minded efforts to prevent anyone from believing Velikovsky, and the catastrophes would not have happened.

Yet we have to wonder if this is the true cause, for it would seem that the perpetrators of these infantile attempts at suppression must have known at bottom that their tactics were doomed. That is to say, did the AAAS organizers really believe that, if people could be brought to laugh at Velikovsky, catastrophes could not happen? Did the Catholic Church really believe that, if Galileo could be forced to recant, the Earth did not orbit the sun? I think not, for surely Galileo's muttered retort *Eppur si muove*, (nevertheless it moves), whether or not he actually said it, would have been evident even to the most fanatical Churchman or member of the ASG. We are not the deity. We *have* to know inside us that we cannot control nature by edict. It is not a rational thought. What then could have been going on in the minds of these people?

In my opinion, it is not fundamentally the Emily syndrome. That is too simple. I believe that what was at work here (and still is) is something rather different. To perceive it, we shall re-evaluate the evidence, which in its portrait of non-rationality or pre-rationality will indicate precisely the root cause of the continuous Velikovsky Affair. What we have is a total rejection in terror and panic of *what we cannot bear to have mentioned*. In this process, evidence and truth are brutally ignored in favor of desire, as we have seen time after time from Aristotle to Lyell right up to Kurtz. There is a maniacal urge to suppress a speaker whenever he raises the possibility of catastrophism, and we, therefore, recognize that what is occurring is strictly passionate, yet the important question is, at whom is it aimed? At nature? I think not. If we are to correctly interpret the desire operative in this process, I suspect that, because the urge is senseless, because even a moment's reflection would reveal our powerlessness to affect reality, that is a powerful sign that our deepest purpose is not to coerce reality but ourselves. The aim of the Emily-rage is to prevent *us* from believing in catastrophe.

I am, therefore, urging a significant modification of the Emily-syndrome explanation. In the light of trauma theory, which I will discuss more extensively in a moment, I would say that the process whereby every intimation of catastrophe that has ever been voiced in our culture has been blindly combatted has occurred not simply because we feel that, as long as it is not said, it is not true, (which, however correct, is superficial), but for a deeper reason which is precisely the opposite—*we know deep down that it is true, but we do not want to know it!* As Velikovsky said in *Mankind in Amnesia*, it is a powerful desire "to not know." It is this deranged urge to have *us* avoid recognition of the trauma that is the true origin of our culture's constant and hysterical endeavor to keep catastrophism from being said. The event was too horrible to be rationally absorbed then, which leaves it still too painful to be uttered now, and that explains the irrational reaction to the spectre of catastrophism whenever it threatens to reappear, despite our rational awareness that we cannot really alter nature. It is *we* who are being fooled by ourselves in a process of post-traumatic avoidance, suppression and denial. I think that a great deal of the anti-catastrophist *animus* has been carried out under this combined Emily-and-avoidance syndrome, the delusion that if we can prevent the catastrophist supporter from speaking the unspeakable, from saying that catastrophes happen, (or block his work and burn his books if he does), then *we can make ourselves believe* that it is not true. The pitifulness of this anthropocentric pomposity is funny, yet it has characterized a significant portion of intellectual debate throughout our culture, as in the castigation and murder of Bruno or the stigmatization and attempted intellectual murder of Velikovsky. From Aristotle to the present, we have been savagely unwilling to allow catastrophist things to be said.

The Solution

Given these three powerful obstacles, we again have to ask how we might overcome them. I think we can, but only after we decide what the true problem is. Remember, I have insisted all along that this book is not only about Velikovsky, it is not only about catastrophism, it is about the appalling behavior of Western, and particularly American, science when faced with the rise of catastrophist theory. It is about the sickness at the heart of Western culture, that we have to keep inventing concepts which will allow us to pretend that the universe is safe, and we forbid any concepts which deny this, and we try to destroy those who propound it. It is a hereditary disease in our culture which this book illustrates, a collective pathology which has deformed our knowledge and coerced our behavior and led us into degraded excesses and made us be less than we can be. *That* is the problem—the illness of denial and its horrid by-products.

It is important for us to deal with this issue because in my opinion it constitutes a great danger to our society. Whenever Velikovsky was asked when he thought the next catastrophe would happen, he always replied that the greatest fear at the moment lay in catastrophes which might be caused by humankind. What he meant was that, if we continue to be the victims of the past, of the repressed trauma, if we continue to live in illusion and deny the truth, if we continue to commit monstrous acts because we are propelled by unacknowledged collective terror, like madmen, we may destroy ourselves long before the next cosmic

intruder will arrive. What lies behind his apprehension is the possibility that we are collectively, culturally crazy. That is to say, as long as we do not admit what happened to the Earth and us, we are a time-bomb and the clock ticks for us as it does for the next fragment from space. When Velikovsky first spoke to Shapley in 1946, Shapley wrote a letter to Prof. Horace Kallen, which ended "If Dr. Velikovsky is right, the rest of us are crazy."²⁰⁷⁴ Shapley meant it sneeringly, (*We know we are right and, therefore, Dr. Velikovsky must be crazy*), but I think that, 50 years later, his words rebound upon himself and mainstream science with an irony he would never have anticipated. What I mean is that, if Velikovsky is right, if we *are* suffering from a huge set of traumas which we will not admit, then we *are* collectively crazy. Western science in particular is crazy with the desire to deny what happened, and everything we as a society do, think and want serves that desire. It rules us, we are its puppets, and to that extent we are very, very crazy. As the chapters in this book have demonstrated, the response of our culture to the idea of catastrophes is a very chronicle of collective madness. It is the inevitable consequence.

This explains the Velikovsky Affair perfectly; for a crazed survivor of a catastrophe, who in terror refuses to acknowledge it, would literally want to destroy the man who speaks of it as if it did not happen. Ergo, Lyell's depiction of Cuvier as evil, the ASG calling Velikovsky a fraud and a charlatan, Plato's desire for the death of those who uphold heavenly instability, and the Church's threat to the very life of Galileo. What we hate and fear we label evil because it menaces our peace, and catastrophists are demonized because they are believed to literally serve the Demon of Destruction. It is part of the collective insanity.

There *is* a collective solution, however, and it is the same as it would be for a deranged individual. If a person is disturbed by a horrifying event, only the patient's awareness of the trauma and its effects upon him can save him, for only then can he escape the ignorance of the trauma which renders him an unwitting prisoner of his past. So it must be for Western culture as a whole. We are a murderous species, but a lot of our brutal excess can be avoided (although in practice it is far from simple) by our admitting the truth about the world, by our interpreting the past honestly, by our not avoiding or ignoring or distorting or rejecting evidence which we do not like, *i.e.*, by our looking reality in the face instead of pretending it is not there. The essence is, no more dinosaur behavior, no more Emily behavior, no more ostrich behavior, no more ASG-like *ex cathedra* proclamations about what can and cannot be in the world, no more dogma created by desire, in defiance of the evidence, in oblivion of reality, no more catastrophobia. We have to do the opposite, to grow up collectively and accept that the world is what it is regardless of us and of what we think and of what we want or don't want. That is to say, we have to adapt to reality and not try to adapt reality to us as uniformitarianism did. The first is useful but the second is a hindrance. The first is adult, the second childish, the first true, the second false, the first liberating, the second enslaving. That is the choice which catastrophist research places before us as we enter the 21st century, to open our eyes or not, to understand the world or not, to grow up or not. If we choose well, we may make this a better world. If we don't, we will just go on doing the hateful, wasteful, destructive things we have been doing, without knowing why and with no hope of escape. It is up to us.

Yet, how are we to make the leap from the first (blinker) view to the unencumbered new one? One of the best answers was given by the physicist Marx Planck 100 years ago, who said that scientific ideas do not change because people in power alter their opinions, but because they die off and the next generation, open to new concepts, is readier to look at the outrageous and nonstandard with greater objectivity. To achieve this, however, we will need a very differently-oriented generation of scientists, more spiritual and Trekki-like, raised on a diet that includes skepticism, holism, imagination and change, before mainstream science can become more open-minded about catastrophism. It will require people able to worry less about dicta than data, able to live with a world that may change irregularly and able to not have to perpetuate a fairy tale about a universe which is perfect and safe. Only if that happens, if the Planckian watershed is crossed, will it initiate a new era of science and leave the dinosaurs like Sagan and Kurtz to fight their increasingly lonely rear-guard actions on the periphery.

²⁰⁷⁴Stargazers, *op. cit.*, p. 54.

Yet how is this to be done? The evidence is already at hand, and much more is coming. That is not the problem. What is required is a *willingness to see the evidence for what it is*. If the implications of these discoveries were to be grasped by a new generation of scientists, armed with new evidence seen in a new spirit, then and only then could mainstream science attain a view of our existence as a vulnerable planet in a vulnerable Solar System in a tempestuous and evolving universe. Perhaps then, after this kind of breakthrough, the reign of the escapist uniformitarian dreamworld would end and science would enter the real world, able to see things as they are, not as a fiction of flight from terror but as a true vision, where catastrophes can occur in our domain as they do everywhere else in creation. This problem is, how is the leap to be achieved, given the deep recalcitrance of science?

Hints of the answer are available in a new book entitled *Trauma: Explorations in Memory*. In this anthology, the different contributors together paint a portrait of catastrophic trauma and its effects and the way of cure. The editor describes trauma as neurotic or pathological responses to "natural catastrophes . . . outside the range of usual human experience,"²⁰⁷⁵ "overwhelming events" accompanied by "the presence of unavoidable danger."²⁰⁷⁶ Because these events are a "radical disruption" of the normal,²⁰⁷⁷ their "incomprehensibility" makes them "unassimilable" in the normal way.²⁰⁷⁸ There is consequently "a psychic closing off,"²⁰⁷⁹ a "cognitive constriction,"²⁰⁸⁰ because "The trauma . . . in its unexpectedness or horror, cannot be placed within the structures of prior knowledge."²⁰⁸¹ What happens, therefore, is an "inability to fully witness the event as it occurs"²⁰⁸² because it is too great "an affront to understanding."²⁰⁸³ The result of such extreme experiences is a different kind of registration: "existing meaning structures may be entirely unable to accommodate frightening experiences, which causes the memory of those experiences to be stored differently and not be available for retrieval under ordinary conditions."²⁰⁸⁴ Out of this comes "traumatic memory,"²⁰⁸⁵ where "affectively charged events" are "dissociated from conscious awareness" and "encoded in an altered state of consciousness."²⁰⁸⁶ The effects, as we might expect, are distortions, hallucinations, numbing, avoidance and especially repression. There is a "denial of active recollection" as "large realms of experience . . . are disowned."²⁰⁸⁷ The individual experiences a psychic "dislocation"²⁰⁸⁸ which is openly called a "disease,"²⁰⁸⁹ and which Freud termed "defensive hysteria . . . caused by the repression."²⁰⁹⁰

Much of the patient's consequent behavior is determined by this process. The problem is that, because of its pathological power, the event is "never fully integrated."²⁰⁹¹ The victims remove themselves

²⁰⁷⁵Cathy Caruth, ed., *Trauma: Exploration in Memory*, (Baltimore, Md., 1995) p. 3.

²⁰⁷⁶*Ibid.*, pp. 4, 80.

²⁰⁷⁷*Ibid.*, p. 4.

²⁰⁷⁸*Ibid.*

²⁰⁷⁹*Ibid.*, p. 80.

²⁰⁸⁰*Ibid.*, p. 81.

²⁰⁸¹*Ibid.*, p. 153.

²⁰⁸²*Ibid.*, p. 7.

²⁰⁸³*Ibid.*, p. 154.

²⁰⁸⁴*Ibid.*, p. 160.

²⁰⁸⁵*Ibid.*, p. 16.

²⁰⁸⁶*Ibid.*, pp. 160, 164.

²⁰⁸⁷*Ibid.*, p. 152.

²⁰⁸⁸*Ibid.*, p. 9.

²⁰⁸⁹*Ibid.*, p. 3.

²⁰⁹⁰*Ibid.*, p. 165.

²⁰⁹¹*Ibid.*, p. 153.

consciously while the trauma occurs, "leaving other parts of their personality to suffer" while the memories are dissociated "in an alternate stream of consciousness."²⁰⁹² Because the trauma "escapes full consciousness as it occurs," the memories of it become "unintegrated experiences."²⁰⁹³ That is to say, they are suppressed, they are kept away from consciousness, but, despite their "lack of registration," they *can* affect the victim because the individual always suffers "possession by the past."²⁰⁹⁴ The pressure of a truly traumatic experience never goes away simply by itself, and the patient may suffer hallucinations or dreams and display unseemly "fear and repugnance" because he cannot surmount "a phobia for the traumatic memory."²⁰⁹⁵ The result finally is that "the traumatized person has to return to the memory"²⁰⁹⁶ in what Freud called the "insistent return" of the event against the will of the patient²⁰⁹⁷ after a period of latency or incubation or "delay."²⁰⁹⁸ In this "return of the repressed,"²⁰⁹⁹ the patient re-enacts the trauma, perhaps with himself as aggressor rather than victim. It is what Freud called the *repetition compulsion*, but it is not, as one might at first think, a desire to re-experience the trauma. Its purpose is the opposite, to keep the trauma suppressed, to prevent it from being consciously remembered.

This is the heart of the problem, that the patient will be possessed by a compulsive desire of which he is not aware: "the crucial factor that determines the repetition of trauma is the presence of mute, unsymbolized, and unintegrated experiences."²¹⁰⁰ As two of the contributors write:

"Freud claimed that, if a person does not remember, he is likely to act out: 'he repeats it, without knowing, of course, that he is repeating, and in the end, we understand that this is his way of remembering.'²¹⁰¹

This is a situation which cannot be avoided without therapy, for, wrote Freud, no matter when the victims experienced the trauma, "at some later time it will break into their life with obsessional impulses, it will govern their actions."²¹⁰² This is, of course, what Velikovsky also feared, following Freud, and, therefore, the vision that has formed the core of Velikovsky's portrait of our collective pathology since 1950 is also how a very distinguished group of psychologists see the issue 50 years later.

There *is* a solution, of course, which these people offer, and it is the same one which Velikovsky sought, which is to turn the traumatic memory into a "narrative memory."²¹⁰³ As well-known American psychologist Robert Jay Lifton said, there is "something resistant or incomprehensible at the heart of traumatic experience." In its extremity, it "is a radical break with any kind of knowledge, or with what we normally think of as experience."²¹⁰⁴ It is "the shattering of prior forms" because the victim "had no prior

²⁰⁹²*Ibid.*, p. 168.

²⁰⁹³*Ibid.*, p. 166.

²⁰⁹⁴*Ibid.*, p. 151.

²⁰⁹⁵*Ibid.*, p. 176.

²⁰⁹⁶*Ibid.*

²⁰⁹⁷*Ibid.*, p. 5.

²⁰⁹⁸*Ibid.*, p. 9.

²⁰⁹⁹*Ibid.*, p. 167.

²¹⁰⁰*Ibid.*

²¹⁰¹*Ibid.*

²¹⁰²*Ibid.*

²¹⁰³*Ibid.*, p. 153.

²¹⁰⁴*Ibid.*, p. 134.

images through which to connect with them."²¹⁰⁵ The result is " a second self . . . a traumatized self that is created"²¹⁰⁶ which denies its own death by imposing death on certain external groups, *i.e.*, by "rendering them victims,"²¹⁰⁷ as the Nazis did to the Jews, "in order to re-assert one's own life."²¹⁰⁸ He calls this a "false witnessing."²¹⁰⁹

Lifton's depiction of false and true witnessing provides us, in my opinion, with a professional, clinical explanation for the denial phenomenon (the desire *to not know*) which I said is the true motive behind the Velikovsky Affair. As he describes it, survivors of immense and inexplicable disasters feel they possess "a changed *world view*" which is the product of "an understanding that the laws of which the natural world has always been governed . . . are now suspended."²¹¹⁰ That is to say, when trauma survivors "begin to look around them, evidence that the world is a place of unremitting danger seems to appear everywhere" and the result is a universal terror in which "the dread is lasting and pronounced."²¹¹¹ In Lifton's view, the sociological result of this terror is the creation of collective belief, (*i.e.*, a *culture*), whose purpose is *to help us forget this awareness*. Lifton's description of this function of culture for the individual survivor of a trauma is identical to how I have described the palliative role of Big Science for our culture as a whole, with regard to our unconscious, ancestral knowledge of catastrophe, and the corresponding aversion of Big Science to Velikovsky.

"One of the crucial tasks of culture . . . is to help people camouflage the actual risks of the world around them—to help them edit reality in such a way that it seems manageable, to help them edit it in such a way that the dangers pressing in on them from all sides are screened out of their line of vision as they go about their everyday rounds."²¹¹²

If catastrophic theory is correct, if we all share by racial inheritance a survivor's unconscious knowledge of vast cataclysms, a knowledge that we cannot consciously face, then this is just what Big Science in its largest dimension does for us—it edits reality. Anyone, therefore, (like Velikovsky), whose ideas impede this urgent, psychological function, would consequently be seen as a great enemy, for, should the placebo of "culture" fail to work, then, as Lifton puts it, "People stripped of the ability to screen out signs of peril . . . are unusually vigilant and unusually anxious."²¹¹³ This is precisely how the Shapley generation reacted to the appearance of Velikovsky, with very unusual anxiety and hyper-vigilance, as if he had torn away the screens between their selves and reality.

The object is to deny what is unacceptable, and it is done through the act of *false witnessing*, which rejects the reality of the trauma and its resultant terror by attempting to destroy those who would remind us of them. Lifton, of course, is wholly unaware of the catastrophe controversy, but the process he describes explains the Velikovsky Affair perfectly. If we are traumatized by ancestral experience of unstoppable

²¹⁰⁵*Ibid.*, pp. 134-135.

²¹⁰⁶*Ibid.*, p. 137.

²¹⁰⁷*Ibid.*, p. 139.

²¹⁰⁸*Ibid.*, pp. 139-140.

²¹⁰⁹*Ibid.*, p. 140.

²¹¹⁰*Ibid.*, p. 194.

²¹¹¹*Ibid.*, p. 195.

²¹¹²*Ibid.*, p. 195 ff.

²¹¹³*Ibid.*, p. 195.

catastrophe, then false witnessing "is a perversion of meaning"²¹¹⁴ which attempts to undo what happened, to make it *as if it had never occurred*:

". . . the Hitler movement centered on . . . witnessing World War I by reversing its outcome. It's what the Rambo movies do for the Vietnam War."²¹¹⁵

In this process, the terrified survivor denies reality and affirms delusion by oppressing "certain groups violently for the sake of coping with one's own death anxiety."²¹¹⁶ Imagine that catastrophism is correct, that we all possess a "death anxiety" because of our inherited knowledge of the past, and that we evade it through Big Science, which is that part of our culture which *knows but has to deny* that vast, devastating catastrophes have happened. False witnessing would not only transfer that terror and destruction to an outsider and remove it from oneself, but would give the act a moral sanction.

". . . one must impose death on others in order to reassert one's own life as an individual and a group."²¹¹⁷

". . . killing . . . is always an attempt at affirming the life power of one's own group Killing becomes a morally necessary act . . . to reaffirm one's moral system or sense of self by destroying, violating, murdering another."²¹¹⁸

Here, in my opinion, is finally the truest cause of mainstream science's vicious oppression of Velikovsky — it is to affirm our collective life, (*i.e.* to deny the possibility of our collective death), by removing literal death from us, (which is what Velikovsky's argument threatened), and imposing intellectual death on an outsider, especially on the one who (like Hitler's depiction of the Jews) *is not part of us* because he is the evil one who first broached the idea. We affirm our delusion by destroying its opponent.

Lifton says, however, that the tactic is not successful, for the purpose on the surface is an attempt to deny death, but the result is the opposite:

"one attaches the taint of death to an other in order to reassert life's power . . . but you're also reasserting a denial of death, or a form of numbing You're taking on death anxiety through a reassertion of numbing."²¹¹⁹

The result, therefore, is not an escape from the anxiety of death but a plunge into a limbo where that which was supposed to be removed remains unremoved. In this condition, denial and self-delusion insulate the

²¹¹⁴*Ibid.*, p. 140.

²¹¹⁵*Ibid.*, pp. 139-140.

²¹¹⁶*Ibid.*, p. 140.

²¹¹⁷*Ibid.*

²¹¹⁸*Ibid.*

²¹¹⁹*Ibid.*, p. 141.

false witness from the reality of death only through the *narcotic* fiction of pretending "to have mastered death while deeply denying it, and numbing oneself to it."²¹²⁰ Lifton calls this a "false knowledge, or perverse witness,"²¹²¹ and that in my opinion is what Big Science is ultimately guilty of *vis-à-vis* Velikovsky. In its largest dimension, as a philosophy of the world, Big Science exhibits a failure to confront the possibility of catastrophism, (which is the presence of death in the natural universe), and a consequent desire to numb itself against that fear by imposing intellectual death on the ideas (and the person) of the surrogate Velikovsky. Better him than everyone. If it could destroy him and his ideas, (a purely local disaster), then death would not exist in nature, (a universal arena), and that leads to the myths of Big Science, which are "false knowledge." Velikovsky *the scapegoat* would carry off with him *privately* the knowledge of death, (which would apply only to him, and which he as such an evil person, therefore, rightly deserves), while the public domain of nature would have its confirmation of its life reaffirmed. Using Lifton allows us to perceive that this is ultimately what Big Science is doing on our collective behalf. It is pathological and pitiable, but, when explained by our catastrophic, traumatic model, inevitable.

The solution, in Lifton's opinion, is to achieve *true witnessing*, "to absorb and in some measure confront what one has had thrust upon one," for "recovery from post-traumatic effects, or from survivor conflicts, cannot really occur until that traumatized self is reintegrated."²¹²² It is only by this act of reintegration of the self, and not by disavowal or self-numbing through the imposition of death on surrogates, that the victim can escape the trauma, for "the struggle in the post-traumatic experience is to reconstitute the self into a single self,"²¹²³ one which, as the editor says, can be "integrated into a complete story of the past."²¹²⁴ The cure can only be achieved when the patient is enabled to *leave* the trauma, which can only happen if the traumatic event is *accepted* and understood, *i.e.*, if the patient "learns to remember,"²¹²⁵ for, if not, the trauma can always be "reawakened by some new precipitating cause."²¹²⁶ If acceptance *can* be achieved, good things result, for this permits "the story to be verbalized,"²¹²⁷ to "be transferred into narrative language."²¹²⁸ This is extremely important, for, *as long as the denial persists*, admitting remains an "impossible saying,"²¹²⁹ but acceptance finally allows the unspeakable to be spoken and enables the victim "to integrate past horror" into comprehensibility, "into one's own, and others' knowledge"²¹³⁰ after which, hopefully, there will no longer be the ferocious desire to annihilate the sayer of the unsayable. We will be free of the trauma and free of the past.

To apply this to catastrophism, it requires only that we make the leap from treatment of the traumatized individual to cure of our entire traumatized society, and Freud does that for us. "If we consider mankind as a whole," he wrote in *Moses and Monotheism*, "and substitute for it a single individual . . . investigation leads us to the same explanation as in the case of the single individual."²¹³¹ This creates the possibility that, if mankind as a whole can be brought to accept and integrate the truth about the great

²¹²⁰*Ibid.*, pp. 141-142.

²¹²¹*Ibid.*, p. 142.

²¹²²*Ibid.*, p. 137.

²¹²³*Ibid.*

²¹²⁴*Ibid.*, p. 153.

²¹²⁵*Ibid.*, p. 167.

²¹²⁶*Ibid.*

²¹²⁷*Ibid.*, p. 153.

²¹²⁸*Ibid.*, p. 176.

²¹²⁹*Ibid.*, p. 10.

²¹³⁰*Ibid.*, pp. 179, 153.

²¹³¹Quoted in *MIA*, *op. cit.*, p. 31.

catastrophes, we will no longer continue to be its victims. This is what Velikovsky believed, and I do too. It is what catastrophism is trying to do. It *is* the solution.

One feels uncomfortable in donning the robes of a salvationist—(believe in us and life will be better)—but science has always done that and few have complained, especially scientists. Now it is our turn, for science and its attitude are at the heart of the problem. The decades before 1950 were particularly the age of scientism, the belief (promulgated even by some of the major physicists like I. I. Rabi) that if everyone were as honest and rigorous and selfless and clear-headed as the scientists are, the world would improve markedly, there would be no war or crime, and all people would join in the pursuit and love of truth, hallelujah. What this book has established, however, calls for a very different view of science. That is to say, if our modern world is sick, and if our assessment of uniformitarianism is correct, then science is not our social healer, but may have played a central role in prolonging our sickness. By fostering the denial of catastrophe, *i.e.* of our true collective past, it has left us living a lie and we do monstrous things as a result, just like an individual who has blocked the memory of a trauma and compensates by neurotic acts. To carry Shapley's unfortunate remark further, mainstream science may not only *be* crazy, but may be keeping us insane.

These are very opposed descriptions of science. Which is more correct? I would say that both are true, that science, in one sense, does discover valid knowledge and, therefore, what it asserts is true, but that in a second dimension classical science can certainly be deemed a creation of our desire, and, therefore, what it hates or disparages is true, yet it is only through science that a proper and lasting understanding of catastrophe can be reached. It must, therefore, be approached with great reserve, as the locus of both insight or denial, as the avenue to freedom or the road to blindness. If we can use it properly, it can save us. If not, if we continue to *not see* what it discovers, it could destroy us. Which will it be? I'm afraid only time will tell. We have the answers, we need only the implementation.

I offer this as a final assessment of a situation where there are many grays but few blacks or whites. The world is complex and we are complex, and our relation to the world must be correspondingly complex. Only in the unreality of the laboratory can things be arbitrarily reduced to yes or no, true or untrue, but that is not the world. Science is true mostly of itself, but the Velikovsky Affair, alas, is true of the world.

* * * *

With this said, I end the book as I began it, reminding the reader that we are not debating whether Velikovsky in particular or catastrophism in general is entirely right or entirely wrong or partially right. That is not the point here. What is the point is that catastrophism deserves to be looked at fairly, unlike the past. Giordano Bruno tried it in 1590:

"In the work entitled *Spaccia della bestia trionfante* (which means 'The Expulsion of the Triumphant Beast,' that is, Platonic and Aristotelean cosmology)."²¹³²

Bruno was executed for his trouble. Four hundred years later, Albert Einstein pictured Harlow Shapley (whom Stecchini called the Cardinal Bellarmine of American science) as "the roaring astronomical lion,"²¹³³ and we have seen what happened to Velikovsky. We must, therefore, wonder how much longer the Beast will continue to distort knowledge by roaring, such that we may be insulated from an awareness of (and,

²¹³²Livio C. Stecchini, "Astronomical Theory," *The Velikovsky Affair, op. cit.*, p. 153.

²¹³³Alfred de Grazia, "Scientific Reception," *The Velikovsky Affair, op. cit.*, p. 193.

therefore, a terror of) catastrophe. Surely, only the truth can set us free, but that can only be achieved if we first recognize that Velikovsky is merely the most recent instance of people who have been treated very badly *because they doubted the eternal safety of the Earth*. Hopefully our review of the Velikovsky Affair points this out and calls for redress. If that is what this book helps to achieve, it will be satisfactory.

Will it happen? Will it happen soon? I hope so.

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FROM CALENDARS TO CHRONOLOGY, By Lynn E. Rose

PART ONE: IDENTIFYING THE PROBLEMS

GEORGE SOKOLSKY

Back in the mid-1960's, already near-dormant in the back of my mind, there was a vague recollection of my once having read a syndicated newspaper column by George Sokolsky, in which he referred to a controversial new theory that some quite unusual events had occurred in early antiquity, and were reflected in various ancient writings. Oddly, my recollection did not extend to just what those unusual events were!

In the 1970's, while I was intensively studying the Velikovsky theory and the Velikovsky Affair, that faded memory was revived when I came across Sokolsky's article in a collection of references to *Worlds in Collision* that had been compiled by Ralph Juergens. I realized that that could have been the same article that I had read. I probably saw it in the *Columbus Dispatch*, when I was sixteen. The title of the column was "Book Burning Professors," and it had appeared in the *Dispatch* on July 10, 1950.

Sokolsky was a conservative political columnist of the time, always delighted to find fault with those whom he perceived to be well to his left, such as professors. Thus the Velikovsky Affair was just his cup of tea. It enabled him to tweak all of those liberal defenders of open inquiry and academic freedom who were then engaged in bookburning.

Although the Sokolsky article was my first exposure to Velikovsky's ideas, the article itself made little impression upon me at the time, except that I vaguely remembered having read it. I never pursued the matter, and I did not retain either the title of *Worlds in Collision* or the name of its author.

"THEOMACHY IN THE THEATER"

After 1950, the Velikovsky theory did not again come to my attention until the later 1960's. Even some of the principal preoccupations of those intervening years, such as disaster films and the fear of atomic warfare, were never viewed by me in any kind of Velikovskian context. Indeed, most of the disaster films of that epoch I did not manage to see at all, though I did see a good many of them much later, on television. One of those that I saw later was even entitled *When Worlds Collide!*

I have *still* not been privileged to see *The Thing* or any of the *Godzilla* series. Thus I missed quite a number of the films that have particularly interested Velikovskians (see John V. Myers and Lewis M.

Greenberg, "Theomachy in the Theater: On the Fringes of the Collective Amnesia," *KRONOS* I:2, pages 23-34). At the time, even H. G. Wells' *The War of the Worlds* (1953) and the now-classic *Forbidden Planet* (1956) passed me by.

I finally saw *Forbidden Planet* on July 2, 1994. This mainly psychological film is thought by some to have been loosely derived from Shakespeare's *Tempest*, particularly with respect to the character of Caliban. It features "monsters of the id" that are brought into being from our own unconscious and that can destroy entire civilizations.

I may have seen at least parts of *The War of the Worlds* in the 1970's or in the 1980's. But I did not see it in its entirety until July 2, 1994 (yes, the same date). *The War of the Worlds* begins with the narrator explaining that the Martians were finding their own planet increasingly inhospitable and that they had looked elsewhere in the solar system for another planet that they might colonize. The narrator then runs through the various planets of the solar system and explains why – for this reason or that – the Martians had decided that every one of them except Earth was uninhabitable. In this enumeration of the planets, it is striking that Venus alone is not even mentioned. Perhaps the script-writers were unsure about conditions on Venus, as were most astronomers at the time (and still today).

THEORY EVALUATION

I paid little attention to these matters back in the 1950's. I was busy majoring in ancient history and classical languages as an undergraduate, and then in philosophy as a graduate student. I was still quite interested in astronomy, as I had been even as a child, but catastrophism simply never came to my attention. Nonetheless, I was very much interested in the *criteria* that are used, or that are supposed to be used, in the evaluation of scientific theories. This was part of my growing interest in the philosophy of science.

Typically, the discoverers of new theories are unable to shed much light on just *how* they did what they did. The processes by which theories come into being are simply not reducible to any set rules or procedures, and the great discoverers are notoriously unable to teach their students how to do what they themselves have done.

There is much talk about how rigorous scientific method is, and there is a traditional emphasis on such things as careful data collection, objective testing, crucial experiment, controlled experiment, replication of results, and so on. But these criteria are often very difficult to detect, at least in the actual *practice* of science throughout the ages.

I did not really accept such criteria anyway. My own view was that all of those things were of no ultimate use.

I preferred to recognize simplicity or logical economy as the *only* criterion for evaluating theories. A true theory gets "the most from the least" (this popular phrase is no doubt inspired by Leibniz), that is, it derives more in the way of theorems from less in the way of axioms. I still believe that once a theory has this it has everything else that could ever matter. Even alleged facts play no decisive role. It is important to recognize that *any* theory can be preserved in the face of *any* alleged facts merely by adding *ad hoc* hypotheses that explain the alleged facts away. But *ad hoc* hypotheses amount to extra unproved starting-points, that is, extra axioms. Thus the theory is to be rejected, if at all, not because it has any problem dealing with alleged facts (which it does not), but because it has too many axioms, and thus has too little in the way of simplicity or logical economy.

* * *

Unfortunately, these arguments about the correct criteria for theory evaluation seem to be of interest mainly to philosophers of science. Besides, they appear to be largely moot, in that the scientists do not always practice what either they or the philosophers preach anyway. In their actual practice, scientists all too often evaluate proposed theories on the basis of whether the proposer is of the right race or nationality or religion, is of the right gender, is in the right discipline or department, has the right degrees, is employed in the right place, knows the right people, and so on. The theory itself may be evaluated on the basis of its consistency with what the evaluator has come to believe, on the basis of its possible impact on the reputation of the evaluator, and even on the basis of its possible impact on scientific budgets. (The actual evaluations of Velikovsky's theory by scientists have of course depended upon *many* of these "unpreached" criteria.)

Thus I was not convinced that there even were any legitimate rules that were routinely followed in scientific practice. Many of the evaluations made by scientists seemed to me to employ illegitimate criteria. And I was always on the lookout for case histories that would illustrate this point.

LONG JOHN

My first serious notice of Velikovsky came in the later 1960's. As a night person, I frequently listened to Long John Nebel, whose all-night radio program was broadcast over WOR in New York City, but could be picked up in Buffalo. As it happened, the theme music of that program was taken from *Forbidden Planet!* (I was aware of that at the time, even though I had never seen the movie.)

One night, Long John's guests were Livio Stecchini and Alfred de Grazia, who, along with Ralph Juergens (not present on that occasion), were the co-editors of *The Velikovsky Affair*, which had been published in 1966.

Stecchini and de Grazia discussed both the Velikovsky Affair and the Velikovsky theory itself. I found it fascinating that the scientists and other scholars who had paid lip-service to some of the more widely preached criteria for evaluating and choosing between scientific theories would have forgotten all about that, and would have used a quite different set of criteria in evaluating the work of Velikovsky. I ordered *The Velikovsky Affair* and read it in its entirety. Particularly informative were the two articles by Ralph Juergens and the two articles by Livio Stecchini, as well as Velikovsky's own "Additional Examples of Correct Prognosis."

A WIDENING PERSPECTIVE

I realized almost from the start that Velikovsky's critics had no decent arguments against him, but I took it for granted that he *must* be wrong, and that it would simply be a matter of digging out the appropriate evidence. Gradually, however, I came to realize that I did not have any more evidence against Velikovsky than his critics did, and I began to consider the possibility that he was right. After all, if he was wrong, surely by now someone would have been able to establish that fact. And yet no one had. As my perspective widened, I ceased to focus just on what the critics had said and done, and looked also at what Velikovsky had said and done. From then on, I was as much interested in the Velikovsky theory itself—especially on the astronomical side—as in the Velikovsky Affair.

I purchased a copy of *Worlds in Collision*, a book that is certainly readable enough, but I found it rather slow going, mainly because I was continually checking things, looking up sources, and so on. It took me quite some time to read the entire book. I also read several chapters of *Earth in Upheaval*, and at least the opening pages of *Ages in Chaos*.

My first contact with Velikovsky himself was on Wednesday, October 13, 1971, when I obtained his telephone number from Information and called him.

As I realized later, it was Mrs. Velikovsky who answered the phone. I told her my name and asked if I could speak to Dr. Velikovsky. I heard her tell him my name (which neither of them knew) and he took the phone immediately.

At that point, there were only two things on my mind. I wanted to ask him if I might have his address, so that I could write to him, and I also wanted to ask him if he had any response to those who would claim that Stonehenge shows that Earth and the Moon have been on their present orbits ever since early in the second millennium before this era.

STONEHENGE

This matter of Stonehenge was of great importance. If Stonehenge did indeed date from early in the second millennium, as the conventional view held, that would be somewhat prior to the time that Velikovsky assigned for the near-collisions between Earth and Venus, and many centuries before his near-collisions between Earth and Mars. But it is usually acknowledged that Stonehenge is so arranged that the Sun would have risen over the Heel-Stone on the day of the summer solstice. That is, Stonehenge is more or less reflective of Earth's present orbit and of Earth's present obliquity. If that is the case, then it is difficult to see how any near-collisions of planets could have taken place in the meantime.

My question about Stonehenge was of course no accident. I had by then been interested in astronomy for nearly thirty years, as well as in antiquities generally. And I had noted that there was no mention of Stonehenge in *Worlds in Collision*. By the time I telephoned Velikovsky, people like Atkinson, Hawkins, and Hoyle were calling considerable attention to Stonehenge: whether Stonehenge was or was not an "astronomical observatory" had become a hot topic of debate. The circumstances in 1950 were nothing like that, which is probably why Velikovsky had not bothered to discuss Stonehenge in *Worlds in Collision*. Nonetheless, it seemed to me that this matter of Stonehenge was crucial and needed to be answered, especially in the current climate. If Stonehenge is indeed a huge analogue calendar of some sort, it seems to reflect the *present* astronomical circumstances. How could a *calendar* reflective of *present* circumstances be nearly 4000 years old, if those circumstances are supposed to have changed in the meantime?

Notice that I was throwing difficulties at Velikovsky from the start. I continued to do that for the rest of his life. He always *encouraged* me to do so. Sometimes he could answer me, and sometimes he could not, but I was *never* rebuffed.

OFF TO TORONTO

Velikovsky first gave me the Hartley Avenue address, and then informed me that he had written at length about Stonehenge in the *Yale Scientific Magazine* (April, 1967), and that he would provide me with a copy. He also mentioned that he and Mrs. Velikovsky were leaving that very afternoon for Toronto, where he would be doing a series of lectures, interviews, and appearances over the next ten days. Toronto is only a few hours from Buffalo, and Velikovsky's thought was that I might be interested in attending some of those events. Indeed I was. When I called, I had not foreseen that I would be meeting him ever, let alone quite this soon!

Antoinette Paterson and I quickly made arrangements to drive to Toronto the following Friday. I had seen Velikovsky's picture, and I happened to spot him and Mrs. Velikovsky as they were about to get into a car in front of the Four Seasons Hotel. Toni and I introduced ourselves, and Velikovsky gave us directions to the home of Robert Zend of the CBC, who was having a reception that evening in honor of their visit.

I have three somewhat disparate memories of that reception.

THE VERY NEXT THOUGHT

(1) At one point during the evening, with everybody in the room listening, a man whose name I no longer recall asked Velikovsky what had been his very *next* thought, just *after* he had first grasped his overall theory. Velikovsky's answer was immediate: "That they will not accept it."

In those early years, I often forgot that Velikovsky was a psychoanalyst. As is emphasized elsewhere in this book, the Velikovsky theory *predicts* the Velikovsky Affair. That there will *be* a Velikovsky Affair is a *theorem* of the Velikovsky theory. It is *deducible* from the basic principles, or axioms, of Velikovsky's theory. It took me quite some time to realize that. Velikovsky seems to have realized it from the very start.

MOTLEY CREW

(2) It was surprising how many people from the United States had gone to Canada in protest over the Vietnam War. I had more conversations with Americans about the war that night than I had ever had on campus in Buffalo.

But this is not to say that all of the guests were activists in the anti-war movement, or that all Velikovskians of that era were of such a persuasion. On the contrary, Velikovskians have always been a motley crew indeed: hawks and doves; pious and impious; male and female; gay and straight; young and old; black, white, and yellow; right-wing, left-wing, and center; Ph.D.'s and barely lettered; hard science and soft science; jocks and couch-potatoes. In Velikovsky's own words about his supporters: "They are all different." He could never detect any distinguishing or even frequent characteristics. It may well be that their *only* common trait was that they had been willing to look at the evidence and had not recoiled in terror from the very idea of interplanetary near-collisions and global catastrophes within historical times.

WHO ON EARTH IS HATSHEPSUT?

(3) I was also surprised to learn that so many people present had read *Ages in Chaos*. At that point, I had still not read more than a few pages of it, even though I had read all of *The Velikovsky Affair*, most of *Worlds in Collision*, and a good portion of *Earth in Upheaval*. At the reception there was much discussion of Hatshepsut and the Queen of Sheba, but all of that was new to me. I had not even known who Hatshepsut was, let alone that Velikovsky had equated her with the Queen of Sheba. I of course resolved to read *Ages in Chaos* as soon as I could. Nonetheless, despite my deep involvement in the Velikovsky movement from that time on, I still did not finish *Ages in Chaos* for several more years.

STAYING AWAY FROM CHRONOLOGY

Looking back, I am not particularly surprised that I had not yet read *Ages in Chaos*, or that it was the last of Velikovsky's published books that I read in its entirety. My initial interest had been in the criteria that scientists and others use in the evaluation of theories, and only gradually did I become interested even in the astronomical scenario, let alone in the rest of the theory. Despite the fact that I had majored in ancient history

and classical languages, the historical and chronological side of Velikovsky's work (as well as the psychological side) never seemed to get much attention from me.

I mention my neglect of these other aspects of Velikovsky's work because that neglect remained habitual with me for many years. I did become a little better informed about the chronological issues as time went on, but most of my own writings remained concerned with the astronomical problems.

THE "THREE PILLARS"

By way of prologue, however, it might be mentioned that Velikovsky had already noted that the conventional chronologies that historiographers have provided for many of the other nations in the area have been derived from the chronology of Egypt, which historiographers have come to see as fixed and absolute. Velikovsky also identified "three pillars" as the principal supports for the conventional Egyptian chronology: the "Sothic" pillar, the "Menophres" pillar, and the "Manetho" pillar. (I shall refer to the latter two as the "Menophreus" pillar and the "Manethon" pillar.) All three of these will be examined in more detail in later sections of this essay.

The Sothic pillar is the conventional belief that the Egyptians, for millennia, paid special attention to the heliacal rising of the star Sirius (or Sothis), and that documents giving the Egyptian calendar-dates of these heliacal risings of Sirius can be located in history with great precision.

The Menophreus pillar is a late report by the Theon annotator placing a certain Menophreus (otherwise unknown) in -1321. The clear implication is that the heliacal rising of Sirius occurred on Thoth 1 Egyptian in that year.

The Manethon pillar is the list of thirty or thirty-one (consecutive?) Egyptian dynasties that we have from Manethon, a writer of the early Ptolemaic period.

Clearly, the first two pillars have much to do with astronomy, and the third has mostly to do with history and chronology. Thus I remained very much in character here: even when I began studying the Menophreus pillar and the Sothic pillar, it was quite some time before I got anywhere with the Manethon pillar.

What finally did it was my re-dating of the Twelfth Dynasty, in 1992. That brought me into *direct* and *personal* confrontation with the Manethonian scheme, and further avoidance of chronology was impossible.

FOR LACK OF A KEY

One reason for my long avoidance of chronology is very simple: the numerous competing theories and the innumerable details—even those pesky *names!*—were impossible for me to keep track of, let alone juggle. Thus whenever I did try to study chronology, I quickly reached a state of overload: I found that the Ages in Chaos left my mind in chaos! No matter how much time I spent on the subject, I just never felt that I had gotten anywhere, I never felt that I had discovered anything worth reporting, and I never felt able even to articulate the various competing chronological theories, let alone debate them.

I needed some kind of handle, some kind of *simplifying key*, before I would be able to sift my way through the piles of claims and data.

I was as overwhelmed as someone in the seventeenth or eighteenth century might have been, trying to read Egyptian hieroglyphics, with all of their vivid, senseless detail, and no discernible pattern. I needed a chronological Rosetta Stone: a key; a clue; a fixed starting-point, *anything*; . . . and *whatever* it turned out to be, it would have to possess some real chronological *clout*.

I have oftentimes wondered if I am entirely alone in having had such difficulties with chronology. Probably not. After all, if I have this kind of trouble, the chances are that others will, too. Nonetheless, it puzzles me that so many people are able to discourse quite merrily about ancient chronology, with no mention of the extreme difficulty of the subject!

A LATE ENTRY

My long-standing avoidance of the chronological issues now seems rather ironic, since one of the most important things that I have ever accomplished turns out to be the radical redating of the Twelfth Dynasty, an achievement that does involve some astronomy but has little to do with interplanetary near-collisions as such (except that they must have been earlier). Its principal importance lies in *chronology*, the very field that I so long neglected!

Anyway, as things have turned out, I now have *two* excellent handles on chronology—the concept of a Velikovsky Divide and the correct dating for the Twelfth Dynasty. Both of these do have *plenty* of chronological clout. They are like templates, or litmus papers, that can be used as *powerful* organizing principles in dealing with the masses of historical material that survive from antiquity. Both will be discussed at some length in the pages that follow. Armed with these two chronological devices, I am finally ready to rumble with a vengeance! I may be a very late entry in these chronological competitions, but I am here to stay. The Zend reception is far in the past.

SUPEREROGATION

On Saturday, the Velikovskys were our guests at dinner. In the afternoon, before we left for the restaurant, Velikovsky discussed with me the several dozen respects in which I thought that his theories were wrong. Looking back at it, I feel that his patience was remarkable. He had just met me. Why should he put up with that from someone he hardly knew?

Ever since the opening *seconds* of our very first conversation, Velikovsky was always willing to listen to criticism from me. That *may* be the most unusual aspect of our relationship. There are some, even among his supporters, who say that he was impatient with criticism, and dismissive of critics, and if he could not be bothered. That picture is entirely inconsistent with my own experience of the man himself. He was never that way with me. Why did he treat me so differently from the way he is said to have treated others?

I might like to think that he had decided right away that I knew what I was talking about and that my criticisms and comments might be of value to him. But these considerations are true of many others who dealt with him, and cannot be the whole story.

Perhaps my manner had something to do with it as well. Usually, I did not tell him what to do; I simply told him what I thought. Thus I did not approach him with any kind of agenda.

Later, I usually *sent* him my criticisms, in written form, so that he could examine them at his leisure. They were not off the cuff, and he was always among the first to know of them. Whatever the reasons, however, I did not get the reception that others complain that they got.

The one time that I told him not to do something, and *thought* that I had succeeded, he did it anyway. Later, he wrote to me acknowledging that I was right to warn him about X; he even mentioned the "duplicity" of X. But in that same letter he reminded me that he had once told me that Y was not to be trusted, but that I had not believed this until I found out for myself. Thus it all balanced out. I think that Velikovsky even derived a certain satisfaction from the symmetry here: he was wrong about X, and should have listened to me, but I was wrong about Y, and should have listened to him!

BEN HA BAYIS

I do not doubt that Velikovsky became something of a surrogate father-figure to me. Certainly there was an atmosphere of camaraderie and open communication that I never achieved with my own father. (I always tried to communicate with my parents as little as possible.) For his part, Velikovsky indicated in any number of ways his positive feeling toward me and the special trust that he placed in me. The following inscription was written on the back of an 8 x 10 copy of the same photograph that was later used for the dust jacket of *Ramses II and His Time*:

To Lynn,
friend faithful and just and fair
with love

Immanuel V.

April 76

And when I asked him to autograph my first printing of *Worlds in Collision*, he did much more than merely autograph it:

My Dear Lynn,
my closest collaborator, primus inter pares,
I inscribe your impressio prima of a book of
legend that became a legend of a book

Immanuel

8 August 978

(Velikovsky would commonly omit the "thousand" from dates.)

One afternoon, in Princeton, Velikovsky needed a nap. I was going to be staying for another day or so, but several visitors who were there for just a few hours would shortly be leaving, presumably before Velikovsky completed his nap. Meanwhile, they were enjoying some cheese (?) and wine that had been served. Velikovsky graciously bade them goodbye and excused himself. It was clear that he did not expect to be seeing them again that day. As he left, he announced with a smile that he was appointing me *ben ha bayis*—"son of the house"—in his absence, and asking me to serve as host during the remainder of their visit. (One might suppose that he was "just" saying something witty here, but, as a long-time Freudian, he knew very well that witticisms usually involve much *more* than mere wit.)

I may not have done a very good job as *ben ha bayis*. When Velikovsky returned from his nap, he was quite surprised to find that everyone was still there. Probably he also noticed that the wine had been killed, and that his guests were somewhat mellower than when he left! *Then* they decided to depart.

IGNORANCE AND INTERVIEWS

My dealings with Velikovsky were not always smooth. Indeed, a major strain developed less than 48 hours after we met.

Sunday morning, Velikovsky was busy at the CBC studios in Toronto, preparing a series of audio tapes for later broadcast. Toni and I were there too, mainly, I thought, to say goodbye before we headed back toward Buffalo that afternoon.

After one segment of the taping had been concluded, Velikovsky decided that he would like *me* to interview him for a while. This was a disastrous decision. I knew something of the astronomical side of his work, but little else. I was uncomfortable even with microphones, let alone broadcasting studios. And I knew *nothing* about interviewing. I was terrified that there might be a pause. So, feeling obliged to say something, anything, I filled the silences, no matter how. I jumped from one thing to another, never following anything up, and rarely even listening to the answers that I had elicited.

Velikovsky was displeased with the way things were going. Then he came up with a solution that was even worse: *he* would interview *me* for a few minutes. So, without warning, he just switched from interviewee to interviewer. But he did not like the answers he got; many of them surprised him. He had set me up as an expert on all aspects of his theories, and then my responses suggested otherwise. He alluded to the widespread student interest in his work, intimated that I taught a course entirely devoted to his work, and then asked me some very precise questions. My answers revealed that this was a graduate-level course with a grand total of *two* students, and that while Velikovsky had been dealt with to some extent, the course itself was on pre-Socratic philosophy. And so it went.

Velikovsky was probably a much better interviewer than I was, but he had made the mistake of asking me questions after he had already intimated what the answers were going to be. He may have thought that he knew what the answers would be, but he did not. It is unfortunate that on that day he was not operating more as a psychoanalyst. As a psychoanalyst, he would *never* have stated the expected answers before asking the questions.

I did not realize quite how bad all of this was, but Toni, who had been listening outside with Mrs. Velikovsky, later told me exactly how bad indeed: that the session was a disgraceful mess, and that it was entirely my fault.

Interviewing is clearly a job for the professionals.

Enter the pro. Whoever was in charge that day (perhaps it was Robert Zend) seems to have decided at that point to commandeer the services of a passing weatherman, whose name I do not recall. This man was no doubt an experienced broadcaster, but he had never even heard of Velikovsky. That did not seem to matter to anyone. They just threw him into the pit, absolutely cold. He asked some introductory questions about who Velikovsky was and what he was doing in Toronto, and listened to Velikovsky's answers. He then followed them up, and continued in this way for nearly an hour, weaving a logical and orderly tapestry that was masterful, clear, and informative. I could never have done that in a million years. Neither could Velikovsky.

THE NEXT EIGHT YEARS

The CBC incident was of course not enough to deter either Velikovsky or me from pursuing our shared interests. Rather quickly, he and I had begun to work much more effectively together than we had at the CBC that morning in Toronto!

Before we left, Velikovsky gave me an inscribed copy of the *Yale Scientific Magazine*, as well as other materials. His discussion of the Stonehenge problem turned out to be quite lengthy, but the key points were

that there had been much construction and reconstruction at the Stonehenge site, as if to keep tabs on a continually changing cosmos, and that those several features of Stonehenge that seem to reflect the present arrangement of the planetary system do not necessarily date from as long ago as the near-collisions between Earth and Mars. In this case, Velikovsky's own answer amounted in part to a *redating* of materials that seem to reflect the *present* cosmos. We shall be looking at that same sort of approach throughout the rest of this chapter.

* * *

During the next eight years, until Velikovsky's death on November 17, 1979, and indeed since then, I have been very much involved in the Velikovsky movement. After that first telephone call to Velikovsky, I had jumped into the movement with both feet, and on a more or less full time basis, although I did have other responsibilities.

Everything seemed to happen at once. Early in 1972, I taught my first course on *Worlds in Collision*. That was also when I met Raymond C. Vaughan; we have been working together on the Venus tablets and other problems ever since. Then Toni Paterson and one of her students, Janice Nuzzo, were responsible for bringing Velikovsky to speak at the State University of New York College at Buffalo, on Monday, March 20, 1972. I had a large reception for the Velikovskys the next day at my home. On Wednesday, Velikovsky spoke at McMaster University in Hamilton, Ontario, during that same trip.

In August of 1972, I participated in the *Pensée* conference in Portland, Oregon. There I met Atwater, Burgstahler, Cardona, Greenberg, Grinnell, Jueneman, Juergens, MacKie, Mullen, Ransom, the Talbott brothers, Willhelm, and many, many others. Except for Toni Paterson and Ray Vaughan, I had been feeling somewhat isolated in the work that I was doing. Now, suddenly, I was meeting Velikovskian researchers by the score.

* * *

My first visit to Princeton was in November of 1973, when Toni and I joined C. J. Ransom, William Mullen, Chris Sherrerd, and several others at the Velikovskys' home in a day-long working session with Professor Irving Michelson, a Chicago physicist who was scheduled to participate in the A.A.A.S. Symposium the following February.

It was during that visit that Velikovsky first spoke of my serving as his literary executor. He continued to speak along those lines during the rest of his life, but he never took any legal steps, and my role was always quite informal.

* * *

The *real* flurry of activity was in 1974.

I did not go to the University of Lethbridge conference in May of 1974, but I did attend the four other conferences that year that featured discussions of Velikovsky's work: the A.A.A.S. Symposium in San Francisco; the McMaster University conference in Hamilton, Ontario, in June; the Duquesne University History Forum in Pittsburgh, Pennsylvania, in October; and the Philosophy of Science Association conference at the University of Notre Dame in South Bend, Indiana, in November.

C. J. Ransom and I did *not* speak at San Francisco, despite the fact that Velikovsky had tried to name us to his team. Vaughan and I co-presented our "Analysis of the Babylonian Observations of Venus" at McMaster. I also spoke at Duquesne, and both Toni and I spoke at Notre Dame.

* * *

In the years following, I made many trips to Princeton, and several to Seaside Heights, New Jersey, where the Velikovskys had purchased a second home. I was involved with all aspects of the A.A.A.S. Affair and its aftermath, as well as with the preparation and publication of *Peoples of the Sea*, *Ramses II and His Time*, *Mankind in Amnesia*, and *Stargazers and Gravediggers*, not to mention many smaller projects. Toward the end of Velikovsky's life, he and I began to co-author *The Sins of the Sons: A Critique of Velikovsky's A.A.A.S. Critics*. With much help from Mrs. Velikovsky, this book was successfully completed after his death, sometimes by my writing around those pieces that Velikovsky had already written.

VELIKOVSKY AND RELIGION

There seems to be a rather wide-spread interest in the question of Velikovsky's religious beliefs, if any. I regret that I cannot give any straightforward account of the matter. All that I can do is report some of the things that I myself noticed. No doubt there are other people who know a great deal more about all this than I do.

When I was visiting the Velikovskys in Princeton, I was given free run of the first floor, including the music room (where many of Velikovsky's books were shelved), the dining-room (which usually served more as an office and work-room), and, when it was built, the several rooms of the new addition in the back. (This addition was personally designed by Velikovsky, who had once thought of becoming an architect; it was built in 1977.) On the second floor, I made use of the history room (where there were more books, as well as many of Velikovsky's manuscripts) and the kitchen (especially for getting my breakfast).

The kitchen was kosher, but casually so. It seemed to be more a matter of custom and tradition than any strict theology. I never did understand the full set of rules, just the ones that pertained to me: the bowl and the spoon that I used for my cereal were to be taken from *here*. Mrs. Velikovsky had explained to me what I needed to know, and I was very careful to comply, but I did notice that neither she nor her husband seemed to be disturbed if *other* guests got their hands on the wrong ware.

The Velikovskys were observant, but only to a point. Yes, their kitchen was kosher, but the restaurants that they frequented were not. They were members and financial supporters of the local temple, but I do not recall that they ever spoke to me about their having gone there. Velikovsky would not sign contracts on the sabbath (he told me that this was a practice of his father's that he wished to respect), but I noticed that he did write checks on the sabbath. It was also clear that Velikovsky *worked* just as much on the sabbath as on any other day — and not just when I was visiting Princeton.

I usually try not to telephone people on what may be a sabbath or other special day for them, at least not until they indicate by their own behavior (such as by telephoning me on such days) that it does not much matter to them. In time, I noticed that Velikovsky's telephone calls to me were about as likely to be on the sabbath as on any other day of the week. After that, I no longer hesitated to call *him* on the sabbath, if it seemed necessary.

By the way, Velikovsky was just as concerned about what might be a bad time to call me. After he learned that I was a night person, he tried to avoid calling me in early or mid-morning, even if he, a morning person, had been up since before dawn. (More than once he managed to awaken me in the late forenoon, but I did not tell him!)

During his ten-day appearance in Toronto in 1971, Velikovsky's formal schedule was kept entirely clear on Saturday, possibly at his own request. But it might be noted that the Zend reception on Friday night was well after the start of the sabbath. This was partly a social occasion, of course, but it was also a *working* session: Velikovsky was put through his paces for at least an hour, answering all sorts of comments and inquiries from the floor. It might as well have been the question-period following a lecture.

In *Worlds in Collision* (page 94), there is a passing reference to "the great Architect of nature." But Velikovsky's writing frequently borders on the lyrical and the poetic. Robert Zend, who was Velikovsky's host that night in Toronto, had even prepared a collection of some of the more lyrical and poetic passages from Velikovsky's writings; he called it *Font Velikovskiy*. And Velikovsky himself had composed a small book of poetry once: it is published in Russian, under the pen-name Immanuel Ram. All things considered, I remain unpersuaded that we can read a great deal into that one very intriguing passage from *Worlds in Collision*.

Velikovsky was a strong Zionist from youth, but this often seemed to be political and cultural rather than theological. After all, Moshe Dayan was a strong Zionist, too, and yet an outspoken non-believer. I do not mean to suggest that Velikovsky shared the religious views of Dayan; all I mean is that Velikovsky's Zionism does not in itself settle any of the usual questions about his possible views on religion.

Did Velikovsky believe in God? He never told me, and I never asked, or even cared. Whenever audience-members inquired about this, following his lectures, Velikovsky would not say. This was not a matter of secrecy or coyness so much as a matter of deliberate policy. He wanted his theories to be judged on the evidence—not on the private views of their author, and not on the private views of his readers. I think that that policy was a wise one. Perhaps its severity and tough-mindedness appealed to the philosopher of science in me.

Velikovsky contemned and repudiated four different groups of people:

(1) Those non-religious people who rejected him mainly because he considered ancient religious texts worthy of serious study.

(2) Those religious people who embraced him mainly because he considered ancient religious texts worthy of serious study.

(3) Those non-religious people who embraced him mainly because he interpreted ancient religious texts naturalistically. (That is, because he explained "miracles" as natural events.)

(4) Those religious people who rejected him mainly because he interpreted ancient religious texts naturalistically. (That is, because he took away what they considered "miracles.")

The only people whose evaluations Velikovsky did take seriously were those who were willing to look at his theories in the light of the evidence. That was all that he ever asked.

There is one more incident that might be relevant here. Velikovsky and I once went to the Department of Astronomy at Princeton (or perhaps they were calling it Astrophysics). Yes, he did from time to time go there! Furthermore, nearly everybody we met knew him, and greeted him in a friendly manner. I was somewhat surprised, but obviously Princeton is no Michigan or Harvard.

Our reason for going there was that Velikovsky wanted to consult with—and to have me meet—a member of the astronomy faculty who had a much more than passing interest in Velikovsky's work. I shall not name him; those of us who are contributing to this book wish to give an account of the continuing Velikovsky Affair, but we do not want to instigate its next chapter.

Anyway, out in the hall there was a huge framed photograph of a starfield, to which Velikovsky called my attention. With considerable amusement, he said that our Sun was just one star among all of the other stars, and that it was quite ridiculous for anyone to suppose that we might be alone in the universe or that we might be of any great cosmic importance!

* * *

These recollections of mine will of course not satisfy anyone who wants to discover the exact nature of Velikovsky's views on religion. All that I have been able to do here is to enumerate those possibly-relevant facts that I happen to know.

BLOOD PRESSURE AND SWEETS

I always tended to see Velikovsky as the author of *Worlds in Collision*—and as the prime target of the Velikovsky Affair—rather than as the physician and psychoanalyst that he had been during his professional life. My tendency to forget that side of him was no doubt facilitated by his own essential persona, which was simply that of a dedicated scholar and an enthusiastic seeker after truth. There was little sign of the clinician. Not once did I even imagine, for example, that I was being scrutinized by a professional eye, or that my psyche was being subjected to analysis. That was simply not his way. Thus it sometimes startled me when there *was* an unexpected reminder of his professional background.

* * *

While we were working on some manuscripts in the music room, and before I even realized it, Velikovsky had suddenly produced from some nearby place of concealment a blood pressure gauge, and he proceeded to test himself. But he did not succeed. It seems that the gauge only registered up to 275, and that Velikovsky was completely off the scale! This struck him as greatly amusing. Nonetheless, he did deign to telephone his own physician, who stopped by a little later to be informed of what the diagnosis was.

I do not know if the high blood pressure went with Velikovsky's diabetes. But his sweet tooth evidently *did*. He would look at the forbidden item, and state his medical opinion regarding it: "I must not eat this." And then he would eat it. Velikovsky was *not* a very good patient.

Despite my tendency to forget Velikovsky's profession, there was at least one occasion on which I *did* address myself directly to the physician and psychoanalyst. I remember it quite vividly.

PATRICIA HEARST

The Patricia Hearst case has always fascinated me. I saw her as rather naive, somewhat maladjusted, not particularly bright, and decidedly ignorant. I also saw her as passive and pliable, and quite lacking in any strength of character or sense of self-worth. My assessment of her may well have been wrong. But she was certainly not a criminal, or a sociopath, or a terrorist. If she had been left to her own devices, probably the general public would never have heard of her.

Through no fault of her own, however, she suddenly became the *victim* of a heinous crime, and she spent many months in the hands of monsters, several of whom were quite brilliant. Anything that she did or said later was quite clearly a result of the brainwashing that she received from *them*. Thus I maintained that she was entirely innocent, by reason of that very brainwashing, and that she should have been charged with nothing.

One day in Princeton, just after lunch, the several others who had been with us at the kitchen table had gone, and Velikovsky and I were left alone at the table, though Mrs. Velikovsky was still doing something over near the refrigerator. There was a lull after we finished whatever it was that we had been discussing, and I chose that moment to ask Velikovsky, "Do you think that Patty Hearst was brainwashed?" For all that I knew, he might have believed that there was no such thing as brainwashing, but I was mildly curious as to what he might have to say.

Velikovsky, who had been quite calm, exploded into a vehement denunciation of the injustice that had been perpetrated in this case. I shall not attempt to quote him, but the articulateness and logic of his comments were no less striking than his outrage. In a manner worthy of Jeremiah, he left no doubt at all about what he thought. I wondered if the dishes in the cupboard would begin to rattle. Indeed, I wondered if I might have to peel him off the ceiling. Velikovsky's anger, of course, was directed not at me or at my question, but at the injustice of the criminal justice system. He thought that it was quite obvious that Patty Hearst had been brainwashed. He spoke at length, and most forcefully, on the nature of brainwashing. He also spoke of the sympathy that he felt for a father whose daughter had first been kidnapped, and had then been *prosecuted* for crimes that were the handiwork of others. At that point Mrs. Velikovsky also lamented the suffering that the parents had had to endure.

GIORDANO BRUNO

It took Velikovsky several minutes to get all this off his chest and to cool down. Velikovsky's delivery was with anger, but his remarks were logically and even beautifully articulated. One seldom sees so much rage and so much eloquence in one and the same discourse. As I think back on that incident, the combination of fire and light reminds me of the writings of Giordano Bruno.

Velikovsky, Toni Paterson, and I were all great admirers of Bruno. Toni's book on *The Infinite Worlds of Giordano Bruno* had been published in 1970. Velikovsky had long planned to write *Three Fires*, which would deal with Deigo Pirez, Michael Servetus, and Giordano Bruno, all burned in the same century. That project was never completed, but in his A.A.A.S. address, Velikovsky referred to "that firebrand, Giordano Bruno," and later quoted a passage from Bruno's *On The Infinite Universe and Worlds*. (See also *Mankind in Amnesia*, pages 66, 117-118.) For a number of years, usually in the history room, Velikovsky kept a framed picture of Bruno that Toni had given him. Sometime after his death, it was returned to Toni. She too is dead now, but Bruno remains on the wall of her kitchen.

NOT JUST STONEHENGE

Even though Velikovsky had quite cogent answers to my initial questions about Stonehenge, I continued to detect the same sorts of problems in other contexts. (As we have seen, the Stonehenge answer had involved showing that those parts of the structure that reflect the present status of Earth and the Moon date from *after* the time when Earth and the Moon acquired that status.)

In a short paper entitled "Do Ancient Calendars Contradict Velikovsky?", I presented evidence from several of those other contexts that I had found, all to the effect that ancient calendars may reflect a year of 365 days or 365¼ days and a synodic month of 29½ days, which would suggest that Velikovsky's near-collisions of planets within historical times could not have happened. I had broached these matters in my letter of August 30, 1973, and I then developed them in the form of that short article in April, 1974. (The main points made in "Do Ancient Calendars Contradict Velikovsky?" will be presented shortly.)

LOUIS GINZBERG

I also sent Velikovsky a 28-page set of notes that examined *all* of his references in *Worlds in Collision* to Louis Ginzberg's *Legends of the Jews*. Many of these notes were critical. I was able to cite any number of passages in *Worlds in Collision* where I thought that Velikovsky was mistaken, or that he had not made proper use of Ginzberg. (Yes, I was an *insufferable* nit-picker. I hope that I have mellowed somewhat.)

PEOPLES OF THE SEA

Even this effrontery Velikovsky bore with patience and understanding. Perhaps because he saw some indication of my industry and attention to detail, he decided to sic me onto a more current and therefore more useful project: he gave me the galleys of *Peoples of the Sea*, the concluding volume of the *Ages in Chaos* series, and asked me to inform him of any mistakes that I might find. I obliged, with a 47-page, single-spaced critique. Nearly half of this was concerned with the Supplement, which was entitled "Astronomy and Chronology."

(That was the point at which I *really* get into working on calendars: checking references, finding original texts and inscriptions, doing retrocalculations, and so on. I even dabbled a little in chronology! I still did not try to juggle all of the chronological data, but confined myself to whatever details I might have felt ready and able to handle at that point.)

I mention these things because many (myself included) have found it so puzzling that Velikovsky kept asking me for critical input while rebuffing others. His patience with my continual nit-picking during those early years was above and beyond the call of duty; it was "more than could be asked"—which is precisely how the term "supererogatory" is usually defined.

ASTRONOMY AND CHRONOLOGY

In the Spring of 1973, "Astronomy and Chronology" had been published as an article in *Pensée* IVR IV (pages 38-49). I had read it quickly, but had not checked anything. Now, with the publication of *Peoples of the Sea* imminent, I went over the corresponding section of the galleys with considerable care. I decided that the *Pensée* version was mistaken in a number of respects, and in need of a thorough overhaul. I set out to convince Velikovsky of that. With some difficulty, I succeeded. He thereupon *changed* many of the positions that he had taken in the *Pensée* version.

It is *only* the *Peoples of the Sea* version that represents Velikovsky's definitive position. I regret that certain of Velikovsky's putative supporters still cite only the *Pensée* version, even though they are well aware that it contains numerous errors that he had later *corrected* in the *Peoples of the Sea* version.

Most of my concerns about "Astronomy and Chronology" had to do with the testimony of Censorinus about the heliacal rising of Sirius in +139, the testimony of Theon about the correspondence of the Egyptian and Alexandrian calendars in -25, and the testimony of the Theon annotator about a heliacal rising in -1321. These matters will be taken up in a short while. But first let us review some of the basics.

ASTRONOMICAL VS. HISTORICAL DATING

I prefer to use astronomical dating rather than historical dating. The difference is that astronomical dating recognizes a year 0, while historical dating does not. Typically, astronomical dating uses a minus sign, while historical dating uses some such expression as B.C. or B.C.E. In any case, the year -237, which was the date of the Canopus Decree, is exactly the same as the year 238 B.C.E.

There are two advantages to astronomical dating. (1) *All* astronomical dates evenly divisible by 4 are Julian leap years. (2) Intervals spanning the beginning of this era are much easier to calculate; from -10 to +10 is 20 years, but from 10 B.C.E. to 10 C.E. (or A.D. 10) is only 19 years!

There are many stylistic variants that one encounters. For example, Velikovsky nearly always used historical dates, but he preferred to write them with a minus sign. Obviously, it is important to determine how any given author is handling this.

SIRIUS

At least since Earth came to be on its present orbit, the star Sirius has disappeared every spring, due to the fact that it has been too close to the Sun to be seen; then sometime in July it has reappeared, in the morning sky, just before dawn. This reappearance is called the heliacal rising, because Sirius virtually rises with the Sun. The Egyptians called Sirius Sopdet, and they spoke of the heliacal rising of Sirius as *pṛt Spdt*. The Greeks usually called Sirius the Dog-Star, because it is in the constellation of the Little Dog (Canis Minor), but they also called it *seirios*, which means scorching. That is how the days following the heliacal rising came to be known as the Dog-Days. The Greeks rather loosely transliterated the Egyptian Sopdet as Sothis.

THE JULIAN AND EGYPTIAN CALENDARS

The Julian calendar is exactly $365\frac{1}{4}$ days long. It has a leap year every four years, namely, in every year that is evenly divisible by 4. (If we use astronomical dating, this would also be true for years from before the common era. Thus -1872 would be a Julian leap year, but 1872 B.C.E. would not.) The Julian and Gregorian calendars use the same months. Indeed, the only difference between the Julian calendar and the Gregorian calendar that many of us use today is that the Gregorian calendar is designed to run a little shorter than $365\frac{1}{4}$ days. This is accomplished by recognizing slightly fewer February 29's, that is, by having slightly fewer leap years than there are in the Julian calendar.

The Egyptian calendar had exactly 365 days. The year was divided into twelve months of 30 days each, with five extra days at the end of each year. These days were called "the days upon the year" by the Egyptians. The Greeks called them *epagomenal* days, which means "on the months." There were three seasons, each containing four months. The three seasons were *Ḥt* (Inundation), *pṛt* (Coming-Forth) and *smw* (Deficiency). The months within each season were numbered. Thus III *pṛt* 17 would mean day 17 of the third month of the season of *pṛt*. Only in later times did any actual names appear for the months: the months of *Ḥt* were Thoth, Phaophi, Athyr, and Choiak; the months of *pṛt* were Tybi, Mechir, Phamenoth, and Pharmuthi; and the months of *smw* were Pachons, Payni, Epephi, and Mesore.

THE QUARTER-DAY

The Canopus Decree that was issued in -237, during the reign of Ptolemy III Euergetes, seems to have been the very first attempt to add a quarter-day to the calendar; it calls for a sixth epagomenal day every four years. This reform was not successful at the time. But when the Romans conquered Egypt, they

insisted that the Egyptians should recognize a sixth epagomenal day every four years, so that the Egyptians would be using a year exactly as long as their own newly-adopted Julian year. The result was what is called the Alexandrian calendar. It is just the traditional Egyptian calendar, but with a sixth epagomenal day in leap years.

Initially, there were some complications in the regulation of the Julian and Alexandrian calendars. Intercalation of the extra day was for a time carried out every three years, rather than every four years. Eventually, this was corrected, and both calendars ran smoothly. (Not that the Egyptian calendar was dropped; it continued to be used, too, right along with the others.)

QUADRENNIA

In studying the Egyptian calendar, we shall find it convenient to speak of quadrennia. An Egyptian-Julian quadrennium is a stretch of four years during which the same Egyptian day will always fall on the same Julian day. For example, Thoth 1 Egyptian = July 20 Julian, from +136 to +139. Because the Julian calendar treats +140 as a leap year, a new quadrennium will then begin; accordingly, Thoth 1 Egyptian = July 19 Julian, from +140 to +143.

TETRADS

It will also be convenient to use what I call a tetrad. This gives the four dates in July on which the heliacal rising of Sirius occurred during an Egyptian-Julian quadrennium. For example, the heliacal rising of Sirius at Memphis was on July 19 Julian in +136, +137, and +138, but was on July 20 Julian in +139. Thus the tetrad would have been 19-19-19-20 during the Egyptian-Julian quadrennium that lasted from +136 to +139.

SIRIUS MOVES THROUGH THE EGYPTIAN CALENDAR

The interval between one heliacal rising of Sirius and the next was just a little more than $365\frac{1}{4}$ days. The difference from $365\frac{1}{4}$ is so slight that the ancients seem never to have noticed it. That is, they seem to have thought that it was *exactly* $365\frac{1}{4}$ days. The reason that the Egyptians were interested in this sort of thing was that the heliacal rising of Sirius would occur on the same day of the Egyptian calendar for four consecutive years, and then move on to the next day of the Egyptian calendar for the next four years. At this rate, the heliacal rising would move all the way through the Egyptian calendar in $365 \times 4 = 1460$ years. This is called a Sothic period.

A four-year stretch of the kind just described amounts to an Egyptian-*Sothic* quadrennium. For example, the heliacal rising of Sirius would have occurred on Thoth 1 Egyptian in +139, +140, +141, and +142. Then a new Egyptian-Sothic quadrennium would have begun, and the heliacal rising of Sirius would have occurred on Thoth 2 Egyptian in +143, +144, +145, and +146.

Since the Sothic year was a little more than $365\frac{1}{4}$ days long, any Sothic period based upon it would have been a little less than 1460 years long. Thus Ingham (*Journal of Egyptian Archaeology* LV (1969), pages 36-40) has calculated that the Sothic period that is said to have ended in the second century of this era would have been 1452 years long. This means that eight of the Egyptian-Sothic quadrennia in that stretch would have been shortened to triennia: such a *triennium* would have occurred whenever the heliacal rising of Sirius was on the same day of the Egyptian calendar for only *three* consecutive years.

BENCHMARKS

It is from Censorinus that we learn that the helical rising of Sirius occurred on Thoth 1 Egyptian = July 20 Julian, +139. This is what implies that the tetrad for +136 to +139 would have been 19-19-19-20.

From the Canopus Decree, it emerges that the heliacal rising of Sirius occurred on Payni 1 Egyptian = July 19 Julian, -238. This indicates that the tetrad for -240 to -237 would have been 18-18-19-19.

Both of these tetrads pertain to Memphis. If the observation-post had been farther to the south, the date of the heliacal rising of Sirius would have been earlier by just about one day for each degree of latitude.

Ingham computes not only that the Sothic period ending in the second century of this era would have lasted 1452 years, but also that the previous Sothic period, ending in the fourteenth century before this era, would have lasted 1454 years. Since the Sothic periods were growing shorter over time, the placement of triennia would be squeezed a little toward the later end of each Sothic period.

The determinations of those two tetrads, 19-19-19-20 for +136 to +139, and 18-18-19-19 for -240 to -237, together with what we have learned from Ingham, finally put us in a position to deduce that the tetrad for, say, the first half of the nineteenth century before this era was 16-16-17-17.

While we can frequently specify the tetrad that would have been in effect during some given quadrennium, we can never calculate precisely *when* that tetrad would have changed to the next tetrad.

The succession of relevant tetrads would be like this:

16-16-17-17 (as from -1872 to -1869)
16-17-17-17
17-17-17-17
17-17-17-18
17-17-18-18
17-18-18-18
18-18-18-18
18-18-18-19
18-18-19-19 (as from -240 to -237)
18-19-19-19
19-19-19-19
19-19-19-20 (as from +136 to +139)

There are eight transitions between the 16-16-17-17 tetrad and the 18-18-19-19 tetrad, each representing a triennium. Three more triennia would have occurred between the 18-18-19-19 tetrad and the 19-19-19-20 tetrad. The *exact* locations of the triennia are unknown; triennia are much too delicate to be retrocalculated, and no observed triennium has ever been reported.

THEON

From Theon it appears that Thoth 1 Alexandrian = Thoth 1 Egyptian = August 30 Julian, -25. *All* that he is trying to tell us is that the Egyptian and Alexandrian calendars would have been in phase at that point. He is *not* speaking of Sirius at all.

"Since the year given us by the Greeks or the Alexandrians is of $365\frac{1}{4}$ days, but that of the Egyptians is, as we have said, of only 365 days, it is evident that in the course of four years the Egyptian year outruns the Alexandrian by one day, and in the course of 1460 years by 365 days – that is, by one Egyptian year. At that time that Alexandrians and the Egyptians again mark the beginning of their year together, as well as the days and the months – the time of the Egyptians having gained a whole year. And at the beginning of the next year the Egyptians again begin to gain by a fourth of a day, and similarly from then on. This so-called return [*apokatastasis*] through the 1460 years from some starting-point occurred in the first year of the rule of Augustus [in Egypt], so that from that time on the Egyptians have again gained a fourth of a day each year."

Not a word about Sirius or Sothic periods or heliacal risings or anything of the kind! It is of course the reference to "1460 years" that has confused people here. This is the 1460 years that it would take for the Egyptian and Alexandrian calendars – having started in phase, that is, with Thoth 1 Egyptian = Thoth 1 Alexandrian, Thoth 2 Egyptian = Thoth 2 Alexandrian, and so on – to get *back* into phase. This sort of cycle would have started in -25, when Thoth 1 Egyptian = Thoth 1 Alexandrian. A *Sothic* period would have started in +139, when Thoth Egyptian = July 20 Julian, the date of the heliacal rising of Sirius. It is only because the Sothic year is so very nearly equal to the Alexandrian year of $365\frac{1}{4}$ days that the number 1460 came up in both contexts. To the ancients, of course, there seems to have been no difference at all between a Sothic year and an Alexandrian year. (The former exceeded the latter by only about two minutes anyway!)

THE ERA OF MENOPHREUS

From the Theon annotator it appears that the heliacal rising of Sirius occurred on Thoth 1 Egyptian = July 20 Julian, -1321. (Notice that this is exactly 1460 years prior to +139.) Actually, what he says is that "from Menophreus" to the end of the Era of Augustus is 1605 years. Even this is rather awkwardly worded:

"A formula for the rising of the Dog Star. At the year 100 of [the Era of] Diocletian, with the aid of the formula for the rising of the Dog Star, we take the [years] from Menophreus to the end of [the Era of] Augustus Together the combined years [are] 1605, to which we add the 100 years from the beginning of Diocletian; together they amount to 1705 years."

The Era of Augustus ended in +284, with the beginning of the Era of Diocletian. This would put "Menophreus" in -1321. It has become customary, with no further justification whatsoever, to speak of an "Era of Menophreus" that began at that point. Of course, no one really knows who or what this Menophreus might have been!

DO ANCIENT CALENDARS CONTRADICT VELIKOVSKY?

We have now looked at the distinguishing features of some of the principal ancient calendars, as well as at the testimonies of Censorinus, of Theon, and of the Theon annotator about those key dates of +139, -25, and -1321, respectively. I would next like to include the most important paragraphs from my short unpublished paper, "Do Ancient Calendars Contradict Velikovsky?" The significance of this material is that, according to the conventional chronologies for Egypt and Mesopotamia, it would seem that *both* a year of 365 days (or even $365\frac{1}{4}$ days) *and* a mean synodic month of $29\frac{1}{2}$ days were recognized well before Velikovsky's Venus episodes – and even longer before his Mars episodes. If that is so, then Earth and the Moon have been undisturbed throughout the last 4000 years or so, and the principal ideas of *Worlds in Collision* cannot be true: there simply were no near-collisions between Earth and Venus, and there simply were no near-collisions between Earth and Mars.

The following eight paragraphs are adapted from "Do Ancient Calendars Contradict Velikovsky?" Please bear in mind that I was not *endorsing* all of what I wrote there; I was writing from the conventional perspective, and thus as devil's advocate. In the rest of this chapter, *much* of the anti-Velikovskian line that I articulated in that paper will be demonstrated to be false.

The Egyptian, Julian, Alexandrian, and Gregorian calendars are called solar calendars, in that their primary purpose is to follow the Sun rather than the Moon. By following the tropical year, they keep the same seasons of the year occurring in the same parts of the calendar, year after year. No effort is made to have calendar dates reflect the movements of the Moon. The "months" are merely schematic, and do not correspond at all to lunar movements.

The Babylonian calendar in the centuries just prior to the beginning of this era was of the type called luni-solar: it combined months that were strictly lunar with a variable year that averaged out to the same length as the tropical year. Twelve lunar months of 29 or 30 days produced a year of about 354 days. Every two or three years an intercalary lunar month was added to produce a year of about 384 days. By adding intercalary lunar months at the proper intervals, the Babylonians were able to keep their months lunar and their average years solar.

Worlds in Collision asserts that Earth, together with the Moon, repeatedly underwent near-collisions with other planets, most recently about twenty-seven centuries ago, and previously about thirty-four centuries ago. It is extremely unlikely that those near-collisions could have perturbed Earth and the Moon onto orbits such that the length of the year and the length of the month and the length of the day would have stood in the same ratios after those near-collisions that they had stood in before those near-collisions.

Indeed, Velikovsky himself has pointed out that the changes in the day, the month, and the year would tend to be rather drastic: he mentions the possibility of months of 20 days or of 36 days, and years of 320 days; he also mentions years containing ten months and years containing eleven months.

Thus if calendars that are more than thirty-four centuries old reflect the *present* movements of Earth and the Moon, this would constitute extremely strong evidence that Velikovsky's theory is false.

There seem to be many calendars of precisely this sort.

* * *

The Pyramid Texts are far older than thirty-four centuries. Velikovsky admits that they contain references to "five days," but he denies that these are the five epagomenal days of later times. He postponed this entire question to the sequel volume to *Worlds in Collision*, which was to deal with events in the period prior to the Exodus. Until his arguments are presented, however, it seems reasonable to retain the view that the "five days" of the Pyramid Texts *are* the five epagomenal days of the 365-day Egyptian year.

There are repeated references to 360 days and to the five epagomenal days in various documents of the Old and Middle Kingdoms. This suggests not only that there was no disruption of the orbits of Earth and of the Moon between the Old and the Middle Kingdoms, but also that there has been no such disruption since then.

Star clocks (dating from the time of the Middle Kingdom and even earlier) refer to twelve months, and provide star lists that may be used to tell time during the night. A different list is provided for each third of every month. At the end of these lists there seems to be a special set of stars for use during the five epagomenal days at the end of the year.

Numerous contracts and other documents have survived from the First Babylonian Dynasty. These pre-Exodus documents show that the Babylonian year consisted of twelve months, about half of which seem to have had 30 days, and the remainder of which may have had only 29 days. They also show that the Babylonians added an intercalary month every two or three years.

There was no particular pattern to these intercalations, and sometimes successive years were intercalary, but in the long run the number of intercalations was about right. The difference between the tropical year—365.2422 days—and twelve lunar months— $12 \times 29.53 = 354.36$ —is about 10.88 days, and 29.53 divided by 10.88 is about 2.7. The fact that the First Babylonian Dynasty followed this approximate rate of intercalation suggests very strongly that their month and their year were substantially the same as ours.

* * *

Thus it seems that the ancient Egyptian and Babylonian calendars dating from prior to the Exodus were adapted to the *present* arrangement of the solar system, and that neither Earth nor the Moon has changed its orbit in the meantime.

Velikovsky and I had no good answers to any of these difficulties, but we did discuss several general approaches that seemed to recommend themselves. (1) The sources might have been misreported or misinterpreted. (2) The sources might have been "corrected" by well-meaning uniformitarians. (3) The sources, and the civilizations to which they belong, might have been grossly misdated.

Velikovsky and I did not discuss the Old or Middle Kingdoms of Egypt, but we did discuss the First Babylonian Dynasty. For some years, Velikovsky had thought that it might belong in the first millennium, but he had no satisfactory proof. On the basis of what I had seen of First Babylonian Dynasty documents (those allegedly related to the Venus tablets that Ray Vaughan and I were studying), I was inclined to agree, but I had no proof either.

* * *

Throughout the next eighteen years, my studies of calendars continued. But most of the problems posed by "Do Ancient Calendars Contradict Velikovsky?" were never really addressed, at least not until January of 1992. By then, I thought that my book, *Sun, Moon and Sothis*, was nearly finished, except perhaps for an appendix or two on the El-Lahun Sothic date and related matters. I was wrong. Those few short appendixes virtually exploded into what is surely the most revolutionary part of the book. What had begun as a "calendars" project now metamorphosed into a "chronology" project as well.

PART TWO: LOCATING THE VELIKOVSKY DIVIDE

"YOU HAVE AN ANSWER"

On March 20, 1972, when Velikovsky had concluded his address at the State University of New York College at Buffalo, one of the first to get to the floor with a question was a confused young man sporting a shaved head, designer eyeglasses, a nice new robe, and some very old sneakers. It was obvious that he had grown up in North America, perhaps even in Buffalo. The politically incorrect though nearly universal term for such a person at the time was "Krishna freak."

This fellow launched into an exposition of the wisdom he had recently attained. I hoped that Velikovsky or someone else would shut him up, but he went on and on. It was probably only a minute or so, but it seemed interminable. He had quite a struggle getting everything out. This might have been stage fright, or the charred synapses, or maybe that his *Weltanschauung* was not nearly as vast as he had supposed. Suddenly he was finished. Of course, none of his remarks had anything whatsoever to do with the topics at hand.

I was angry that Velikovsky would have to put up with this sort of thing, but I need not have worried. He simply said, to the delight of the audience: "You do not have a question; you have an answer." Then he called on someone else.

THE CAUTIOUS PLODDER

Velikovsky himself was always slow to find answers. Despite his reputation for offering answers about this, that, and everything, he was in reality very cautious, and something of a plodder. Much of his life's effort was devoted to getting a handle on the facts.

There has always been a surfeit of theories. Velikovsky's principal task was to distinguish those theories from the actual facts.

Look up Ramses II in a careful book and you may learn that he inherited the throne of Egypt from his father Seti and that he reigned from -1289 to -1223. Look him up in a less careful book and you may also be told that he was the pharaoh of the Exodus, *à la* Yul Brynner and Cecil B. De Mille. What is fact here, and what is but theory? It takes a great deal of effort to sort it all out. The so-called facts are usually "theory-laden," as N. R. Hanson put it—or, as Giordano Bruno had put it nearly four centuries before him, they result from our own "*intenzioni*." To separate the facts from the sometimes bizarre theoretical contexts in which they are imbedded is no easy matter.

A RECONSTRUCTION, NOT A THEORY

"My work is first a reconstruction, not a theory," said Velikovsky in his A.A.A.S. address. I would find it quite difficult to avoid the word "theory" in speaking of what Velikovsky offered us, since in some sense even a reconstruction is a kind of theory; nonetheless, I do appreciate Velikovsky's point, and I recommend that all of us adopt it as a guiding principle in our own work. We should pay more attention to facts, and less attention to theory development. We need more questions, and fewer "answers."

I might remind you that *Ages in Chaos*, Volume I, gives us no absolute chronology, but leaves everything up in the air. Even the subtitle (at least the one on the dust jacket) uses the word "reconstruction":

this work is to be "A reconstruction of ancient history from the Exodus to King Akhnaton." But only a *relative* chronology is worked out. The chronologies of Egypt and Israel are successfully meshed, from the Exodus to Akhnaton, but it is *not* shown whether Egyptian history is 600 years too long or Hebrew history is 600 years too short.

To Velikovsky, *both* of these alternatives seemed unacceptable. Thus we are told on page 99:

"No student of ancient history will see the slightest possibility of altering the history of the kings of Jerusalem by a single century, much less by six, without disrupting all established data and concepts."

In *AEON I*:6, page 50, Clark Whelton has quoted just this one passage, perhaps leaving the impression that it alone represented Velikovsky's opinion. In the interest of completeness, however, there are at least several other passages that must also be quoted. Farther down that same page, Velikovsky says that "Hebrew history is closely related to Assyrian history." Then he goes on to say (pages 99-100) that

". . . the dates of events in which Assyria or Babylonia participated are, in a number of instances, established with precision, to within a year.

"The period of the kings of Jerusalem ended with the exile to Babylon in the days of Nebuchadnezzar, who destroyed Jerusalem in -587 or -586. Cyrus, the Persian, in the second half of the same century, conquered the Chaldean-Babylonian Empire. The Persian rule, king after king, the years of each of them known from many contemporary Greek authors, lasted until Alexander the Great. Where are the six hundred years to be inserted?"

There are two points that must be emphasized here.

(1) Velikovsky himself stresses the linkage between Hebrew history and the histories of Assyria, Babylonia, and Persia. Shift Hebrew history, and you must shift those others. Keep Hebrew history in place, and you must keep those others in place. (This point has mainly to do with the first millennium.)

(2) But that is only one side of the coin. It is equally difficult to imagine any shortening of Egyptian chronology. Thus the *other* side of that same coin must also be quoted, from pages 100-101:

"If the fault lies in Egyptian history, the only possibility is that events of that history are described twice, and six hundred years are repeated. It would follow, then, that the events of many others peoples are described in wrong succession. But this seems to be a preposterous statement, which insults the sound judgment of many generations of the entire scholarly world, whoever learned, investigated, wrote, and taught history.

"Both these alternatives appear to be chimerical: that six hundred years disappeared from the history of the Jewish people, or that six hundred years were doubled in the history of Egypt and in the history of many other peoples as well."

Despite the impression left by Whelton, Velikovsky is not in these pages asserting the impossibility of lengthening Hebrew and Mesopotamian history by 600 years. Rather, he is describing *both* the apparent impossibility of lengthening Hebrew and Mesopotamian history by 600 years *and* the apparent impossibility of shortening Egyptian history by 600 years.

Look again at his own words (page 101):

"Both these alternatives appear to be chimerical: that six hundred years disappeared from the history of the Jewish people, or that six hundred years were doubled in the history of Egypt and in the history of many other peoples as well."

There is equal-opportunity impossibility here: these are *matching* chimeras.

Even when Velikovsky had reached the end of *Ages in Chaos*, Volume I, he still did not claim to have established an answer to his own questions. Thus on page 338 he admits that:

". . . we still do not know which of the two histories, Egyptian or Israelite, must be readjusted."

The resolution of this had to await the later volumes of the series. Only then could the solution be found.

By 1952, of course, and even by 1945, when *Theses* was published, Velikovsky knew very well where those subsequent volumes would lead, but the point to be noted is that in his investigations during the early 1940's he *did* follow the sequence of thought that he elaborates in the *Ages in Chaos* series: he worked forward in time, generation by generation, sometimes even year by year, trying to establish links between the two histories. And he could not be sure how it would all end until he got to the end.

Besides, there is no sweeping theory here; no *a priori* determination; no flight of fancy. There is just a careful reconstruction, based upon his having sifted through the myriads of facts in an effort to see how they might mesh.

His followers should be so cautious.

WANTON REVISIONISM

Over the years, I have grown increasingly skeptical about much of the research and theorizing done under the Velikovskian banner. It seems to me that a wanton proliferation of alternatives to Velikovsky has been undertaken much too quickly and without nearly enough advance preparation. Also, those who do engage in this sort of thing usually have to presume that vast areas of Velikovsky's reconstruction are *passé* and need to be replaced. Such revisionism ought to proceed at a careful crawl, not a wild gallop.

These putative Velikovskians need to spend far more of their time studying the evidence, and they need to spend far less of their time inventing all of those vast theories that have come to litter the intellectual landscape during the last several decades.

For the most part, these "Velikovskian" colleagues of mine will remain nameless. The unwanted shoes that I am about to cobble will fit many unwitting Cinderellas, named or not.

* * *

I suppose that one should distinguish those who are simply out to find fault with Velikovsky from those who in addition seek to offer a reconstruction of their own. After all, some of the latter do show at least *some* imagination—some of the time.

(The former simply need to get a life. If they continue to spend all of their time and energy beating what they themselves consider a dead horse, perhaps it is because, deep down, they believe that the horse is not dead. Why else would they devote their lives to such an idle and worthless activity? Especially when they themselves keep claiming that the horse is indeed dead!)

As a recovering nit-picker myself, I might suggest that to nit-pick a true theory, with a view to making it nit-free and stronger, is quite sensible. But why nit-pick a theory that you consider false? Nits do cry out for picking, I suppose, but there are usually much more important matters to attend to.

Those who behave in such a manner, doing lots of nit-picking and little else, and with respect to a theory that they consider false, are actually *proving* the very theory that they seek to disprove. They themselves *are* evidence—albeit distasteful evidence—that that theory is correct. If they persist in attacking Velikovsky for no apparent reason, perhaps there *is* a reason: namely, that Velikovsky is right and that they cannot stand it.

PROPER NIT-PICKING

But the fact is that I now view with disdain and contempt *all* of those who seek to catalogue Velikovsky's alleged errors. It has been my experience that most of Velikovsky's actual errors are quite trivial, and that none of those errors affects the correctness of his overall reconstruction.

Indeed, I would like to contrast my own behavior in this area with that of certain others, and to suggest, rather immodestly, that my behavior be accepted as a standard and paradigm that all other Velikovskians would do well to follow. Readers will recall that I had once sent Velikovsky a 47-page, single-spaced critique of the galleys of *Peoples of the Sea*, and that, even before that, I had sent him a 28-page analysis of his uses and sometimes misuses of Ginzberg in *Worlds in Collision*. I did this for his own information, and I considered it *worth* doing because his theory seemed to me to be correct. All that I wanted to do was to make Velikovsky himself aware of whatever I had found, and to try to make his future publications even stronger. If I had not believed that his theory was correct, I would not have bothered. I did not even *think* about publishing this material. Any idiot can pick nits. If that is *all* that some people can do, then clearly they *do* have some sort of mental deficiency, and they might better expend their energy in other directions, such as clearing brush.

THE TYPICAL REVISIONISTS

As for those who do have a reconstruction of their own to offer, they come in many varieties, but there seems to be one fairly common pattern to their work: quite a few of them throw out the later part of the chronological reconstruction—say, all that lies after the Eighteenth Dynasty—and they also throw out the later part of the astronomical drama—in other words, they throw out the Mars scenario, which Velikovsky

called the final act of the cosmic drama, and which is described in Part II of *Worlds in Collision*. There are variants, of course, and there are many alleged "Velikovskians" who profess to have thrown out everything (why are they still here?), but the following seems to be a typical and fairly frequently encountered pattern in recent thought: keep the Venus scenario, that is, keep Part I of *Worlds in Collision*, which Velikovsky called the first act of the cosmic drama described in that book, and also keep the first volume of the *Ages in Chaos* series; but throw out Part II of *Worlds in Collision*, that is, throw out the second act or the Mars scenario, and also throw out each of the remaining volumes of the *Ages in Chaos* series.

I am not suggesting that *all* of the dissident Velikovskians do this, just that quite a few of them seem to fall into this sort of pattern.

MARCH 23, -686

Part of the attack on the Mars scenario has to do with the quality of the evidence for March 23 Julian, -686. *Both* the March 23 Julian *and* the -686 itself have been challenged. Was -686 indeed the year of the last interaction between Earth and Mars? Even some of Velikovsky's supporters have preferred to put that last interaction somewhat earlier, say, in -701 or in -716. My concern is not with these details, but with the general point that it was *somewhere* in that vicinity (or even a little earlier) that Earth seems finally to have been pulled onto what was essentially its present orbit. That is, the last near-collision between Earth and Mars was *about* twenty-seven centuries ago (or even a little earlier). But perhaps the exact date can be left open, as far as our purposes here are concerned.

While I remain *unpersuaded* that the -686 date is incorrect, and while I continue to use that -686 date as a working hypothesis in much of my research, I do at the same time remain completely flexible on the question of that last encounter between Earth and Mars. As far as I am concerned, it just *might* turn out to have been in -746, or even in -775! The last word on that event has not yet been heard. But whenever it was, we cannot retrocalculate the orbits of Earth and the Moon past that point.

RETROCALCULATING

With only a few exceptions, the ideas that I am about to broach are simple and clear. But I admit that they were neither simple nor clear to me, until quite recently. I studied the Velikovsky theory for 25 years before they became simple and clear to me. I hope that I can enable my readers to view them as simple and clear, too, and they will be spared the 25 years that I needed in order to think these things through.

* * *

For the times since that last near-collision between Earth and Mars, we *can* retrocalculate orbits. Mars would have been a temporary exception, in that it may not have been thrown onto *its* present orbit until some while *after* -686 (or whatever). This matter will have to be discussed in more detail in a later section.

For the times that are prior to, say, the late eighth or early seventh century, or whenever it was that Earth was pulled onto its present orbit, we of course cannot retrocalculate *anything* that involves Earth's orbit. But we might be able to retrocalculate certain *other* things that do *not* involve Earth, such as the movements of Jupiter and Saturn—or even some of the eighth-century movements of Venus, provided that we can ascertain that Venus was by then on its present orbit. (These matters, too, will receive more detailed treatment in a moment.)

Wherever this line is to be drawn, it represents a barrier to retrocalculation, and thus amounts to what we might think of as a *Velikovsky Divide*.

THE VELIKOVSKY DIVIDE

Almost immediately, I had begun referring to this crucial but rather imprecise point in time as the Velikovsky Divide. But when I noticed how that would inevitably be abbreviated, I began calling it the Great Historical Divide! Now I am once again back to calling it the Velikovsky Divide, which seems to me to be the only appropriate label.

In a loose sense, the Velikovsky Divide might refer to whatever limit to retrocalculation we happen to have in mind on any given occasion. In a stricter sense, however, the Velikovsky Divide would be marked by the *last* major orbital perturbation, the point at which the planetary system came to be essentially as it is now.

My criticisms of various "Velikovskians" will be couched in terms of this notion of a point of transition to the present astronomical state of affairs, as well as in terms of the notion of the last Earth-Mars near-collision as the cut-off point for most retrocalculation. At first glance, it might be tempting to think of the Velikovsky Divide itself in terms of Earth's last encounter with Mars. Indeed, I thought of it that way for quite some time. But the cut-off point for retrocalculation will *vary* from planet to planet.

For Saturn, Jupiter, and Mercury, we could presumably retrocalculate orbits much farther into the past, perhaps several more millennia, depending upon our working hypotheses.

Even Venus might be retrocalculated for a century or so prior to the last Earth-Mars near-collision—either back to the last Mars-Venus encounter or possibly back to the petering-out of the Sherrerd effect, whenever that was. (See Sherrerd, "Venus' Circular Orbit," *Pensée* IVR I, page 43.) This Sherrerd effect is essentially a matter of tidal friction. Thus if a planet is very hot, *and* is highly plastic, *and* has a very slow rotation, its orbit will tend to become circular:

"If such a body approaches the intense gravitational field near the Sun on an elliptical orbit, considerable energy of motion will be converted by tidal friction into heat. This will: a) tend to keep the body plastic or molten; and b) change the orbit to that, by the laws of Cassini, which minimizes energy loss by tidal friction. That is, the effects of tidal friction tend to decrease the magnitude of the velocity at nearest passages, which in turn reduces the ellipticity of the orbit. If the spin rate is high (short day), such an orbit will be still elliptical, with a shifting perihelion and slowly decreasing major axis; if in addition the initial orbit and equator are severely non-coplanar, strong librations will also result. If the spin rate is very slow, on the other hand, that orbit will be nearly circular."

Since Ray Vaughan and I have found that the Ninsianna tablets seem to show a Venus that was already on its present orbit, it is reasonable to suppose that any Sherrerd effect had probably petered out by, say, very early in the eighth century. In any case, Venus could be retrocalculated considerably farther back than Earth.

* * *

We have not yet determined just when or how Mars arrived at its present orbit, but it seems clear that Mars cannot be retrocalculated quite as far back as Earth. Something must have happened to change the orbit of Mars, *after* that last encounter between Earth and Mars.

Strictly speaking, however, if we wish to refer to the transition to the present arrangement of the entire planetary system, that would include Mars, and we would have to put the Velikovsky Divide itself somewhat later than the last Earth-Mars near-collision.

For us, however, it is Earth that is most important, so I suggest that we continue to use the last Earth-Mars near-collision, whenever that was, as the cut-off point for most of our retrocalculations. In a loose sense, *that* can be our Velikovsky Divide!

But it needs to be emphasized that that last Earth-Mars near-collision was *not* what ended the string of near-collisions: there must have been a *later* event that brought an end to the Age of Theomachy. In the strictest sense, it is that later event that marks the actual Velikovsky Divide. And *its* date has not yet been established.

THE PRINCIPAL SIGNIFICANCE OF THE VELIKOVSKY DIVIDE

The significance of the Velikovsky Divide is that it might be thought of as a boundary between that earlier era, when there was an entire sequence of near-collisions, and the present era, when there seems little chance that any of the planets will collide. Thus the Velikovsky Divide is a very *real* and vitally significant divide, not just some point that happens to represent the last near-collision before our own time.

For purposes of calendars and chronology, however, it is the point after which Earth and the Moon were on their present orbits that counts. *That* is the *terminus a quo* for all of our retrocalculations of years and months. Thus I shall be referring to *that* point as the practical limit of retrocalculation in the rest of this essay. Since we are operating from a terrestrial vantage-point, what usually counts for *us* is how far back we can retrocalculate Earth and the Moon.

In what follows, the context will very often be loose enough that it is not even necessary to specify whether we mean the point where this or that planet reached its present orbit, or whether we mean the point where all of the planets had reached their present orbits. Thus it may not even be necessary to specify whether we mean to refer to a last near-collision between Earth and Mars in -775, -746, -716, -701, -686 — or to the eventual arrival of Mars upon its present orbit. The expression, "the Velikovsky Divide," can often be used very generally, so as to allude to *all* of these indiscriminately.

THE UTTER POINTLESSNESS OF HISTORICAL PROBABILITY CALCULATIONS

Are not some of these astronomical scenarios rather improbable? Yes, indeed. But the issue here is not one probability, but of historical fact. Theories of mathematical probability were never intended to assess the likelihood of some particular event, by listing its detailed characteristics and then computing the probability that all of those characteristics might occur together by chance. If you do do that, the probability will of course become vanishingly small. Does that mean that the event did not happen? Certainly not! *Every* event that has *ever* happened, in the entire history of the universe, would be disqualified by any such "logic" as that.

Sagan in particular likes to take some event recognized by his opponents, list all of its distinguishing characteristics, and then multiply all of their allegedly "independent" probabilities into minute oblivion.

Such "logic" makes a travesty of serious inquiry, especially in the hands of someone like Sagan.

(Many people have wondered whether Sagan knows better, and is a liar, or does not know any better, and is a fool. I cannot really say. Perhaps it does not matter, so long as we recognize that his is a *shameful* misuse of probability formulas.)

Ironically, there is a quite *legitimate* procedure that is being perverted here. The probability that a coin will come up heads is $\frac{1}{2}$. The probability that two coins will come up heads is $\frac{1}{2} \times \frac{1}{2} = \frac{1}{4}$. The probability that three coins will come up heads is $\frac{1}{2} \times \frac{1}{2} \times \frac{1}{2} = \frac{1}{8}$. The more such independent probabilities you multiply together, the smaller the product. But what is the probability that Abraham Lincoln would have been shot on April 14, 1865, in Ford's Theater, while watching a play called *Our American Cousin*, and so on, and so on, and so on? Assuming that it even makes sense to assign numerical probabilities to all of these distinct circumstances, now multiply them together, and the probability of the event in question will shrink so low as to appear virtually impossible. *Every* event that has ever occurred is of precisely that sort. The probability that *any* such event could ever have occurred is vanishingly small. Yet they all happened. We must always consider what *did* happen, and not get lost in Sagan's parlor tricks.

BLIND SUPPORT

The Velikovsky Divide has been unwittingly recognized even by establishment writers. They seem to sense that on this side of the Velikovsky Divide the reports of astronomical events usually make good sense (read: they fit the present orbits) and that on the other side of the Velikovsky Divide the reports of astronomical events do not make sense (read: they do not fit the present orbits).

Bryant Tuckerman's 1962 book is entitled *Planetary, Lunar, and Solar Positions 601 B.C. to A.D. 1 At Five-day and Ten-day Intervals*. The 601 B.C. that he takes as his starting-point is actually a round number, or at least equivalent to one, since 601 B.C. historical (without a year 0) is the same as -600 astronomical (with a year 0).

Tuckerman indicates that he carried out his project in consultation both with Otto Neugebauer and with Abraham Sachs. Apparently it was on their advice that he chose to suspend his retrocalculations at -600. Neugebauer and Sachs are typical of those who believe that almost all of the more interesting astronomical observations are from later than that. Characteristically, Neugebauer gives even the Ninsianna or Venus observations short shrift: "From the purely astronomical viewpoint these observations are not very remarkable" (*The Exact Sciences in Antiquity*, page 95).

But what if those records from earlier times reflect what was really going on then? Shouldn't we be asking what astronomical circumstances are *implied* by those records from earlier times? It is Velikovsky's position that the astronomical arrangements that we see today have been in effect only since the seventh century before this era. To reject all of the records from earlier times, just because they reflect astronomical arrangements that are different from those obtaining at present, would be entirely unjustified.

The Ninsianna or Venus tablets are themselves to be counted among those earlier records that are incompatible with what we see at present. Vaughan and I consider the Ninsianna observations "very remarkable" indeed. They have been the subject of our long (and on-going) series of articles. We have even concluded that they derive from a time during which Earth was not yet even on its present orbit!

Sachs, who shared in the advising of Tuckerman, was as contemptuous of the Venus tablets as was Neugebauer. Sachs was cavalier, careless, and grossly irresponsible even in his *cataloging* of the four Venus tablets that he included in his *Late Babylonian Astronomical and Related Texts* (1955). This was the subject of an unpublished paper by Vaughan and me, entitled "How Reliable Is The Scholarship of Abraham Sachs?" The

answer, of course, is: "At least in *this* case, not very!" We have lately softened that title to "Sachs and the Four Venus Tablets." But it is nonetheless striking that so many extremely intelligent and learned people, possessed of the best brains that this planet has ever produced, can wane into scotoma and incompetence whenever they are dealing with materials indicative of catastrophism—especially if the name Velikovsky is mentioned.

* * *

As we have seen, the Babylonian "diary" texts indicate the positions of the Sun, the Moon, and the planets against the background of constellations. These texts usually cover one-half of a year, either the first six or seven months, or the last six or seven months, but sometimes they cover an entire year. When Neugebauer and Sachs were advising Tuckerman about his project, the earliest Babylonian "diary" text that was then known was VAT 4956, the one for the year -567/566 (= Year 37 of Nebuchadnezzar). It is probably for that reason that Neugebauer and Sachs wanted to have the cut-off point at the year -600. Subsequently, however, the "diary" text for the year -651/650 was identified by Sachs. If Neugebauer and Sachs had known of this older "diary" text, B.M. 32312, they might have advised Tuckerman somewhat differently. Clearly, Tuckerman's calculations should have been carried back *at least* an additional fifty-one years.

Velikovsky's -686 is now looming ominously close. In an unintended way, the attitudes of people like Neugebauer and Sachs even seem to corroborate Velikovsky's proposals. Their behavior is dictated by the presence of a great historical divide, whether they choose to recognize its existence or not.

The real reason that Neugebauer dismisses the Ninsianna observations is that he cannot make sense of them in terms of the present orbital arrangements. If he had ever dreamed that perhaps he *could* explain them on the basis of the present orbital arrangements, he would no doubt have advised Tuckerman to carry his retrocalculations—especially for the Sun, the Moon, and Venus—all the way back to, say, -2000. That would safely encompass all of the conventionally-recognized epochs of the Ninsianna observations. But Neugebauer's choice has always been to ignore any of the earlier astronomical reports, such as the Ninsianna observations, and to study only those later documents, the ones that seem to make sense in terms of the present orbits.

It may not have become all that apparent yet, but I have great respect for Neugebauer and Sachs. (A later section will be entirely devoted to the "rehabilitation" of Sachs.) Even Neugebauer's negative attitude toward the Ninsianna observations can be taken as corroboration of the fact that he is far ahead of those of his fellow uniformitarians who keep hoping to reconcile the Ninsianna observations with the present orbits. Neugebauer is almost alone among uniformitarians in recognizing that retrocalculation is of no value here, and that the Ninsianna observations cannot fit *any* retrocalculated orbits, no matter what epoch we might choose. Neugebauer will have no part of the quixotic project of trying to date the First Babylonian Dynasty by fitting the Ninsianna observations to retrocalculated orbits.

Many of my own investigations—such as my work on the calendars of Hellenistic and Roman Egypt—have also been concentrated on materials from later times. But my motivation is altogether different from that of people like Neugebauer and Sachs, in that I am avowedly a catastrophist. Only when it is a matter of dealing with documents from *this* side of the Velikovsky Divide is retrocalculation a legitimate tool. Indeed, for the study of these later materials, retrocalculation is not only very effective, but even indispensable. My copy of the Tuckerman book is quite well worn! So too the Langdon-Fotheringham-Schoch book, the Goldstine book, and several more that have to do with retrocalculation.

One other very useful book is by Parker and Dubberstein, and is entitled *Babylonian Chronology 626 B.C. -A.D. 75*. The authors list the first days of all of the lunar months within that 701-year stretch. Their "626 B.C." may not seem such at first, but it too is a round number, or equivalent to one, since 626 B.C. (historical) is the same as -625 (astronomical). Why would a book like this begin so soon after the time of the Velikovsky

Divide? All of the calculations of Parker and Dubberstein are based on the tables of Carl Schoch. Those tables can be utilized all the way back to 4000 B.C.; Schoch was trying to encompass just about any period that historiographers might conceivably be interested in. But Parker and Dubberstein *choose* to begin at -625. That decision is entirely their own, and it is just one more example of how uniformitarians display an unwitting respect for the Velikovsky Divide.

HYPERBOLE

Uniformitarians routinely *claim* that documents from the other side of the Velikovsky Divide fit the present astronomical arrangements. This usually involves either hyperbolic speculation or blatant lying.

In the case of the Venus tablets, as handled by such ne'er-do-wells as Huber, much lying is needed in order to achieve even the illusion of a fit between the Venus tablets and the present orbits. (Huber's charlatantry is discussed in the A.A.A.S. section.)

THE KUDURRUS

In the matter of the Kassite *kudurru*s, for example, there has been much frivolous and groundless speculation about the identities of planets, stars, and constellations, and about the significance of the various juxtapositions in the text. All this is needed in order to create the illusion of a fit between the *kudurru* and the present orbits.

Because an alleged icon of a planet is above an alleged icon of a constellation, even though the latter is three lines below the former, some "scholars" claim that the planet must have been *in* the constellation at the time the *kudurru* was issued!

TOUCHSTONE

The Velikovsky Divide serves as a convenient and simple criterion for rough-dating societies and cultures. Once it is determined that a given document or monument has a well-established text and has a well-established provenance within some society, that society can be placed on one side or the other of the Velikovsky Divide on the basis of whether the implied astronomy of the document or monument is or is not consistent with the present astronomical arrangements.

For example, any peoples who refer to years of $365\frac{1}{4}$ days or to months of $29\frac{1}{2}$ days must have lived on *this* side of the Velikovsky Divide. Using that touchstone, we can determine that the Middle Kingdom, the very latest portions of the Old Kingdom, and the later Dynasties of the New Kingdom all belong on this side of the Velikovsky Divide. So does the First Babylonian Dynasty.

Thus the Velikovsky Divide affects not only astronomical matters, but chronological matters as well!

CALENDARS AND CHRONOLOGY

We now see that calendars and chronology are inseparably *linked*, and in a very simple way. Calendars tend to reflect the astronomical situation obtaining at the time: the length of the month, the length of the year, and so on. The calendars will normally be from a period of time when those were the conditions that actually obtained. If there is a clash or conflict, then we need to take very seriously the possibility that

both the calendars *and the society that used them* may have been misdated by historians. This gives us a very powerful criterion to use in correcting some of the large-scale errors of the historiographers.

THE EGYPTIAN FOUNDATION

The conventional second-millennium chronology for many of the peoples of the eastern Mediterranean rests upon Egyptian chronology. Thus Egyptian chronology is generally regarded as being the only solid and *absolute* dating scheme that is known for any of those earlier peoples. This is mainly because the entire *sequence* of Egyptian history is (thought to be) recorded, from the earliest times right down to the fourth century.

Consequently, other ancient peoples, who might very well have a *relative* chronology of their own, are often dated by way of their interactions with the Egyptians. The Mycenaeans, for example, are placed in the mid-to-late second millennium because there are good indications that the Mycenaeans were contemporary with the Eighteenth Dynasty of Egypt. And the First Babylonian Dynasty is put in the early second millennium at least partly because of the extensive archaeological evidence that the First Babylonian Dynasty was roughly or at least nearly contemporary with the Twelfth Dynasty of Egypt.

What is it that permits this *absolute* chronology for the Egyptians? As we have seen, Velikovsky identifies "three pillars" upon which Egyptian chronology has been built. These are the Sothic Period pillar, the Menophreus pillar, and the Manethon pillar.

The Menophreus pillar may be not quite as important as the Sothic Period pillar or the Manethon pillar, but it seems to me that Velikovsky is correct in tracing the principal errors in Egyptian historiography to these three pillars. There is no need to be looking for any other pillars.

ABRAHAM SACHS

Ray Vaughan dislikes polemical attacks; thus our joint writings have usually taken a somewhat higher road than I might have chosen for myself. I can recall only two occasions when Vaughan felt that extremely harsh language was justified.

(1) He urged me, and in fact helped me, to toughen up the language in my "Just Plainly Wrong': A Critique of Peter Huber," especially about Huber's dishonest claims regarding the attested intercalary months during the reign of Ammisaduqa. In order to show me exactly what he had in mind, Vaughan even *drafted* the sort of remarks that he thought I should make. I edited them slightly, and then, with his blessing, *used* them. (That's OK; there have been at least a couple of sentences of his that were actually drafted by me!)

(2) He went along—albeit reluctantly—with "How Reliable Is the Scholarship of Abraham Sachs?" as a title for one of our unpublished papers. He had come to believe that the manner in which Sachs had treated the four Venus tablets in his *Late Babylonian Astronomical and Related Texts* (1955) fully justified this sort of language.

* * *

With Vaughan's permission, I now summarize the main points of our critique of Sachs' handling (or mishandling) of the Venus tablets in LBAT.

(It is on page xxxvi that Sachs describes these four fragments. But there are also drawings of all four of them, by Pinches, on page 249.)

Sachs compares B.M. 34227 to Section I, which is the first of the two Chronological Sections of the Ninsianna document; in fact, B.M. 34227 is from Section IV, which is the Listing by Months that is sometimes included as part of the overall Ninsianna document.

Sachs seems also to compare B.M. 42033 to Section I; in fact, B.M. 42033 is the Corrigenda Section of the Ninsianna document.

(Sachs does detect that B.M. 42033 + B.M. 34227 is a legitimate join. What is most interesting, however, is that, as far as Sachs is concerned, this join is largely a jig-saw matter. Except for the fact that both of them were Ninsianna fragments, Sachs does not rely on their detailed content—which he failed to grasp anyway.)

Sachs is unable to tell the obverse from the reverse²¹³⁴ of B.M. 41498. He first calls the reverse the obverse, and identifies it as a "Variant" of Section I; in fact, it belongs in Section IV, the Listing by Months. He then calls the obverse the reverse, but he has no idea where to put that; it is the obverse, if anything, that belongs in Section I.

B.M. 41688 contains the very end of Section II, which is the Artificial Insertion of the Ninsianna document; next it lists all of the entries from Section III, which is the second of the two Chronological Sections; then comes the footing of Section III; and finally the first part of the Corrigenda Section. Sachs does know where most of B.M. 41688 belongs, but so does everyone else: those four entries of Section III—Years 19, 20, 21a, and 21b—would be almost impossible for anyone *not* to recognize, especially since Year 19 contains a rather conspicuous intercalary month. Nonetheless, Sachs still messes up: he compares B.M. 41688 to K. 160, which had Sections I, II, and III, and a Colophon. But B.M. 41688 is from a different sort of document, one that had Sections I, II, and III, a *Corrigenda Section*, and *then* (presumably) a Colophon. The presence here of the Corrigenda Section escapes Sachs completely.

* * *

Sachs actually had *two* blind spots:

- (1) His inability to describe correctly, or even to *label* correctly, these four Venus tablets that he had to include in LBAT in 1955.
- (2) His intemperate attack on Velikovsky at Brown University, in March of 1965.

* * *

Throughout his remarks at Brown, Sachs uses the refrain: "Not being a cuneiformist, Dr. Velikovsky . . ." Most of Sachs' remarks have to do with historiography. I will not try to discuss all of these in detail, but they are certainly easy to summarize, from his very own words. Thus he says that "Dr. Velikovsky's

²¹³⁴The various designations for the front and the back of a clay tablet, a papyrus, or a coin may be confusing. The front and the back, respectively, are usually called the obverse and the reverse in English, the recto and the verso in Latin, and the Vorderseite and the Rückseite in German. Confusion can sometimes arise because of the fact that the word for front begins with *r* in Latin and with *v* in German, while the word for back begins with *v* in Latin and with *r* in German! Abbreviation is not advised.

historical reconstructions . . . turn out to be bubbles of self-deception." He also says: "It takes very little blowing to collapse this house of cards." Not being a psychoanalyst, I cannot explain Sachs' fondness for metaphors of exhalation.

In order to see if there is all that much merit in Sachs' position, let us take his own handwritten remarks about the Venus tablets as a sample:

"In the so-called Venus Tablets of Ammisaduqa, which were copied and recopied over many centuries, Dr. Velikovsky sees in the few scribal errors evidence for the irregularity of Venus and carefully avoids the rest of the text that shows a high degree of regularity indeed. Not being a cuneiformist, Dr. Velikovsky quotes the 1920 opinion of Hommel to the effect that the year-formula of King Ammisaduqa found inside these texts was inserted by a scribe of the 7th century B.C. As every cuneiformist has had to learn for himself by sad experience, Hommel was senile by 1890, and his condition had certainly not improved perceptibly by 1920."

That last crack is the sort of thing that occasionally tempts me to forsake the high road in dealing with such people. Not being a cuneiformist, I know little inside dirt from the field of cuneiform studies, but I wonder why those who were active in that field continued to publish and to cite poor old senile Hommel. I notice that giants like Sayce, Virolleaud, and Langdon seem to be quite happy about having their own work appear together with that of Hommel, as in the very first volume of the *Babyloniaca* series (1906-1907). I also notice that Hommel's idea about the later insertion is cited by Fotheringham in *The Venus Tablets of Ammizaduga* (pages 39-40), which is itself perhaps one of the most widely cited books on that subject. (Fotheringham doesn't *agree* with Hommel at all, but the point is that he does *not* consider him unworthy of mention.) If Fotheringham can cite Hommel, why can't Velikovsky cite Hommel?

* * *

Not being a psychoanalyst, I do not know how Freudian slips should be interpreted. But here is one that I find interesting. What Sachs *first* wrote about Hommel was that "his condition had certainly improved perceptibly by 1920." Then, after he saw what he had written, he of course added a little caret below the line and a "not" above.

* * *

My only quibble with Hommel's basic suggestion is that the *terminus ad quem* for the insertion of the year-formula must have been in the late eighth century, rather than the seventh, since that insertion is already present in a tablet from the reign of Sargon and in other tablets from even earlier. But the important thing is that the insertion is *not* from the conventional time of Ammisaduqa, in the first half of the second millennium.

There *was* an insertion here, an insertion of *extraneous* material. The year-formula is simply jammed into the text, right where the last part of the entry for Year 8b must once have been and should still have been. So what is all that bad about Hommel's basic idea here? Not being either a cuneiformist or a

gerontologist, I cannot say whether "Hommel was senile by 1890." But even if he was, I would say more power to him. Hommel, by the way, was all of 36 years old in 1890! (Unfortunately, Sachs' slander about Hommel is the sort of thing that is easily parroted by others, especially those who are deficient in balance and judgment. See John D. North, "Venus, By Jupiter!", *Times Literary Supplement*, June 25, 1976, page 771, reprinted in *Stars, Man, and Fate* [1989], page 42.)

* * *

As for Sachs' other claims, there is no evidence whatsoever that the Venus tablets "were copied and recopied over many centuries." Velikovsky did indeed quote some of the more striking passages from the Venus tablets, namely, Years 9, 10, 11, 12, 13a, and 13b. But there is again no evidence whatsoever that all such passages are what Sachs would like to dismiss as a "few scribal errors."

What they really are is incompatible with the present astronomical situation. And the reported dates that are incompatible with the present astronomical situation are not just those "few." For it is in fact the entire *set* of dates that is incompatible with the present astronomical situation. Even if you leave out the lines that Velikovsky quoted, the rest of the reports *still* conflict with the present astronomical situation.

Years 8b, 15, and 20 report superior conjunction invisibilities of 3 months 9 days, of either 2 months 15 days or 3 months 15 days, and of 2 months 29 days, respectively. And Year 14 reports an inferior conjunction invisibility of 1 month 7 days. These reports are likewise difficult to reconcile with the present astronomical situation.

Actually, not only the lengths but also the *spacings* of the invisibilities, throughout the document, are quite incompatible with the present astronomical situation. Thus it is highly misleading to say that "the rest of the text . . . shows a high degree of regularity indeed."

Sachs would apparently like to leave the impression that if you take away the several lines that Velikovsky quoted, all the rest of the text is in perfect harmony with what we see today. If that is indeed his intention here, then Sachs is being totally dishonest.

Or *is* he? I wonder if Sachs ever took a good hard look at the Venus tablets. Perhaps he was too busy averting his eyes. Not being a Velikovskian, Sachs perhaps could not bear to look too closely at documents that were inconsistent with the present arrangement of the planetary system. He would have resisted and lashed out at anything or anybody that would force him to look at evidence of planetary gods inflicting global catastrophes on the human race.

* * *

Probably the two most important subjects that Sachs ever touched upon in his life were Velikovsky (in 1965) and the Venus tablets (in 1955 and again in 1965). Unlike Shapley, who is doomed to be remembered for only one thing, Sachs will probably be remembered for these *two* blind spots.

* * *

I have come to see Sachs' behavior not as a cause for revenge, but as something quite tragic. He saw so much, yet what he could not discern right before his face has now brought very serious injury to his reputation.

The Harlow Shapley story is a comedy. After all, does not Aristotle say that the subject of comedy is the ridiculous?

The Abraham Sachs story is indeed a tragedy. He was not a clown, like Shapley or Sagan, and he was not ridiculous. Rather, he was a victim of forces that he could not control.

* * *

I was the one who had originally pushed for "How Reliable Is the Scholarship of Abraham Sachs?" as the title of the paper that Vaughan and I were writing. Ironically, I am also the one who eventually suggested that that might be too harsh. Our original title does seem inflammatory, and perhaps even nasty. Of course, that was the idea: it was *intended* to strike readers *exactly* that way. This was pay-back time, long overdue.

I have always wanted revenge on the various villains of the Velikovsky Affair, whether from 1950 or from 1965 or from 1974. Thus I would no more pull punches with Sachs than I would with Shapley. They deserved whatever we could manage to give them. Even if they were dead.

Now, alas, I seem to be coming down with a late case of the mellows.

If I had known *only* of those two instances, perhaps my hostility to Sachs would have remained high. But over the last few years I have had occasion to study a number of Sachs' writings. They are quite lucid, and they are of the highest quality. I find him very helpful indeed.

Sachs, unlike Shapley and many of the others of that ilk, was serious, talented, learned, industrious, careful, objective, scrupulous, fair, and accomplished.

Here indeed was a scholar to emulate.

We should not vilify him for the two major lapses that occurred during his life. Indeed, we should perhaps look at those episodes as highly informative exceptions.

After all, his scholarship was quite solid in other areas.

Why did he slip up when he had to discuss the Venus tablets in 1955, and *why* did he again slip up when he had to discuss Velikovsky (and the Venus tablets once more) in 1965?

Could it be that he knew deep down that the Venus tablets were uniquely important? Could it be that he knew deep down that Velikovsky was right?

And could it be that his scotoma and his lashing out were instances of Freudian resistance—just like nearly everything else in the continuing Velikovsky Affair?

* * *

As we have just seen, Sachs seems to be one of those believe that the Venus tablets can be used to show that Venus was already on its present orbit early in the second millennium, and that Velikovsky is therefore wrong. That is why Sachs speaks of "a high degree of regularity indeed." Others who have used this sort of argument against Velikovsky are Payne-Gaposchkin, Kaempffert, Edmondson, de Camp, and Huber. (See, for example, Greenberg and Rose, "L. Sprague de Camp: Anatomy of a Zetetic," *KRONOS* III:1 (Fall, 1977), page 53, as well as *The Velikovsky Affair, passim*.) The arguments of Huber in particular are discussed in greater detail in the AAAS section of this book. For now, let me just mention that Huber not only argues from Venus, but also from the Moon: if the First Babylonian Dynasty is in the second

millennium, and shows months of 29 and 30 days, just as today, and also shows an intercalation rate consistent with our present year, then Earth and the Moon must have been on their present orbits back then, and Velikovsky's scenario cannot have happened. We shall see that Huber is entirely mistaken about all this, but the point for now is that the difficulties that Huber and the others have raised are precisely the sorts of difficulties that I was enumerating in "Do Ancient Calendars Contradict Velikovsky?" And it is precisely because of those continuing charges from Velikovsky's critics that this present sort of inquiry is necessary.

* * * * *

PART THREE: MOVING THE MIDDLE KINGDOM

EL-LAHUN

Late in the nineteenth century, many interesting and valuable papyri were found at El-Lahun in Egypt, in the precincts of the funerary temple of Sesostris II of the Twelfth Dynasty. A number of these papyri seemed to belong to Sesostris III and to Amenemhet III, but it has also been assumed that some of them might belong to the reign of Sesostris II himself.

One interesting El-Lahun document is from Year 7 of the reign of Sesostris III. It indicates that there was a heliacal rising of Sirius on IIII *prt* (Pharmuthi) 16 of that year. By simple retrojection of the Egyptian and Julian calendars, it can be determined that IIII *prt* 16 would have occurred on July 17 Julian, from -1872 to -1869.

But did the heliacal rising of Sirius actually occur on July 17 Julian at that epoch? Unfortunately, the retrocalculations of the heliacal risings of Sirius are not all that precise. Thus there is an area of uncertainty here, quite aside from any catastrophist considerations.

Among the El-Lahun documents, there are a number of lunar dates. For the fact is that the Egyptians used a lunar calendar right alongside their civil or 365-day calendar. The lunar months of the Egyptians began not with the first visibility of the new crescent (which the Jews, the Greeks, and the various peoples of Persia and Mesopotamia followed), but rather with the first *invisibility* of the *old* crescent. The morning on which the old crescent could no longer be seen was taken as the start of the lunar month.

If the old crescent was still visible on the morning when day 29 ended, then the new day beginning at that point would be counted as day 30 of the old month. If the old crescent had ceased to be visible, then the new day was counted as day 1 of a new month.

In the event of bad weather on the morning when day 29 ended, the practice seems to have been that they would count the new day as day 30. This might put their own reckoning one day later than our astronomical retrocalculations. Given the fact that there *would* occasionally have been bad weather (or bad "seeing," as the astronomers say), it is only to be *expected* that they would be running late from time to time. Indeed, if this did *not* happen, there might be cause for surprise.

(The Egyptian lunar calendar was in fact what is called a *luni-solar* calendar, in that an intercalary lunar month would be added every two or three years to keep in step with the natural year.)

THE NATURE OF THE DATA

Sometimes, one of the El-Lahun lunar reports will contain just about everything that we might need: the year of the reign, the full Egyptian date, *and* the exact lunar circumstances. Most reports, however, are incomplete. They may give the month and the day but not the year, and so on. But even these more fragmentary reports could become useful, if they turn out to be part of an overall pattern.

Almost always, the name of the reigning king is omitted. For example, there is one document from Year 3 of an unnamed king that tells us that *psdntyw* was on III *smw* 16 Egyptian. (This is Berlin Museum Papyrus 10090 recto, which Parker simply calls A.) Since *psdntyw*, or the day of the first invisibility of the old crescent, was taken as the first day of the lunar month, we may be able to retrocalculate the Moon's behavior at that epoch, and identify the exact Julian date on which such circumstances obtained. With a number of such reports to draw upon, the prospects are even better.

Sometimes the lunar tie-in is subject to alternative interpretations. We may be told the exact Egyptian calendar date on which some lunar feast occurred. But on what day of the lunar month was that feast celebrated? We know that *psdntyw* was lunar day 1, and it is usually *assumed* that the New Crescent Feast was on lunar day 2, that the feast of Draping was on lunar day 4, that the Full Moon Feast was on lunar day 16, and so on. But up to now it has not been established when the Wag-Feast was held, despite the claims of various scholars to have done so. Indeed, we do not even know how the word *wæg* is to be translated. Many *other* lunar feasts are likewise uncertain, in that the lunar day on which they were held has not yet been determined.

THE EDGERTON CHALLENGE

Despite the difficulties, a number of efforts have been made to use these lunar documents to fix the time of the Twelfth Dynasty. The challenge here has been described by Edgerton as follows (*Journal of Near Eastern Studies* I (1942), page 310):

"Several Twelfth Dynasty papyri apparently refer to lunar months in connection with regnal years, months, and days. Future research may perhaps prove that some one of these texts belongs to a particular reign; and if the reign proves to be that of Sesostris III such evidence in combination with the Sothic date may enable us to equate the sixteenth day of the eighth month in his seventh year with a particular day in a particular Julian year B.C."

My only reservation is that it does not even have to be Sesostris III. A solid tie-in by way of Amenemhet III would work just as well. In any case, if we can date the Twelfth Dynasty, we will of course have a better handle on the Eleventh Dynasty as well. (Together, the Eleventh and Twelfth Dynasties constitute the so-called Middle Kingdom.)

DOCUMENT D

Among the lunar documents, there is one that stands out. This is Berlin Museum Papyrus 10056 A verso, which Parker calls D. This document is concerned with months of priestly service in the temple of Sesostris II. One phyle of priests would be in charge of the temple for a period of one lunar month, then another phyle would be in charge for another month, then another phyle for another month, and so on. In fact, this document specifies only the *alternate* periods of monthly service, that is, it gives *every other* such period of monthly service. But the result is that we do seem to have twelve consecutive lunar dates. It has usually been taken for granted, with no discussion whatsoever, that at least the *initial* dates of these periods of monthly service are *psdntyw*. Let me give the Parker translation of document D (*The Calendars of Ancient Egypt*, page 64):

"II *smw* 26 down to III *smw* 25

III *smw* 25 down to regnal year 31, I *šht* 19

Regnal year 31, II *šht* 20 down to III *šht* 19

III *ḥt* [19 or 18] down to I *prt* 18

II *prt* 18 down to III *prt* 17

III *prt* 17 down to I *smw* 16."

Parker omits the three introductory lines of the text, as well as the names of the phyle-leaders that occur at the beginnings of each of these lines, but that is just as well for our purposes.

The expression "down to" is as ambiguous in Egyptian as it is in English. Does it mean "down to and including" or "down to and not including"? Parker calls the former Possibility I and the latter Possibility II. Until the correct reading is settled here, the dates over to the right in D are not of much value. Parker suggests that they should not even be used until the chronology is settled (*CAE*, page 65):

"It is clear that for chronological purposes it is safe to utilize only the initial dates of each pair as being certainly *psdntyw* [lunar day 1]. After definite results have been established, it might then be possible to make an intelligent choice between the other alternate dates."

THE FOUR PRINCIPAL "SOLUTIONS"

One of the earliest precise chronologies utilizing this lunar material was offered by Borchardt, who favored -1874 for Year 7 of Sesostri III. He had relied mainly upon four of the lunar documents: Berlin Museum Papyrus 10090 recto; Berlin Museum Papyrus 10062 A recto; Berlin Museum Papyrus 10006 recto; and Berlin Museum Papyrus 10056 A verso; these are the four documents that Parker labeled A, B, C, and D, respectively. (Later, as we shall see, Krauss labeled four other documents E, F, G, and H.)

The four principal "solutions" are those of Borchardt (*Die Mittel zur zeitlichen Festlegung von Punkten der ägyptischen Geschichte und ihre Anwerndung*, 1935), Parker (*Calendars of Ancient Egypt*, 1950), Krauss (*Sothis- und Monddaten*, 1985), and Luft (*Die chronologische Fixierung des ägyptischen Mittleren Reiches nach dem Tempelarchiv von Illahun*, 1992). They can be distinguished on the basis of where they put Year 7 of Sesostri III: Borchardt favors -1874, Parker favors -1871, Krauss favors -1829, and Luft favors -1866. Or, they could be distinguished on the basis of where they put D: Borchardt favors -1851 and -1850, Parker favors -1812 and -1811, Krauss favors -1787 and -1786, and Luft favors -1823 and -1822.

These four "solutions" have been discussed at considerable length in my article, "The Astronomical Evidence for Dating the End of the Middle Kingdom of Ancient Egypt to the Early Second Millennium: A Reassessment," *Journal of Near Eastern Studies* LIII:4 (October, 1994), pages 237-261. See also my forthcoming review of Luft in that same journal.

EQUATING THE YEARS

It is customary to equate Egyptian years with Julian years, even though this may not be strictly accurate. Thus when Parker claims that Year 7 of Sesostri III = -1871, all he means is that there was a substantial overlap between those two years, not an exact identity. Actually, if the two calendars are retrojected, we find that Thoth 1 Egyptian fell on December 3 Julian, -1872. But *most* of that Egyptian year did lie in -1871. As long as everyone understands this, there is no need to spell it out every time.

THE DATA AND THE FITS

The number of usable lunar dates has been increasing over the course of the twentieth century: Borchardt and Parker had 14, Krauss had 19, and Luft had 36.

These numbers have had to be adjusted slightly; they are not the exact numbers that were or would have been used by the authors themselves.

One problem is that a damaged date in D was known to be either 18 or 19, but could not be read. Eventually it was ascertained that there is just enough of the lower right of the number to show that it is 19 rather than 18. Thus IIII *ait* 19 is the date that must be matched by any proposed "solution."

Another problem is that we have not had published photographs of any of these documents until Luft's book. For example, we learn from Luft's photographs that B has no day-number: there is a month-number and a season, but then we come to the edge of the papyrus! Borchardt apparently *guessed* that the date was I *smw* 16 (just because there was a I *smw* 15 in an earlier column), but all that we can be sure of is the I *smw*. Any day-number would have been well over the edge!

Two other papyri that have no dated lunar events are 10056 A recto and 10052. I suggest that both of these, and B, should be dropped from the data set; the fact is that they *have* no data that we can make use of here. Their "lunar tie-ins" have simply been *guessed*, and are not to be found in the texts.

The scores for the four principal "solutions" are as follows: Borchardt hits 7 of 14; Parker hits 8 of 14; Krauss hits 8 of 19; and Luft hits 21 of 36. The average of these four "batting averages" is only 0.519. Good in baseball, no better than random in historical astronomy. We need to find a better way.

THE GREAT LEAP FORWARD

I then took a step that has been described as "shocking" and as "extremely bold": I moved a full Sothic period later, to see what could be found there. (Bold it may be, but there was nothing unreasonable about it. I already knew that the nineteenth century would not work, so it was only logical to search for an era that *would* work.)

Simple retrojection of the Julian calendar shows that IIII *prrt* 16 Egyptian = July 19 Julian, during the Egyptian-Julian quadrennium that lasted from -420 to -417. From the benchmarks provided by Censorinus and by the Canopus Decree, and from the calculations of Ingham ("The Length of the Sothic Cycle," *Journal of Egyptian Archaeology* LV (1969), pages 39-40), it can be shown that at that epoch the tetrad for Memphis was 18-18-18-19. Thus the Egyptian-*Sothic* quadrennium would have run from -417 to -414.

FIRST FITS

My attitude at that point was that the principal clues were the Sothic date and document D. The few other documents were mere "outliers," that is, single, isolated dates located somewhere in the general neighborhood of these two important documents, but not carrying very much weight in themselves. It might have been nice to find homes for them, but the major task was to locate the Sothic date and document D.

At first, I worked with the -417. I also followed the Turin King-List, which gives Sesostris III a reign of 30 + x years: I made it 36, the figure that Parker had favored. This put Year 36 of Sesostris III in -388 and Year 1 of Amenemhet III in -387. And that put Year 30 and Year 31 of Amenemhet III in -358 and -357,

respectively. The D dates came out two or three days too early there. From my familiarity with the 25-year cycle table, however, I knew that they would do better three years later. (See *CAE*, page 15.) So I gave Sesostris III 39 years, which put his Year 39 in -385 and Amenemhet III's Year 1 in -384. That put Year 30 and Year 31 of Amenemhet III in -355 and -354, respectively. D seemed to be very much at home there. Of the twelve dates, there were nine hits and three misses, but each of the misses was one day late, and could have been caused by bad weather.

Only because A and C seemed to come a little closer to fitting with -414 than with -417, I then chose to work with -414. I also went back to giving Sesostris III a reign of 36 years, as had Parker. This left Year 36 of Sesostris III in -385 and left Year 1 of Amenemhet III in -384. It also left Year 30 and Year 31 of Amenemhet III in -355 and -354, respectively.

I then decided not only to give Sesostris III the maximum 39 years that the Turin King-List allowed, but also to move Year 7 back to -417, where I had had it in the first place. This seemed desirable, in that -417 was the first year of the Egyptian-Sothic quadrennium. It also seemed appropriate, in that I had not been able to get A and C to fit with -414 anyway, and my only reason for favoring -414 over -417 had been to give A and C a somewhat better chance to fit.

There matters remained, for nearly a year. I was now quite satisfied that I knew the correct chronology of the Twelfth Dynasty. My fit was substantially better in the fifth and fourth centuries than anything that had been done for the nineteenth century – especially with regard to D and the Sothic date. A and C still missed, but they were only outliers. (B, with Borchardt's reconstructed "16," had actually hit for me in -356, but I still did not know that it was only a reconstruction and not a datum.)

MORE LETTERS

In December of 1992, I was offered the opportunity to revise the paper that I had submitted to the *Journal of Near Eastern Studies* and to take account of Krauss' 1985 book, which I had not known about. When I obtained the book itself, in January of 1993, I found that Krauss had assigned the letters E, F, G, and H to four other dates. But G and H were the two Wag-Feasts that Parker had discussed, and I already knew about them. They were not very weighty, because the exact day of lunar month on which the Wag-Feast might have been held was unknown. E and F were interesting, but they were already known to me from Borchardt. Besides, they were only outliers, and I still felt that the principal weight had to be attached to the Sothic date and to D, with its twelve *consecutive* dates.

THE TAIL WAGS THE DOG(-STAR)

About four months later, in May of 1993, I learned of Luft's 1992 book, when the editor of the *Journal of Near Eastern Studies* asked me to take account of it, too, in my forthcoming article; he also invited me to write a review of Luft's book.

Now it was a new ball-game. Luft had almost *doubled* the number of dates, and had *tripled* the number of documents.

All of the new dates were what I had been calling outliers. But now there were so many of them that their collective weight was considerable.

I could still not imagine that D would ever have to be shifted in the light of these new outliers, but the overall weight of D did have to be considered less, at least in percentage terms, now that there were so many other dates to be reckoned with.

I had also been pushed in another new direction, or, rather, in two new directions.

Some of my earlier efforts had followed Parker in giving Sesostris III 36 years. That precise figure was not used by everyone, but at least there seemed to be a consensus that the 30 + x of the Turin King-List was probably right. In the end I had settled on 39.

I had also accepted the usual view that Sothic dates were obtained from a *Memphis* observation-post.

But in recent years both of these assumptions have been challenged.

There is today a growing tendency to think that the reign of Sesostris III was of only 19 years, and that the reign of Sesostris II, who *had* been given that 19 years, should be reduced to as little as *six* years.

There is also today a growing tendency to think that the official observation-post for the heliacal risings of Sirius may have been at *Elephantine*, rather than at Memphis. This would make for a difference of about 5¾ days in the date of the heliacal rising of Sirius.

Thus even the Sothic date itself would now need to be situated on the basis of the outliers. The tail was wagging the Dog!

First, the general location of the Sothic date tells us the general era of the outliers. Then, the outliers tell us the exact year of the Sothic date, and thereby settle the question of the observation-post. This is not a vicious circle. It is indeed a circular procedure, but it is an entirely virtuous one!

* * *

Since D was immovable, and since Amenemhet III was *ipso facto* immovable, I first simply cut Sesostris III to 19 years. That put his Year 7 in -397, which was at least in the Elephantine ball-park.

Now I needed to test the Sesostris III outliers. There were fourteen of these. As a group, they were running two or three days too early. From the 25-year cycle table, I knew that these Egyptian dates would work much better if I shifted them to three years later. But I also knew (or thought I knew) that that was ultimately impossible, because D and Amenemhet III were immovable. Even if I shifted Sesostris III, and improved the fit for him, I would ruin what I had already achieved for the reign of Amenemhet III.

MISSION IMPOSSIBLE

I decided to go for it. Nearly half of the dates hit, but the rest came a day or two late. Fortunately, my work-sheets had contained the dates of the new crescents, but only as a sort of rough check on my other determinations. (It is interesting that if I had used a computer, those new crescent dates would have been deemed unnecessary, and would not have been there!)

I noticed immediately that there was a pattern: the dates that had hit were where the new crescents fell on lunar day 2, the dates that were one day late were where the new crescents fell on lunar day 3, and the dates that were two days late were where the new crescents fell on lunar day 4.

I could smell total success, at least with Sesostris III. All I had to do was assume that the New Crescent Feast (and the Feast of Sand-Moving) were observed on the actual day of the new crescent, whenever that was, and that the other lunar feasts, like Draping, Joy, Land-Excursion, Wag, and Line of the Nile Mile, were observed a fixed number of days *after* the day of the new crescent, whenever that was. (The Full Moon Feast seems to have been governed directly by observation; that is, it seems to have been held on the actual day of Full Moon.)

HOW IT WORKS

Berlin Museum Papyrus 10092, for example, tells us that in Year 5, on II *ꜥht* 27, there was a Feast of Draping. Retrocalculation shows that while the first invisibility of the old crescent occurred on II *ꜥht* 23 = January 22 Julian, -396, which would be *psdntyw* or lunar day 1, the first visibility of the new crescent did not occur until II *ꜥht* 25 = January 24 Julian, -396, which would have been lunar day 3. If the New Crescent Feast was held on the actual day of new crescent, and if the Feast of Draping was held on New Crescent Feast + 2, then the Feast of Draping would have been on II *ꜥht* 27 = January 26 Julian, -396, and 10092 would be correct.

More often than not, the new crescent does occur on lunar day 2. If that had been the case here, then the Feast of Draping would indeed have been on lunar day 4. But the usual view that Draping was *always* on lunar day 4, that Joy was *always* on lunar day 5, that Land-Excursion was *always* on lunar day 10, and so on, has merely been taken for granted, and has never been argued or otherwise substantiated. That usual view needs to be abandoned. The truth is that these feasts were geared not to any fixed day of the lunar month, but rather to their distance from the day of the New Crescent Feast (which was also the actual day of the new crescent):

New Crescent (and also Sand-Moving)	+ 0
Draping	+ 2
Joy	+ 3
Land-Excursion	+ 8
Wag	+16
Line of the Nile Mile	+19

The positions of various other lunar feasts are not known, but every occurrence of each of these listed feasts fits exactly. I suspect that the other feasts will work the same way, if we ever find comparable data about them.

THE DATES FROM SESOSTRIS III

For purposes of retrocalculation, it is convenient to give the "reduced" dates (Parker's term), that is, the implied dates of *psdntyw*, or lunar day 1, rather than the dates actually reported. Thus for 10092 I give the "reduced" date of II *ꜥht* 23 for *psdntyw*, rather than the reported date of II *ꜥht* 27 for the Feast of Draping. The Feast of Draping always occurred two days after the day of the new crescent, which was on II *ꜥht* 25 or lunar day 3 in this case. Where the lunar feast is geared to the new crescent, one could of course give the retrocalculated date of new crescent. But since several of the other dates are *psdntyw* anyway, I shall follow the usual practice of giving everything in terms of *psdntyw*.

In the following tables, the numbers to the far right are the lunar days on which the new crescents occurred. If they are in parentheses, I actually used them in carrying out my "reductions." If they are in brackets, the new crescents are irrelevant; thus I did not have to use them, and they are there only for information.

SESOSTRIS III

	Recorded Egyptian Dates ("Reduced" Where Necessary)		Equivalent Julian Dates		Calculated Julian Dates (From Schoch)	
10092 Year 5	II <i>ꜥht</i> 23	=	January 22	=	January 22, -396	(3)
10009 Year 5	II <i>pꜣrt</i> 22	=	May 20	=	May 20, -396	(2)
10282 Year 6	I <i>ꜥht</i> 13	=	December 12	=	December 12, -396	(3)
Year 6	II <i>ꜥht</i> 12	=	January 10	=	January 10, -395	(3)
Year 6	III <i>ꜥht</i> 12	=	February 9	=	February 9, -395	(3)
10130 Year 8	II <i>ꜥht</i> 21	=	January 19	=	January 19, -393	(2)
Year 8	III <i>ꜥht</i> 21	=	February 18	=	February 18, -393	(2)
10003 (E) Year 9	III <i>pꜣrt</i> 8	=	June 4	=	June 4, -392	(2)
10112 Year 10	III <i>ꜥht</i> 29	=	March 27	=	March 27, -391	(3)
10412 Year 11	I <i>ꜥht</i> 19	=	December 17	=	December 17, -391	(3)
10165 Year 12	II <i>smw</i> 3	=	August 28	=	August 28, -389	(4)
10248 (F) Year 14	II <i>ꜥht</i> 16	=	January 12	=	January 12, -387	(3)
10011 Year 16	II <i>pꜣrt</i> 23	=	May 19	=	May 19, -385	(2)
10016 (G) Year 18	I <i>smw</i> 29	=	August 22	=	August 22, -383	(3)

PROBLEMS WITH PERFECTION

This Sesostris III fit was actually quite troubling.

On the one hand, it was *perfect*: 14 dates, 14 hits. No need to appeal to "scribal errors." Not even a *trace* of "bad seeing." Yet there was also no way that these dates could have been forged; even a Kepler could not compute first invisibilities. All of this had to be *observed*. And in the *fourth century*.

Hitting 14 out of 14 dates inspired great confidence, even approaching certainty. It *had* to be that my way of seeing the New Crescent Feast and the subsequent lunar feasts that were in step with it was correct. It *had to be* that these dates for Sesostris III were correct.

On the other hand, it was also quite hopeless. I knew that Amenemhet III and D lay just down the road, guaranteeing failure, and rendering the entire enterprise impossible. Moving closer to a Sesostris III fit was equivalent to moving away from a D fit. I saw no way out.

The lowering of Sesostris III by those three years had to be. It was an irresistible force. But Amenemhet III, locked in by D, was an immovable object. Because of the Sesostris fit, the lower dates seemed certain; because of D, they seemed impossible. How could there be unimaginable success in the one case, and unavoidable failure in the other?

How could Sesostris III hit 14 of 14, when D would soon show that this was all wrong anyway? It made no sense.

KINGS IN COLLISION

I went ahead anyway. Here I at least found some bad seeing situations: dates that were one day late, but could be explained by way of bad weather.

At this point, I was including everything that Luft had accumulated, all 39 dates.

I had not yet taken a sufficiently hard look at B, or at 10052, or at 10056 A recto. Neither had anybody else. B has a lunar tie-in and a year and a month, but no day! 10052 and 10056 A recto have precise Egyptian dates, but no lunar tie-ins. Scholars have simply *guessed* what this missing material might have amounted to.

All three of their *reconstructed* dates missed here, by coming one day late. These could have been explained by bad seeing, but they should more properly have been excluded from the data set, on the grounds that they in fact contain no usable lunar data.

That still left me with misses on A and on 10104. But bad seeing would explain both of these.

As expected, *all* of the dates in D missed.

At this point, however, another very instructive pattern came to light. D did miss on everything, all 12 dates, but most of the dates seemed to be coming out on what retrocalculation revealed to be lunar day 3.

(Only one date, II *šit* 20, was a clear exception to this: it came out on lunar day 4. This one miss could not be explained by bad seeing, since the previous such event was 31 days away. But it was, after all, just *one*.)

Could D have been *written* with day 3 in mind? If it was, I would have been able to claim 11 hits on D.

"DOWN TO" THE SOLUTION

At this point, what proved most instructive was document C. Krauss and Luft had shown that this document really involves two lunar dates.

C₁ is to the effect that a period of priestly service ran from II *šit* 9 down to III *šit* 7. Notice that if we are to avoid a 28-day month, "down to" must be read in accordance with Possibility I, that is, as "down to and including."

C₂ seems to indicate that III *šit* 6 is *psdntyw* and that III *šit* 7 is New Crescent Feast.

As the only way of reconciling C₁ and C₂, I had already decided that C₁'s period of monthly service must have run from lunar day 3 down to and including lunar day 2. I had also noted that III *šit* 6 and III *šit* 7 both had the same phyle-chief: Sə-bəstt's son, Sə-bəstt. This seemed to confirm that both lunar day 1 and lunar day 2 were in the same service-month. (According to D, this same man, Sə-bəstt's son, Sə-bəstt, had also been phyle-chief about twelve months earlier, from II *šit* 20 [or was it 19?] down to III *šit* 19.)

If C recognized lunar day 3 as the start of the new service-period, why not D?

I thereupon "reduced" each date in D on the assumption that D was referring consistently to lunar day 3's. (One advantage of changing the guard on lunar day 3 was that there would be two days warning. If they had tried to change the guard on *psdntyw*, they would often not have known just when *psdntyw* would occur until it was upon them. For example, the new phyle would have to be *standing by* at dawn as each lunar day 29 came to an end. More than half of the time, they would not even be needed, and they would just have to twiddle their thumbs for another 24 hours. This would be a *very* inefficient way to do business.)

I further assumed that in D the expression "down to" meant "down to and not including." In other words, C used Possibility I and D used Possibility II. This seems entirely appropriate, in that C mentions only one service-period, in which case it is perhaps more natural to refer to the first and last days of the month in question, while D gives a lengthy series of (alternate) service-periods, in which case it might have seemed more natural to refer to the first day of one such period and the first day of the next such period.

In this way, I found an unexpected solution to the problem of the irresistible Sesostris III meeting the immovable Amenemhet III: the latter was *not* immovable. Sesostris III had now knocked Amenemhet III to three years later, and for that matter into a *better* fit. Now all was well.

A has usually been put in Year 3 of Amenemhet III, where it misses by one day. This is readily explainable in terms of bad seeing. But what if it was under Sesostris II? In -404, A would hit!

Leaving out the three texts that have no useable lunar data, and reading both C and D in the "lunar day 3" mode, with C using Possibility I and with D using Possibility II, and putting A in the reign of Sesostris II, we now have this as our overall picture:

Recorded Dates,	Equivalent	Calculated
"Reduced" Where	Julian Dates	Julian Dates
Necessary, With		(From Schoch)
Possibility I		
For C, And With		
Possibility II For D		

SESOSTRIS II

10090 (A) Year 3 III *smw* 16 = October 13 = October 13, -404 [2]

SESOSTRIS III

10092 Year 5 II *ꜥht* 23 = January 22 = January 22, -396 (3)

10009 Year 5 II *pꜣrt* 22 = May 20 = May 20, -396 (2)

10282 Year 6 I *ꜥht* 13 = December 12 = December 12, -396 (3)

	Year 6	II <i>šht</i> 12	=	January 10	=	January 10, -395	(3)
	Year 6	III <i>šht</i> 12	=	February 9	=	February 9, -395	(3)
10130	Year 8	II <i>šht</i> 21	=	January 19	=	January 19, -393	(2)
	Year 8	III <i>šht</i> 21	=	February 18	=	February 18, -393	(2)
10003 (E)	Year 9	III <i>pṛt</i> 8	=	June 4	=	June 4, -392	(2)
10112	Year 10	III <i>šht</i> 29	=	March 27	=	March 27, -391	(2)
10412	Year 11	I <i>šht</i> 19	=	December 17	=	December 17, -391	(3)
10165	Year 12	II <i>smw</i> 3	=	August 28	=	August 28, -389	(4)
10248 (F)	Year 14	II <i>šht</i> 16	=	January 12	=	January 12, -38	(3)
10011	Year 16	II <i>pṛt</i> 23	=	May 19	=	May 19, -385	(2)
10016 (G)	Year 18	I <i>smw</i> 29	=	August 22	=	August 22, -383	(3)

AMENEMHET III

10166	Year 9	II <i>šht</i> 16	=	January 9	=	January 9, -373	(2)
58065 (H)	Year 9	II <i>smw</i> 10	=	August 31	=	August 31, -373	(4)
10018	Year 10	II <i>šht</i> 5	=	December 29	=	December 29, -373	(2)
10079	Year 10	III <i>šht</i> 5	=	January 28	=	January 28, -372	(2)
10344	Year 11	III <i>šht</i> 24	=	February 15	=	February 15, -371	(2)
10104	Year 24	III <i>pṛt</i> 2	=	May 21	≠	May 20, -358	(2)
10056 (D)	Year 30	II <i>smw</i> 24	=	September 8	=	September 8, -352	[3]
		III <i>smw</i> 23	=	October 7	=	October 7, -352	[3]
		III <i>smw</i> 23	=	November 6	=	November 6, -352	[3]
	Year 31	I <i>šht</i> 17	=	December 5	=	December 5/6 -352	[2/3]
		II <i>šht</i> 18	=	January 5	≠	January 4, -351	[2/3]
		III <i>šht</i> 17	=	February 3	=	February 3, -351	[2]
		III <i>šht</i> 17	=	March 5	=	March 5, -351	[2]
		I <i>pṛt</i> 16	=	April 3	=	April 3, -351	[3]
		II <i>pṛt</i> 16	=	May 3	=	May 3, -351	[2]
		III <i>pṛt</i> 16	=	June 1	=	June 1, -351	[3]

		III <i>prt</i> 15	=	July 1	=	July 1, -351	[3]
		I <i>smw</i> 14	=	July 30	=	July 30, -351	[3]
10006 (C)	Year 32	II <i>ꜥht</i> 7	=	December 25	=	December 25, -351	[2]
		III <i>ꜥht</i> 6	=	January 23	=	January 23, -350	(2)
10206	Year 36	II <i>ꜥht</i> 24	=	January 10	=	January 10, -346	(2)

THE TWO MISSES

Thus there are 34 hits out of 36. One of the two misses (10104) is readily explainable in terms of bad seeing. The other one, in line 3 of D, is more complicated, but it too is amenable to some kind of understanding: I suspect that the original from which D was copied said II *ꜥht* 19 and used Possibility II ("down to and not including") and that the scribe of D read that source with Possibility I ("down to and including") in mind; that gave him the appearance of a 31-day month, which he carefully "corrected" by emending II *ꜥht* 19 to II *ꜥht* 20, thus leaving us with D exactly as we have it!

Some of the scores—not only my 34 of 36, but also the scores that Borchardt, Parker, Krauss, and Luft obtain for the early second millennium—might have to be *slightly* altered in the event of improved retrocalculations at some time in the future. But I do not expect very much change from this direction. There are simply too many unknowns: the exact values of the *arcus visionis*, the altitude of the observation-post, the height of the horizon, the level of atmospheric pollution, the experience and visual acuity of the observer, and so on. Thus it seems quite unlikely that there will ever be any *radical* improvement in the quality of the retrocalculations.

ELEPHANTINE

The Elephantine adjustment is *really* neat. Remember that for every degree of latitude that we move to the south, the heliacal rising of Sirius will occur just about one day earlier. Elephantine is about $5\frac{3}{4}$ degrees south of Memphis. Thus a Memphis tetrad of 18-18-18-19 would correspond to an Elephantine tetrad of 12-12-13-13. Notice that $5\frac{3}{4} \times 4 = 23$, and that $-417 + 23 = -394$. Bingo.

WHY SEBEKNEFRU?

Gardiner wonders how Sebeknefru could have been seen as ending an age (*Egypt of the Pharaohs*, page 147):

"Since the passage of Time shows no break in continuity, naught but some momentous event or sequence of events can justify a particular reign being regarded as inaugurating an era. What caused Sebeknofru, or Sebeknofrur_’ as later sources call her, to be taken as closing Dyn. XII will doubtless never be known"

But now it is known: the timing makes it quite clear that the Twelfth Dynasty ended with the coming of Alexander the Great in -331! (For further details, see Chapter Twenty of *Sun, Moon, and Sothis*.)

THE CHRONOLOGY OF THE TWELFTH DYNASTY

I cannot here discuss all of the details of my various arguments about these matters, but the following table represents my chronology for this period. Some portions of the table will require more detailed explanation. (It-towe was the Twelfth Dynasty capitol; it means "Seizer of the Two Lands.")

Chronology of the Dynasts of the Residence It-towe

-500 - x + y	Amenemhet I, Year 1 (ascension on II <i>smw</i> 9 = September 30 Julian?)
-480 - x + y	Amenemhet I, Year 21 = Sesostris I, Year 1
-472 - x + y	Amenemhet I, Year 29 = Sesostris I, Year 9
-471 - x + y	Sesostris I, Year 10
-438 - x + y	Sesostris I, Year 43 = Amenemhet II, Year 1
-436 - x + y	Sesostris I, Year 45 = Amenemhet II, Year 3
-435 - x + y	Amenemhet II, Year 4
-406 - x + y	Amenemhet II, Year 33 = Sesostris II, Year 1
-404 - x + y + z	Amenemhet II, Year 35 + z = Sesostris II, Year 3 + z
-403 - x + y + z	Sesostris II, Year 4 + z
-401	Sesostris II, Year 6 + x - y
-400	[Sesostris II, Year 6 + x - y + 1 =] Sesostris III, Year 1
[-399 + y - 2	Sesostris II, Year 6 + x = Sesostris III, Year y]
-394	Sesostris III, Year 7, IIII <i>prt</i> 16 = July 13 Julian
-382	Sesostris III, Year 19
-381	{Sesostris III, Year 20} = Amenemhet III, Year 1
{-362 - x + y - z	Sesostris III, Year 39 - x + y - z = Amenemhet III, Year 20 - x + y - z}
{-361 - x + y - z	Amenemhet III, Year 21 - x + y - z}
-344	Amenemhet III, Year 38 = Sebeknefru's last complete year as co-regent?
-343	Amenemhet III, Year 39 = Amenemhet IV, Year 1 (replacing Sebeknefru?)
-335	Amenemhet III, Year 47 = Amenemhet IV, Year 9

- 334 Sebeknefru, Year 1
- 332 Sebeknefru, Year 3
- 331 Sebeknefru, Year 4 (until III *smw* 25 = October 4 Julian)

The actual length of the Twelfth Dynasty is 169 years. But the total of the reign-lengths, including co-regencies, is 213 years, as reported by the Turin King-List. This has been achieved by giving Amenemhet III a 20-year co-regency with his father, Sesostris III. (The conjectural co-regency of Sebeknefru would have been terminated years before she reached the throne, and it is *not* being counted here; when she did finally attain the throne, her year-count seems to have been started afresh, anyway.)

x is the number of years that Sesostris II completed after his Year 6.

y is the number of years that Sesostris III had completed as a co-regent and that were later counted as part of his own reign.

z is the number of years that Amenemhet II completed beyond Year 35.

I suspect that x, y, and z all might be 0, but we do not know. Until we do know, I must allow for them in the table. Any one of them that is discovered to be 0 should be deleted. If any one of them is discovered to have a definite value higher than 0, the table should be altered as indicated.

If $y = 0$, all of the bracketed material is to be deleted. If $y = 1$, then the bracketed line for -399 is to be deleted, but the bracketed material in the line for -400 must be retained. If $y = 2$ or more, all bracketed material must be retained.

If $x - y = 1, 2, 3,$ or 4, then the date in line one must be changed to October 1 Julian; if $x - y = 5, 6, 7,$ or 8, then the date in line one must be changed to October 2 Julian; and so on. (If $x - y \neq 0$, then A misses.)

In all of these situations so far, the total of the reign-lengths would remain at 213 years, but the actual duration of the Twelfth Dynasty might vary, up or down, from our initial figure of 169 years.

The game of musical chairs played by Sebeknefru and Amenemhet IV is not certain, nor is the co-regency of Sesostris III and Amenemhet III. If that latter co-regency turns out not to have occurred, then the material in braces should be deleted. In that case, however, the reign-lengths would no longer total 213 years.

What I do regard as very *solidly* established here are that Year 1 of Sesostris III was -400, that the Sothic date is from -394, and that Year 1 of the long reign of Amenemhet III was -381. That locks in more than enough for our purposes.

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PART FOUR: ASSESSING THE REPERCUSSIONS

THE VERY NEXT (AND SOMEWHAT LATER) THOUGHTS

On first finding the actual dates for the Twelfth Dynasty, my *immediate* thoughts were along the lines of: "Wow! That's it! That's the answer! That's the way it happened! That's just *when* it all happened! No student of ancient Egypt has ever *seen* this before! I am the very *first* to contemplate those beautiful results that now lie before me!"

Most of my "next" thoughts were not all that immediate, however, but were spread over a period of some weeks—especially with regard to psychoanalytic resistance, whose import I have always been somewhat slow to grasp anyway.

The following summary of my early attitudes is accurate, even though it is much overstated and lacks various reservations and qualifications that would surely be prudent.

People will readily accept this. It is cut and dried. The evidence is solid and unanswerable. Anyone who *looks* at the data will inevitably be convinced.

And people can accept something like this with clear heads, just by weighing the evidence. No repressed collective memories of catastrophes stand in the way.

Psychoanalytic resistance should not even come into play here. Unless they see some of the remote catastrophist consequences, and recoil! But probably they won't even notice.

Perhaps this can be sold to the historians without their even *thinking* of Velikovsky. After all, this redating is *not* anything that is automatically associated with Velikovsky.

My other work was always somewhat problematic, in that it might well have been wrong, or at least was not conclusive. This redating of the Middle Kingdom *is* conclusive, and could hardly be wrong—except perhaps in the general sense that anything could in principle be wrong.

All Velikovskians worthy of that name will accept it as soon as they examine the evidence.

I could have sold it to Velikovsky in five minutes, and I can sell it to any of his supporters in fifteen.

But I know full well that I will not be the one to finish all that has to be done here. I have spent too much time on astronomy, and not enough time on history and archaeology and language. I just don't know the terrain.

The Velikovsky movement contains many who will be able to *build* upon this redating of the Middle Kingdom. They will construct edifices that I cannot even imagine. They will prove theorems tomorrow that I would never have thought of in a million years, let alone accepted. They will sift through the

masses of historical data that I have always found unsiftable, and they will put everything in its proper place.

They will destroy fundamentals and axioms that I still see as indubitable. This redating of the Middle Kingdom is a powerful weapon, with which they will be able to demolish the conventional chronology – and more. But that demolition will seem almost incidental, in comparison to what they will then be able to *build*.

All of this is quite a bit overstated, yes, but those were indeed some of my very early thoughts!

THE RAMESSIDE TAX-LIST

Let me give one such deduction that I *do* understand. The links between the Twelfth Dynasty and the Thirteenth Dynasty are quite firm, even though we know very little about the Thirteenth Dynasty. If the Twelfth Dynasty ended with the coming of Alexander the Great, then the Thirteenth Dynasty must belong in the Macedonian period – even if some of them were holdovers from the Twelfth Dynasty, which is entirely possible.

Probably a good many of those numerous "kings" were little more than nomarchs or district administrators under the Macedonians. And probably many of them were contemporary with one another. (Gardiner, with respect to an earlier but quite similar situation, speaks of "provincial princes." See *Egypt of the Pharaohs*, page 114.)

A King of Kings like Alexander the Great liked to have any number of other "kings" subservient to himself. Appointing a King Abdalonymos of Sidon or reappointing a King Poros of India could only *enhance* his own status as the King of Kings. In this he was but following the Persian custom.

Unfortunately, many of these nomarchs and vassal kings have made their way into history as full-fledged kings. They seem to have come along thick and fast, with much in the way of overlap and rough contemporaneity, and with relatively short reigns (not at all suggestive of lifetime tenure). Often nothing is known but the name of the "king" and/or his "reign"-length. Yet several of these individual "reigns" are longer than Alexander the Great's eight remaining years of life after he left Egypt. For that reason, I feel that I must put the end of the Thirteenth Dynasty in Ptolemaic times, that is, *after* the death of Alexander.

* * *

I have some distaste for the expression "Turin Canon." This is indeed the customary way of referring to the King-List that is written on the reverse side of the Turin Papyrus. But that King-List should probably *not* be considered all that "canonical"! That is why I prefer to call it the Turin King-List.

The Turin-King List gives us the names of a great many of those Thirteenth Dynasty kings. And this King-List itself seems to be from *after* the Thirteenth Dynasty. Most likely, then, the Turin King-List is *Ptolemaic*, since the Thirteenth Dynasty, whatever it may have been, seems likely to have extended at least a little beyond the death of Alexander the Great.

The Turin King-List may even be later than Manethon, rather than being one of his thousand-year-old sources, as is usually thought. But the Turin King-List has no dynastic numbers, just the Manethonian *order* for most of Dynasties One through Seventeen. (Dynasties Seven through Ten and Dynasties Eighteen

through Thirty-One are apparently omitted, and Dynasties Thirteen through Seventeen are blurred into such an implausible stew of unfamiliar names and numbers that no one takes that part of the King-List very seriously anyway.)

The obverse or recto of a papyrus was routinely inscribed first; only *then* was the verso or reverse side used. The obverse of the Turin Papyrus contains tax material that is best described as Ramesside. Some people do try to assign it to the reign of Ramses II of the Nineteenth Dynasty, but that is merely conjecture, and we should probably just leave it at Ramesside, a term that would encompass the Ramesside kings of the Twentieth Dynasty as well.

Lines $x + 5$ and $x + 6$ of Column VIII of the obverse or recto of the Turin Papyrus do contain the same cartouche, which is that of a "Beloved of Ammon, Ramses." But this could have been any of the Ramessides—though probably not Ramses I, who seems not to have used that title.

In any case, this papyrus may have been stored away for a considerable time, before someone appropriated it as scrap paper (or "scrap papyrus") and used the reverse for our King-List.

High quality papyrus was relatively expensive, and this had after all been a royal document. It was 41 centimeters high and at least 170 centimeters long. A large blank writing surface of such excellence would not have been lightly discarded. Eventually, the tax records on the obverse might have been obsolete (perhaps everybody had died), and the papyrus could then have been appropriated for other use, even personal use.

The King-List on the reverse side of the Turin Papyrus is usually thought to have *itself* been Ramesside, on the grounds that the reverse would have been written some time after the obverse, namely, long enough afterwards for the obverse to have lost its importance, but not so long that there would have been an implausible amount of time during which the papyrus was being preserved but never used. To most people who have discussed this sort of thing, a period of something under a century seems indicated, as a reasonable interval between the writing of the tax material on the obverse and the writing of the King-List on the reverse.

Where does this leave us? If the reverse was written in Macedonian times, under, say, Ptolemy I Soter (or just possibly Ptolemy II Philadelphos), and if the obverse was written somewhat under a century earlier, then the most likely epoch for the Ramesside tax-records on the obverse would have to be in the fourth century. This clearly supports Velikovsky's claim that the Twentieth Dynasty (= the Twenty-Ninth and Thirtieth Dynasties) belongs in the fourth century. Actually, this "Sebennytic" Dynasty, as Velikovsky calls it, may have begun just before the turn of the century, but the great *bulk* of it would still have been in the fourth century. Thus Velikovsky puts Nektanebo I/Ramses III in the first half of the fourth century, followed by Tachos/Ramses IV, and with Nektanebo II/Ramses VI straddling the middle of the fourth century.

I do not claim that this argument is the sturdiest that I have ever seen. But it does seem to me to be a sound argument. It is clearly valid, and it rests upon premisses that are quite probable. There is a very solid *chain* here, and I do not really see any link that I can break.

It would seem, then, that the radical lowering of the Twelfth Dynasty *requires* something very much like Velikovsky's own late placement of Ramesside materials, at least those of the later Ramessides. This is a rather surprising result. I anticipate that there will be many another such result from the radical lowering of the Twelfth Dynasty. That lowering *must* have *vast* implications, whether or not we can as yet see them all.

If Velikovsky is correct in saying that the Nineteenth and Twenty-Sixth Dynasties are the same, and are from the seventh and sixth centuries, then the reign of Ramses II should probably be eliminated as a possible provenance of the Turin Papyrus, since it is not very likely that the reverse would have gone unused for as long as three hundred years! Typically, such papyri would be recycled much sooner than that.

(Notice that the *conventional* placement of the Ramessides in the second millennium becomes completely unworkable, once we know that the Twelfth Dynasty was ended by Alexander the Great, and once we know that the Turin King-List is from somewhat later than that.)

JOHN KEATS

Despite the fact that I have ancestors named Cortez, I do not much admire Hernando Cortez. Nor do I think that his reputation can be restored by crediting him with the accomplishments of Balboa. In any case, I am perhaps too much the pedantic professor and too little the poet to think very highly of anyone like John Keats, who could confuse Balboa with Cortez, meter notwithstanding. I refer of course to Keats' sonnet, "On First Looking into Chapman's Homer," in which Keats speaks about the mystery and the thrill of discovery:

Then felt I like some watcher of the skies
 When a new comet swims into his ken;
Or like stout Cortez when with eagle eyes
 He star'd at the Pacific – and all his men
Look'd at each other with a wild surmise –
 Silent, upon a peak in Darien.

I never really knew what Keats was talking about. Now, after finding the true anchorage for the Middle Kingdom, I am beginning to see this sonnet in a somewhat different light.

Keats' effort to capture the *feel* of discovery, whether it is his own discovery of Chapman's Homer, or the discovery of the Pacific by "Cortez" (scanably "stout," but still the imposter!), is something to which I can now relate much better than I ever could before.

BEHAVIOR MODIFICATION

I used to have a drink or three before dinner, almost every day, whether alone or in company. When I was in the midst of doing the calculations that moved the Middle Kingdom, I stopped drinking. Nor have I resumed, except at parties and social occasions, or at various other gatherings that require no deep thought (such as scholarly conferences).

The reality of moving the Middle Kingdom gave new meaning to the phrase "success experience." It provided a high unmatched by anything else.

I also gave up sleeping for a time, except for short stretches. I would work all night and into mid-morning, and it was only with great reluctance that I allowed myself to sleep for a few hours before resuming. That was in about the third week of July, 1993. Since then, I have typically imbibed only on those largely social occasions that I mentioned, and rarely more than one drink even then. This pronounced change in my behavior is directly related to my belated discovery of what a reality-based high can be like. I may not understand what that change means, but I know what caused it.

A NON-KEATSIAN ACCOUNT

Take Dom Perignon champagne, caviar, filet mignon, sex, applause, drugs, Lamborghinis, Tiffany lamps, million-dollar jackpots, holes-in-one, and whatever else you may wish to add to that list. Then combine them, deleting any downside. All together, they fall short of the way it feels to move the Middle Kingdom!

AMMISADUQA AND HAMMURABI

In a series of publications from the 1980's and 1990's, Gunnar Heinsohn has argued, mostly on the basis of stratigraphy, that many of the ancient kings, dynasties, and peoples are but duplications or even triplications of much *later* kings, dynasties, and peoples. This makes for a radical lowering and shortening of ancient history, perhaps even more radical than Velikovsky's. For example, Heinsohn has suggested that the First Babylonian Dynasty and the Persian Empire are identical. As part of this thesis, he maintains that Hammurabi of the First Babylonian Dynasty is identical with Darius the Great of the Persian Empire, and that Ammisaduqa of the First Babylonian Dynasty is identical with Artaxerxes III Ochus of the Persian Empire.

I did not find any of Heinsohn's own arguments convincing, but neither did I have any objections to these suggestions of his. Ammisaduqa is conventionally connected to the Venus tablets (which are even *called* the Venus tablets of Ammisaduqa), but that connection has never been established, and Vaughan and I have always maintained that the Venus tablets have nothing to do with Ammisaduqa!

So long as Vaughan and I can keep the Venus tablets in the eighth and seventh centuries, it does not matter to us that Ammisaduqa might be identical with Artaxerxes III Ochus. If Ammisaduqa did live some centuries *after* the Venus tablets, that would not bother us at all.

Nonetheless, there are two other lines of argument that did eventually convince me of Heinsohn's thesis. Both involve retrocalculation, and thus both would be condemned by Heinsohn himself. He is accustomed to dismiss any such retrocalculations as "phony," "unscholarly," and all-around "pseudo-!"

One line of argument proceeds from the rather extensive archaeological evidence that the First Babylonian Dynasty would have been nearly, or at least roughly, contemporary with the Twelfth Dynasty. But there is overwhelming astronomical evidence that the Twelfth Dynasty ended with the coming of Alexander the Great. Thus I *must* put the First Babylonian Dynasty at about the time of the Persian Empire anyway.

The other line of argument directly addresses the identification of Hammurabi of the First Babylonian Dynasty with Darius the Great of the Persian Empire, as well as the identification of Ammisaduqa of the First Babylonian Dynasty with Artaxerxes III Ochus of the Persian Empire.

I *love* to be able to prove my case by citing my opponents. Thus it is with great pleasure that I call your attention to an esoteric work by Peter J. Huber. It is entitled *Astronomical Dating of Babylon I and Ur II*, and it contains the attested intercalations under Ammisaduqa (page 60) as well as the attested 30-day months under Ammisaduqa (page 65). We can also make use of Langdon-Fotheringham-Schoch here (pages 61 and 77). Further, I remind you of the useful little book by Parker and Dubberstein, entitled *Babylonian Chronology 626 B.C.-A.D. 75*; it contains retrocalculations of all of the lunar months under Artaxerxes III Ochus (page 35).

I will not go into all of the details, but it is possible to get rather good fits here.

If we have Year 1 of Ammisaduqa start on February 12 Julian, -357, then 24 of the 28 attested 30-day months seem to check out. All four of the misses are one day late and are at the end of 29-day months; thus bad seeing at those four points is very strongly indicated.

If we have Year 1 of Hammurabi start on April 1, -520, then 25 of 27 attested 30-day months seem to check out. (See Huber, pages 57 and 62-63, Langdon-Fotheringham-Schoch, pages 63-64 and 77, and Parker-Dubberstein, pages 30-31.) There is one complication in the Hammurabi-Darius case, in that Hammurabi reigned for at least 43 years and Darius for only 36, but I would argue that there was a revised year-count, somewhere between Year 7 and Year 26, which was for the purpose of back-dating the reign by nine years, and thus depriving the hated Kambyses of his very existence! This was a common practice in antiquity. It seems that the Babylonian-"Hammurabi" tradition went along with this move, but the Persian-"Darius" tradition did not. (A peculiarity of the Hammurabi intercalations in the later stretch of the reign is that on the average they seem to occur about twice as often as they would with the "normal" rate of once every 2.7 years. Perhaps Darius/Hammurabi was in a big hurry to get the Babylonian calendar in phase with the Persian calendar.)

The details of all this are in Appendix One of *Sun, Moon, and Sothis*.

I conclude that Heinsohn's thesis is correct, even if he himself has had the bad judgment to condemn as "phony," "unscholarly," and "pseudo-" the very sorts of evidence that have finally made his case for him!

MANETHON

Manethon recognized either Thirty or Thirty-One Dynasties. (Opinions are divided, but my own inclination is to treat the Thirty-First Dynasty—the returning Persians—as authentically Manethonian.) What happens to Manethon if we move the Middle Kingdom?

Once again, this is not the place to go into full detail, but Manethon is the subject of Chapter Twenty-One of *Sun, Moon, and Sothis*. The gist of my position is that Manethon may have recognized *three* stretches of Egyptian history that seemed to run more or less side-by-side, but that he then numbered *all of them* consecutively, no doubt for the purpose of giving the Egyptians a longer-looking history than various of their neighbors.

Dynasties One through Thirteen could have been of the Valley (perhaps with some deletions: who knows what Dynasties Seven through Ten might have amounted to?). Dynasties Twenty-Two through Thirty-One could have been mainly of the Delta. Dynasties Fourteen through Eighteen, then a gap, then Nineteen, then another gap, and then Twenty-One straddling Twenty would constitute another, somewhat catch-all sequence. Velikovsky has Nineteen identical with Twenty-Six anyway, and Twenty identical with Twenty-Nine and Thirty. I do not know what the exact relationship of the Eighteenth Dynasty and the Old Kingdom was, but the Old Kingdom does seem to straddle the Eighteenth Dynasty, at least temporally.

Whether any of this is workable remains to be seen. But as for now, I am suggesting that Manethon was working with these three sequences of dynasties, and that he assigned consecutive numbers to every one of them, as if they were all in a single string. Thus many of the numbers functioned more as labels than as sequential indicators: Twelve, for example, came well after Eighteen, not well before it. These Manethonian numbers caused later historiographers great difficulty, but Manethon was able to add up the years of all of those dynasties, just *as if* they were consecutive, and thereby give Egyptian history a much greater antiquity than it in fact deserved. Whether he fooled anybody *at the time* is unknown.

THE TEFLON ICONOCLAST

In *KRONOS* VIII:2 (page 70), when I was summarizing my debate with Parker, I mentioned that:

" . . . not a single one of the mistakes by Parker or by me that have come to light in our exchanges is to be found in the Supplement to *Peoples of the Sea*. Velikovsky managed to discuss all of these subjects there without falling into any of the traps that snared Parker and me."

When the Middle Kingdom is lowered by 1477 years, this would presumably be disruptive to almost any chronology, conventional or revised. Or would it? Just what is the damage to Velikovsky here?

Much less than one might think.

The end of the Middle Kingdom is of course no longer to be associated with the Exodus, and any of Velikovsky's statements to that effect will have to be reexamined. But for the most part Velikovsky began his chronological work with the Exodus, that is, somewhere in the middle of the second millennium. He did assume that the Middle Kingdom was *ending* then, but the fact is that he had relatively little to say about the Middle Kingdom itself.

Thus the subtitle of *Ages in Chaos* is "From the Exodus to King Akhnaton." And in the subtitle of the *Theses*, Velikovsky refers to what he took to be that same starting-point: "From the End of the Middle Kingdom in Egypt to the Advent of Alexander the Great." *That* was the stretch on which his attention was always concentrated.

Velikovsky's placement of the Exodus can be retained, along with the Venus episodes, but it will now be necessary to look somewhere else than the Middle Kingdom for the long-sought "Pharaoh of the Exodus." (The early *Old Kingdom*, perhaps?) The lamentations of Ipuwer and the shrine of El-Arish would also have no further relevance to the end of the Middle Kingdom—though it might still be argued that they do pertain to the Exodus.

* * *

What about the later chronology? Well, here again there is no really radical impact upon Velikovsky's work.

The Twelfth Dynasty would have begun early in the Twenty-Seventh Dynasty and would have ended at the time that the Thirty-First Dynasty ended. Could the Twelfth Dynasty have existed during all the turmoil that is described in *Peoples of the Sea*?

Yes. All that we have to do here is to recognize that Egypt, referred to by the Egyptians themselves as the "Two Lands," was indeed Two Lands: the Delta and the Valley. Very often they were in different hands, even though every claimant to the throne boasted of having prevailed over all of his rivals. Whatever the facts, and no matter how small a territory you actually controlled, the fashion was to call yourself ruler of *both* the Two Lands. This was almost as true of the foreign conquerors as it was of the native kings.

I do not mean to suggest that a division of the Two Lands meant complete isolation, or constant warfare. Indeed, there seems to have been a tradition of cooperation, or at least of limited toleration.

Donations were made from afar, and even temples and other edifices were sponsored at the holy places, all without much regard for boundaries. That was the ancient way. And at Thebes, for example, the burial rights of all Egyptian kings were usually respected, whether they had actually ruled in Thebes or not.

Apparently the Twelfth Dynasty was a Valley power, and the Persian invaders (the Twenty-Seventh and Thirty-First Dynasties), as well as the intervening native rulers (the Twenty-Eighth, Twenty-Ninth, and Thirtieth Dynasties) were primarily active in the Delta.

Otherwise, *Peoples of the Sea* is left intact. Velikovsky can still equate the Twenty-Ninth and Thirtieth Dynasties with the Twentieth, and thus get what he calls the Sebennyitic Dynasty.

* * *

I do not know how long the Eleventh Dynasty lasted, but if it ended in about -500, then there is every reason to believe that it had a considerable temporal overlap with the later part of the Twenty-Sixth Dynasty, which Velikovsky equates with the Nineteenth. Here again, the implication is clear: if the Valley was in other hands, this Tanitic Dynasty of Seti II and Ramses II must have been active primarily in the Delta. (Of course, it is also possible that the early Eleventh Dynasty was a vassal dynasty, under the Tanitic thumb.)

In any case, *Ramses II and His Time* is left largely intact. Once these territorial limitations and other such factors are recognized, there is little else about Velikovsky's later chronology that would need to be changed.

* * *

How is it that moving the entire Middle Kingdom by a full Sothic period can do so little damage to Velikovsky's chronology?

If we discover something that seems to be true, even though no one expected it, we should really not be surprised if that discovery proves to be consistent with an older chronology that, at least with respect to the main points, is itself true. Truth is *always* consistent with truth. Only a *false* chronology will have any trouble dealing with a Middle Kingdom that was ended by Alexander the Great. *Velikovsky's* chronology can *easily* accommodate a Middle Kingdom that was ended by Alexander the Great.

But it is not simply that Velikovsky is *undamaged* by this lowering of the Middle Kingdom; he is also greatly *benefitted* by it. The Middle Kingdom evidence that I assembled in "Do Ancient Calendars Contradict Velikovsky?" features years of 365 days and mean lunar months of about 29½ days. Such evidence, if left in, say, the early second millennium, would be *deadly* to the astronomical scenario of *Worlds in Collision*. But when it is lowered to the middle of the first millennium, and is safely *this side* of the Velikovsky Divide, it presents no problem whatsoever for Velikovsky! That is why I stress that not only is Velikovsky not *hurt* by the lowering of the Middle Kingdom, but also he is tremendously *helped* by such a lowering.

TRANSCRIPTS OF THE MORNING AND EVENING SESSIONS OF THE A.A.A.S. SYMPOSIUM ON "VELIKOVSKY'S CHALLENGE TO SCIENCE", HELD ON FEBRUARY 25, 1974

Transcribed and Edited by Lynn E. Rose

INTRODUCTION

Full, verbatim transcripts were prepared by me between 1977 and 1979, covering both the Morning Session and the Evening Session of the A.A.A.S. Symposium in San Francisco; these were based not only upon my own tapes but also upon other tapes kindly provided by Warner B. Sizemore and by Frederic B. Jueneman. A few spots that have remained inaudible are marked with "[?]," "[inaudible]," or the like.

The prepared papers themselves are simply *mentioned* at the points where they were delivered; they are not included as part of the transcripts. All six of the speakers eventually published papers elsewhere anyway, either in *Pensée* IVR VII or in *Scientists Confront Velikovsky* or in *Velikovsky and Establishment Science* (*KRONOS* III:2). Velikovsky's paper was ready to be printed on the very day of the Symposium, and three of the other papers were also published more or less as delivered. In various noteworthy respects, Huber's paper was established *altered* prior to publication (see *KRONOS* IV:2, especially pages 33-34 and 53-54). Sagan's own paper, as many now realize, was *radically* revised and *greatly* expanded, virtually into a new paper. Much of that new paper, including *all* of the much-touted Appendices, was not seen by Velikovsky or by any of his supporters until nearly *two years* after the Symposium. Meanwhile, Velikovsky was being required to answer in 30 days a paper that Sagan had taken nearly two years to produce! But that is another story.

The editing of the transcripts themselves has in nearly all cases been by way of deletion. If a speaker repeated the same word, or the same string of words, I have deleted the repetitious material. If a speaker made an error, and immediately corrected that error, I have deleted the incorrect version. If a speaker began a sentence, abandoned it, and started a new sentence, I have deleted the incomplete sentence. (All "uhs" and the like have also been deleted.)

If a speaker made an error, and did not correct it himself, I have not amended his actual remarks. In such situations, and in other situations, as well, I have sometimes inserted editorial notes in square brackets. But I emphasize that everything *not* in square brackets was actually spoken.

For the sake of readability, I have sometimes deleted a superfluous word, or even an inappropriate s. In other cases, an ungrammatical form has been deleted in its entirety, but then replaced by the correct form in square brackets.

Let me illustrate some of these editorial procedures. When Velikovsky referred to his *New York Times* article of the "twenty-first of July, nineteen thirty-sixty-nine," I simply deleted the "thirty." But when Velikovsky referred to Hatshepsut of the "Nineteenth Dynasty," and did not catch himself, I let that stand, and added a correction in square brackets. At one point Storer's actual remarks were: "No, I don't, I don't think that the, the panel has been set up. It's not rigged. And as far —It's, It's an occasion for the public to

watch a scientific debate." After deletion of the repetitions and the false start, this became: "No, I don't think that the panel has been set up. It's not rigged. It's an occasion for the public to watch a scientific debate."

Two of the participants (Velikovsky and Huber) were not native speakers of English, but I think it should be pointed out that the remarks of all of those who spoke (myself as well, when I raised a question from the audience!) seemed to cry out for the kind of vetting by deletion that I have just illustrated in the case of Storer. All of the participants have benefitted about equally from this. In no case have any of my editing procedures affected matters of substance.

Lynn E. Rose

THE MORNING SESSION

KING:

Good morning. I would like to welcome you to this first session of the American Association for the Advancement of Science, and to apologize, first of all, for our delay in getting started. One of our speakers has not yet arrived.

One of the functions of A.A.A.S. is to act as a bridge between scientists and the public, and, as science becomes more specialized, this responsibility becomes more important.

Today we are going to consider a set of ideas that have at their core a completely unconventional picture of planetary motion. Most scientists would say that this picture is totally impossible, because it violates many of the most firmly established principles of physics.

To this Dr. Velikovsky would reply that there is overwhelming evidence that these events really did occur, and that, if they cause difficulties for the scientists, it is up to the scientists to resolve their own problems.

No one who is involved with the organization of this symposium believes that Dr. Velikovsky's ideas are correct. Yet millions of people have read his books, and, after more than twenty years of condemnation by the scientific establishment, he still has a large and often devoted following.

It is for this reason that we believe that discussion of his ideas at a meeting of the A.A.A.S. is a public service. It's in this spirit that we present this morning's symposium.

The program includes six speakers. Since early on the first morning of the meeting some of you will have been unable to visit the registration desk to pick up your programs, I'll outline it briefly.

The first speaker is going to be Professor Norman Storer, of the City University of New York, who will give a sociological talk devoted to "The Sociological Context of the Velikovsky Controversy."

Then we'll have Professor Peter Huber, of the Eidgenössische Technische [sic] Hochschule of Zürich, who will talk about "Ancient Historical Records."

The third speaker will be Dr. Velikovsky, whose talk is entitled, "The Challenge to Accepted Ideas."

Fourth will be Professor J. Derral Mulholland, of the University of Texas, who will talk on "Considerations of Dynamics."

We will then have Professor Carl Sagan, of Cornell University, speaking on "Venus and Dr. Velikovsky."

And the sixth speaker will be Professor Irving Michelson, of the Illinois Institute of Technology, who will give a talk entitled, "Mechanics Bears Witness."

And, finally, as we have it scheduled, there will be an opportunity for Dr. Velikovsky to give an answer at the end of the program.

I would like to remind you also that our schedule goes on just this morning. We must vacate the room by one o'clock, and I do hope that if [only for the?] sake of the weariness of the audience, that we don't go on that long. [laughter] But we will resume our meeting again at seven-thirty this evening, where we will have all the panelists at that time seated on the platform, and we will have an open discussion, without any formal program, with the opportunity for everyone who wishes to participate.

We will have an opportunity after each speaker talks this morning for questions from the audience. I would like to ask that the questions be framed in the form of questions, and that members of the audience not use the occasion to make speeches. [laughter] I am sure you will bear with us in that. The time is somewhat limited, and we'll do our best.

Each speaker will have twenty minutes, and after each speaker we'll have about ten minutes available for discussion. There will be one exception to this rule. When the program was originally put together, Dr. Velikovsky insisted that he should have at least thirty minutes for the presentation of his ideas. I only learned last night that Dr. Velikovsky intends to overrun even this time limit. I can only deplore this, and hope that Dr. Velikovsky will return our courtesy in inviting him here by keeping the length of his talk within reasonable bounds. [laughter]

Well, you haven't come here to hear me talk, [laughter] so let's move on now to our program. [laughter] The first speaker is Professor Norman Storer, of Baruch College in the City University of New York, where he is Chairman of the Sociology Department. Professor Storer has made a specialty within sociology of studying the sociology of the scientific community, and he's going to give us a talk entitled, "The Sociological Context of the Velikovsky Controversy."

And may I mention that I have, courtesy of my wife, a little timer, and I'll ring a bell at eighteen minutes and set it again for two minutes.

STORER [to King]:

Do you want me to field questions . . . [inaudible] . . . ?

KING [to Storer]:

I will come up again and help you take questions.

STORER [to King]:

Great.

STORER:

[Storer's paper, entitled "The Sociological Context of the Velikovsky Controversy" was presented at this point.]

That's the end! [applause]

KING:

We have some time now for questions from the other participants or the audience. Yes.

QUESTIONER:

Yes. Dr. Storer?

STORER:

Right.

QUESTIONER:

Yes, I would like your comment on the introduction that Dr. King gave, which, to me, put this symposium in the context of the recognized scientists' setting the laymen straight on what's really going on, with no mention of the validation of some of Dr. Velikovsky's assertions, not that that makes his conclusions correct.

STORER:

All right. The question is, would I comment [delayed applause], would I comment on Professor King's introduction, which the questioner *construed* as saying, "Here is real science, and we're gonna show you people what's wrong with Dr. Velikovsky." I don't think it needs to be read that way. [laughter] As a matter of fact, my stance, anyway, is, is determined, dogged neutrality on this. [laughter] Nobody would believe me if I said, sure, comets do this or that.

No, I don't think that the panel has been set up. It's not rigged. It's an occasion for the public to watch a scientific debate.

STORER and KING [briefly conferring]:

... [inaudible] ...

STORER:

Next. The lady over there.

QUESTIONER:

As a sociologist, I would seriously like to challenge a great many of the things that Professor Storer has been telling us about the sociology of science. I can't begin to go into some of the reasons why I feel it's very much open to question. I would like to recommend that some of you look at Stuart Blume's *Toward a Political Sociology of Science*. And he also ... [inaudible] ... the power of lobbying.

STORER:

Could you give the second reference again?

QUESTIONER:

The separate table of the power of lobbying ... [?]

STORER:

Oh, I see. Yeah, I happen to be reading that book right now. It's a good book.

QUESTIONER:

Stuart Blume, *Toward a Political Sociology of Science*.

STORER:

Toward a Political Sociology of Science, by Stuart Blume, published by Free Press in this year.

KING:

Back there.

QUESTIONER:

I wonder if Dr. Storer, offhand, could give me just two examples in which a brilliant new idea now accepted as fact was welcomed by the scientific community. [laughter, applause]

STORER:

I am tempted to defer this to some of the historians of science here. [laughter] It's my understanding that Albert Einstein's ideas met very little resistance among the top physicists of that day. You disagree with that statement.

QUESTIONER:

... [inaudible] ... the mathematicians.

STORER:

I'm sorry. What?

QUESTIONER:

He was attacked by the mathematicians. The second rank took him off.

STORER:

Oh. [laughter]

KING:

Dr. Mulholland.

MULHOLLAND:

I would like to reply to the last question. I think, [laughter] I think two examples that can be brought to answer that question are the discovery of mass concentrations on the Moon and the internal heat in the Moon, which have both thrown the discussion of the history, the evolution of the Moon, into a state of extreme excitement, and has totally rejuvenated the entire subject. [applause]

KING:

I should mention that, with the lights shining in our faces here, it's a little bit hard for me to see people's hands, so raise them high.

QUESTIONER:

May I ask—

KING:

Yes.

QUESTIONER:

I would have thought the normal way of dealing with a crackpot is to ignore him. Is it the usual practice in scientific publications to review books by proclaiming that you have not read them before you review them? [laughter]

STORER:

It's frequently charged by the injured authors of those books, [laughter] and denied just as often by the men who did review them.

KING:

One more question.

VOICE:

Mr. Velikovsky had his hand up.

KING:

Oh, I'm sorry. Did you wish to say something? [laughter]

VELIKOVSKY:

I wish to ask Professor Mulholland whether he knows who was the first to claim, in time, a steep thermal gradient under the surface of the Moon?

I wish also to ask whether there is an explanation for the mascons on the Moon, beside the explanation that the Moon was close to some heavy, gravitating body that pulled out some mass towards the surface? [applause]

And besides, would you consider these two observations as fundamental theories?

VOICES:

No, no.

KING:

Can you answer that briefly?

MULHOLLAND:

Yes. [delayed applause] I regret to say I do not, in fact, know who might have first suggested the Moon was hot inside. I will acknowledge definitely that Dr. Velikovsky did say so, many years ago. And I must blushing admit that he has put a finger on a weak point in my statement, because what I gave as the response a few moments ago were observational determinations rather than theoretical structures. [applause]

VOICE:

I think we refuted it . . . [remainder inaudible] . . .

KING:

I am sorry we have not been provided with a second microphone. What I will ask, since it's understood that people are asking questions rather than making speeches, I'll ask that, if a question is not easily audible, that the person who is up here at the microphone repeat the question, as Dr. Storer did with at least the first question that was asked of him.

We'll move on to our second speaker now. Professor Peter Huber, of the Eidgenössische Technical [sic] Hochschule in Zürich, has made a study of the ancient archaeological records relating to astronomy. He also, incidentally, has a second specialty in statistics, and we're very pleased to have him speaking to us today on "Ancient Historical Records." Professor Huber. [Huber, of the Eidgenössische Technische Hochschule in Zürich, has statistics as his first and only professional specialty. He also, incidentally, has repeatedly described himself as a "hobby-assyriologist." Thus King has conferred upon Huber a profession status that Huber does not have. The A.A.A.S. Program misrepresented Huber in the same sort of way, describing him as a "Prof. of Ancient History" (page 23).]

HUBER:

[Huber's paper, entitled "Early Cuneiform Evidence for the Planet Venus," was presented at this point.]

That's the end! [applause]

VOICE:

Question?

KING:

Dr. Velikovsky says he has several questions, and would like to use the microphone for them.

VELIKOVSKY:

Understand, I had not chance to have your paper before this morning, so I did not know the phenomena that you would record.

We had yesterday a short chat. You mentioned that the most important statement is an eclipse that was calculated for something like – what would it be?

HUBER:

Perhaps I get the document. [pause] What is most important eclipse is a total eclipse of -708 [astronomical; 709 B.C. would be historical], July – which, I've forgotten – July 17, which –

VELIKOVSKY;

It is from China?

HUBER:

It's from China, from these *Spring-Autumn Annals*.

VELIKOVSKY:

What is from Ras Shamra? You spoke of Ras Shamra.

HUBER:

No, I didn't mention Ras Shamra.

VELIKOVSKY:

But you mentioned to me yesterday –

HUBER:

No.

VELIKOVSKY:

– that most important –

HUBER:

No, not Ras Shamra.

VELIKOVSKY:

Fine.

HUBER:

I'm sorry.

VELIKOVSKY:

Well, Chinese date, was in this document mentioned also the place?

HUBER:

For this particular eclipse the place is not mentioned, but – [laughter]

VELIKOVSKY:

As long as –

HUBER:

But there is something else. For some other eclipses it is mentioned that the eclipse happened in the province. The inference is that this particular eclipse happened at the capitol. And to make precise, what I mean is, if you take the probably most reliable eclipse we have now from antiquity, it's Babylonian eclipse of -135 [astronomical; 136 B.C. historical], and use this to determine the –

VELIKOVSKY:

Which eclipse?

HUBER:

Babylonian eclipse, -135. We only learned about it last December. [laughter] It's very definite, description of a total eclipse, with all the details. If you take this eclipse, which is absolutely certain, and –

VELIKOVSKY:

That's 135?

HUBER:

Ja. And if you use this eclipse to determine the values for the secular accelerations, and calculate back to -709 [historical], you obtain the eclipse as total right at the capitol of where this dynasty was reigning.

VELIKOVSKY:

Let me ask you, Professor Huber, are you familiar with the same discussion that I had with Princeton astronomer Stewart, printed in June, 1951 issue of *Harper's*?

HUBER:

Ja.

VELIKOVSKY:

You are. He brought at that time, on the basis of a lecture of Fotheringham, three ancient eclipses: one from China, one from Assyria, one from Babylonia. I replied. Stewart claimed that three only existing established dates of full solar eclipses. I replied. I have the reply with me. Do you agree with Fotheringham and my opponent, or do you agree with me today?

HUBER:

I agree you were quite right in rejecting these three Fotheringham eclipses as right evidence.

VELIKOVSKY:

Yes.

HUBER:

[They are] not well-dated.

VELIKOVSKY:

So—

HUBER:

The date is established astronomically in these cases.

VELIKOVSKY:

So in that case we will say so, that the argument that was brought by astronomers in 1951 in the debate on the pages of *Harper's*, three eclipses as if established, were, well, answered by me, and I showed that none of them was really eclipse, neither the date could be a date of eclipse, because eclipse doesn't happen on the twenty-sixth of a lunar month, neither the places were indicated, and neither they fit into chronology. Place is very important. If the total eclipse is in Brazil, you cannot look into records of North America.

Now, next question. Do you believe that, as you have written to me, there is some very strong argument, for one specific eclipse that is beyond any doubt, established by Stephenson, I believe?

HUBER:

Stephenson and Muller, yes.

VELIKOVSKY:

Did they publish their work?

HUBER:

It's not yet published. I learned about this last January.

VELIKOVSKY:

Yes.

HUBER:

It's going to be published in the proceedings of a conference on changes in the rate of rotation of the Earth—

VELIKOVSKY:

Do you know the year of the eclipse?

HUBER:

Which eclipse do you mean?

VELIKOVSKY:

Of Stephenson, the one he claimed [as] the one, and you believe, it is the most strong evidence?

HUBER:

The most strong evidence against these catastrophes, in minus sixteen [presumably meaning the eighth century]—?

VELIKOVSKY:

Yes.

HUBER:

That is the one of minus seven hundred and eight, July 17.

VELIKOVSKY:

No, I asked you about the work of Stephenson.

HUBER:

Yes, that's the work of Stephenson.

VELIKOVSKY:

Did not Stephenson wrote about the eclipse discovered in the library of Ugarit?

HUBER:

I am not aware of—

VELIKOVSKY:

Are you aware of his publication in *Nature*?

HUBER:

Which publication in *Nature*? We had a discussion—

VELIKOVSKY:

About the eclipse yesterday.

HUBER:

We had a discussion—

VELIKOVSKY:

Yes.

HUBER:

—yesterday—

VELIKOVSKY:

About the eclipse.

HUBER:

—and we couldn't agree on which publication it was.

VELIKOVSKY:

He published only one paper in *Nature* on one eclipse, that he believes this is the only one [that early] that he established with complete, absolute, so to say, firmness, and he referred to the library of El-Amarna [meaning Ras Shamra].

HUBER:

I am not aware of that.

VELIKOVSKY:

You were not aware. It was published in *Nature*. It was published by Stephenson in *Nature*. This issue is of November 14, 1970. He speaks about the eclipse of 1375. He believes that this is the only one [that early] that is established beyond doubt, and let me say, if you have read my *Ages in Chaos*, you know, of course, that Ugarit is no more, in reconstruction, related to the fourteenth century, the library of Ugarit, but to the ninth century. So in that case, of course, all the calculation would not fit.

Interestingly, also, it is said that Rashap, which is Mars—correct?—was in attendance. Interestingly, this eclipse is described in Greek sources; [it] is described, however, as something very different from regular eclipse. The Sun was disturbed in its motion, and Stephenson printed: "The Sun went down (in the daytime) with Rashap [or Mars] in attendance." And we have exactly the same statement in Greek sources, referring to the date when Romulus supposedly was born, that Mars caused disturbance in movement of the Sun, and at the same time it occurred that Sun and Moon were in eclipse.

Well, let us come to the question of Sumerian materials that you claim that Venus was referred [to] in early ages. You refer to 3000 B.C., and to 1900 B.C., and to the time of Ammizaduga tablets.

Now, let me ask you, this Sumerian hymn, in your opinion, refers—and is the best proof that Venus was already observed earlier than it became a morning and evening star. That Venus was observed before it came into conflict with Earth is clear from what I wrote. It did not come from Jupiter just on the eve of that collision. [laughter] It came thousands of years before. It could be seen. However, you are right. In that hymn, Venus is referred [to] as connected with morning and evening. But what is else in that hymn? And I am very thankful to you for giving me the text of that hymn.

First, it is in Sumerian. Sumerian as a living language really extinguished rather early. But Sumerian was the Latin of the cuneiform-writing people, and it survived as long as Latin survived, past the Roman Empire, so the fact that it is written in Latin doesn't say much about the age.

Here is spoken about Inanna. Let us assume that Inanna referred to Venus. So we know that Ishtar—and I stressed this in my book—at some time in the past was the name for Jupiter, became later the name for Venus.

Now, "Inanna shines as bright as the Sun." Is Venus shining as bright as the Sun today?

Now, in the same hymn, says, Inanna is a star foreign to us, *fremdartige Stern*, not from this family.

Now, it's again said, on daytime, on midday, it shines as bright as the Sun. Does it today?

HUBER:

I . . . [inaudible]

VELIKOVSKY:

Also it says during the night as the Moon.

HUBER:

You are twisting the translation from German into English.

VELIKOVSKY:

"Zur Nachtzeit sendet sie Licht aus wie der Mond, am Mittag sendet sie Licht aus wie die Sonne."

HUBER:

Which means that —

VELIKOVSKY:

"shined as bright as the Moon in the night, shined as bright as the Sun — "

HUBER:

The "bright" is not there.

VELIKOVSKY:

Where is?

HUBER:

She sends out light like the Sun.

VELIKOVSKY:

Like the Sun?

HUBER:

And this passage —

SAGAN:

Dr. Huber, talk into the microphone, I can't hear.

HUBER:

Yeah. This passage, actually it was used by Schaumberger in the third *Ergänzungshefte* [to Kugler's *Sternkunde und Sterndienst in Babel*] as an argument that Venus was visible during the day, and you quote, in

Worlds in Collision, that passage from Schaumberger, if I remember correctly. [See *Worlds in Collision*, page 164.]

VELIKOVSKY:

Yes, and I quoted many other passages from Babylonian sources that say that Venus is like a torch, like a torch in the sky, that Venus covers all the sky. And this is not only from Babylonian sources.

Now, also here is spoken about honey and cakes being given to Inanna. If it is Venus it would be exactly what was given later to Athena, and which is also observed in so many religious cults up to today. [laughter]

Now, let me ask you, [laughter] as to this Sumerian hymn, it would be good if you could discuss it on the basis of the original, because this is the German translation, again translated into English. Do you read Sumerian? [laughter]

HUBER:

I read cuneiform, but I do not really speak Sumerian language. [laughter]

VELIKOVSKY:

No, I didn't ask whether you speak Sumerian language. I asked you whether you read Sumerian language.

HUBER:

I'm not so familiar with Sumerian as a Sumerologist would be.

VELIKOVSKY:

Fine. So you are not familiar with Sumerian language. [laughter] Let us say, let us ask you, [as laughter finally dies away] let us ask you whether cuneiform in Akkadian language is, well, your main occupation. Do you teach cuneiform or ancient history in Zürich?

HUBER:

No, I don't.

VELIKOVSKY:

You don't. SO you don't teach [them]. You teach, I understand, and you are very foremost in your field of statistics, and it is correct that Akkadian language, self-taught, is your hobby?

HUBER:

Yes.

VELIKOVSKY:

Correct?

HUBER:

Not quite self-taught.

VELIKOVSKY:

Well. Well. [laughter] Now, let us say this. The Babylonian sources, by Weidner and by many others, show the fact that for long periods of time, as also in India, [there] was in Babylonia four-planet system. Later Venus was figured, as you have seen, together with the Sun and the Moon, in a triad, separately from the planets, and it was called the new planet that joined the other planets.

And then it of course was referred to as moving not in a perfect orbit. Here were the tablets of Ammizaduga. As to tablets of Ammizaduga—in the hard-cover edition of *Worlds in Collision*, pages 199-200 [the entire discussion being cited extends from page 198 to page 200], if my memory is right, are dedicated.

It is not as it was shown here [in Huber's slides], *if* Venus—this is a translation, because otherwise it could not be understood. In the Akkadian text there is no such things as, *if* Venus appears on this day or on that day. Just it is said, it appears on this day or on that day. And there is a way to check on it. It is mentioned. It appears on that day. It disappears on that day. And in between ar so many days. You have the way to check, because if from fifteen to Sivan to the seventeen of Tammuz, or whatever the dates are, you can calculate by the calendar, but, interestingly, by the calendar of thirty days in a month, and thirty days in the month without intercalary months is the prerequisite to understand what is going on there.

Those who try to understand those tablets and to translate them needed to correct the translators and ascribe to scribes great errors. West is changed into east. Evening is changed into morning. Nine months and five days are changed just into five days [the interval of nine months and five days is based on B.M. 36395; several other tablets suggest that the interval was nine months and four days], to make sense, because, as today, Venus, when in inferior conjunction, which means between the Earth and the Sun, disappears from sight for approximately one single day, but when it is in superior conjunction, which means when the Sun is intervening between Venus and the Earth, today it is about—not always exactly so—two months and six days.

Now, in the tablets it is nine months and several days, and very different other figures which are not given to understanding. It is nothing of the "if." It is just as it is.

Now, interesting again, as I say, it is a calendar of thirty days, without intercalary months, even if there are two references to Elul the second. Will you say that there is no reference in Langdon and Fotheringham to thirty-day calendar, without intercalary—

PANELIST:

Give him the microphone.

PANELIST:

Give him the mike!

KING:

Could you let Dr. Huber have the microphone?

VELIKOVSKY:

Yes.

KING:

He has a number of things to answer now.

VELIKOVSKY:

Yes.

HUBER:

One point is the question of the "if." Now, that's really a question pertaining to essentially all omina. Many of these omina begin with just a vertical bar at the beginning. Now this vertical bar is either the stenographic notation for *summa*, "if," or it's something like our horizontal bar, if you make a list. Usually it's taken as the "if" nowadays, and I just joined the majority. It doesn't really matter if you replace it by a horizontal bar. The factual meaning is the same.

But the question of the intercalary months is: we have intercalary months from documents which were written in the old Babylonian times, and I thought I made quite a fuss about the fact that seven intercalary months were recorded in contracts written in the time of Ammizaduga, and that these same intercalary months could be established from the Venus tablets. [Actually, there are eight or even nine attested intercalary months from the time of Ammizaduga, and only four of these clearly fit the months that would be required for a uniformitarian reading of the Ninsianna tablets; in addition, there are three months required for a uniformitarian reading of the Ninsianna tablets that are *not* attested from the time of Ammizaduga: Huber's claimed seven-for-seven fit is a fabrication.] That was my main argument for establishing the date of the Ammizaduga tablets. And these intercalary months are discussed by Fotheringham in Langdon-Fotheringham-Schoch. That's one comment.

The second comment, you said something about Venus joining the rank of the great stars, if I am quoting correctly. Now, I followed that quote through. This is one of the quotes which I mentioned in the beginning, as they are based on a questionable translation. I took care to take along the cuneiform text of that. And I can tell you exactly what happened there. The cuneiform text has something—"the great star which is beyond the great stars which in the certain part of the sky." Now, "the great star which is beyond the great stars." That is a literal translation. Somehow, this got into "the great star which joins the great stars." But there's a grammatical technicality involved. Akkadian doesn't have the superlative. You have to express the superlative by syntactical means, and what this means is nothing more [than] "the great star which is the greatest of the great stars" which is, oh, that's a grammatical question. And I didn't want to go into these details, but since you started it, I have to do it. [See *KRONOS*, IV, 2, pages 35-36.]

VELIKOVSKY:

I wish to refer again to Ammizaduga tablets. Ammizaduga tablets were tablets describing twenty-one years of appearance and disappearance of Venus. These tablets were ascribed by [that is, "to"] Ammizaduga by Jesuit Father Kugler. Before this they were thought by astronomer and orientalist

Schiaparelli, as referring to events of the seventh century B.C., not of the time of Ammizaduga, which would be fifteen, fourteen, or whatever century, or even earlier.

When I presented those pages in *Worlds in Collision* and quoted Ungnad and Langdon and several others to the effect that they cannot understand and explain these movements of the planet Venus if we assume the planet Venus was, as today, moving on the very same orbit, I did not insist on the time to which those planets [meaning "tablets"] refer. There is no name of Ammizaduga on them. There is only year formula found on one of the tablets [that is, in one of the *entries* on the tablets, the one for Year 8b], and Professor Hommel, who investigated the question, past Schiaparelli, came to the conclusion that this year formula was inserted by a scribe, and the tablets do *not* belong to the time of Ammizaduga.

Now, again, what is the time of Ammizaduga? Ammizaduga was the last king of the First Babylonian Dynasty that started with Hammurabi. When I started my work, the research on it, Hammurabi was put in twenty-second century. Since then, the work of Albright and Sidney Smith reduced it more and more, until today it is 1680, approximately, the time till when Hammurabi ruled, and Ammizaduga would be at least a hundred years later. So Ammizaduga would be in that case just before the time of the Exodus, or the end of the Middle Kingdom in Egypt.

But if Hommel and Schiaparelli are right—and there is reason to think that they are right—the reason is exactly the fact that the calendar used in these calculations of the scribes is thirty-day months, and there is no mistake on this. This needed to be stressed. When in the tablets it is mentioned from this day to that day, immediately is given also the way of checking, by number of days inserted—not inserted later, inserted immediately in the text—they show that the months were thirty days long, and there were only twelve months, and there were no intercalary months, even if some occasion was Elul second.

Now, on this basis, I come now to the conclusion to which I had not yet come when I wrote *Worlds in Collision*, namely, that those tablets were a little earlier than Schiaparelli thought, but not so much earlier. Certainly they are not of the time from the First Babylonian Dynasty. It would make no difference for the thesis that catastrophic events took place, that Venus did not move as it moves, but it is just for the purpose of establishing something of historical value.

Thirty-day months, twelve months, year of 360 days: as I put quite a long list, actually, from all ancient calendars, from Inca and from Mayas, from Peru—which [Mayas] means in Mexico—from all ancient European, like ancient Roman and Greek, and also Asian, Near Eastern, and Far Eastern civilization. From each of them I put quotes from authority: twelve months of thirty days, strange as it is, without intercalary. Intercalary months were brought later in. And so later there were two Moon's calendars, Moon calendar of thirty days, and the new Moon calendar.

Well, in these circumstances, I come to the conclusion that Ammizaduga tablets were created between the time of the catastrophic events of the middle second millennium and the catastrophic events that took place from the 776 on, from which the Greeks counted their Olympian Age, and more probably in the later part of it [that is, probably in the tenth, ninth, or eighth century], and then it will be very plainly what it is.

However, this disappearance to nine months and more, interestingly, is not a disappearance due to going of Venus beyond the Sun, as it would be in superior conjunction, because even then Venus was seen like a torch, and going behind the Sun would not hide it enough.

However, we have a series of data from many civilizations, also from China, like Soochow table, that Venus at that time was traveling to the south, was not traveling in ecliptic, which means in the plane of Earth's revolution. It was traveling to the south and reaching the star Sirius. Now, this is in various sources. Now, in that case, the disappearance of Venus would follow, not from going behind the Sun, but from disappearing as any southern star would disappear from the northern latitude where Babylonia or Egypt are located.

Thank you. [applause]

KING:

This is a discussion that clearly could go on for a long time. [laughter] I have put my head together with Dr. Huber, and have induced him not to reply to this until the evening session, in the interests of getting on with our morning program. During the evening we will have a free discussion, and I think I can freely predict that this particular vein will continue. [laughter]

Our next speaker on the program is Dr. Velikovsky. [laughter, applause] He has informed me that he has prepared manuscript which he has gotten together in the interests of speaking clearly, so that everyone will understand what he has to say. I have already said that I regret the length of it, but we'll allow him time to go through this manuscript. [applause]

VELIKOVSKY:

[Velikovsky's paper, entitled "My Challenge to Conventional Views in Science," was presented at this point.]

And thank you. [applause, lasting 35 seconds]

KING:

Thank you very much for your talk, Dr. Velikovsky, and also for your excellent and clear delivery.

I am getting very concerned about the hour of the day. We have three speakers remaining. We had planned a half hour per speaker, including the discussion, and we must be out of this room by one o'clock. Things are going to be very tight.

I will ask if there are any questions now that can be answered briefly, and I would like the answers to be brief, because we must get on to the other speakers. Yes.

QUESTIONER:

I was wondering if any of Dr. Velikovsky's predictions have turned out to be untrue so far, and if he would talk about those, if there are any, I don't know.

VOICE:

Repeat—

KING:

The question is, have any of Dr. Velikovsky's predictions turned out so far to be untrue, and would he discuss those?

VELIKOVSKY:

I do not know of any prediction proven to be disproven.

Professor Hess, the late Chairman of Geology at Princeton, who claimed that he knows at least one of my book by heart, *Earth in Upheaval*—it is a required reading in geology and paleontology at Princeton for over fifteen years—he was also Chairman of the Space Science Board of National Academy of Sciences that

has supervision over NASA activities—he made a public statement in writing that my predictions were made long in advance of discoveries, that when they were made they were far away from what was commonly thought, and actually in contradiction, and that he does not know a single prediction that went wrong. If anybody knows, let me hear.

KING:

Dr. Sagan.

SAGAN:

Right. These microphones wired?

KING:

I think this is the only one that is connected yet.

SAGAN:

I think I know a large number of predictions which are incorrect, and I also think that I can show that the ones which are correct are not original with Dr. Velikovsky, but I will get to that when it's my talk.

What I would like to ask, just to ask a specific question. In Dr. Velikovsky's presentation to us now, he has said that the hydrocarbon clouds of Venus are consistent with all ultraviolet, visible, near infrared, and far infrared observations, with the refractive index, and the volatility.

That is not my impression, so I'd like to ask, which organic compound has a refractive index of 1.44, as we know the Venus clouds do, from the polarization data, has a 3.1-micron and 11.2-micron absorption feature in the infrared, and is able to explain the discontinuity in the water abundance above and below the clouds?

I ask this because about a seventy-five percent solution of sulphuric acid explains all of these very well, and I know of no organic compound which does. And I've read the papers by Burgstahler and Velikovsky in the latest issue of *Pensée*.

VELIKOVSKY:

What Professor Sagan here said is in advance of what he will say, so I cannot judge what he would claim as wrong predictions. I had only the chance to read *Newsweek* magazine statement this week, in which Sagan was quoted, after his visiting *Newsweek* editorial staff, that Velikovsky predictions are either very vague, or they are in contradiction to physical laws, or that they are not original.

I believe that he will have a hard time to prove this. Maybe we will not be able to discuss it all in the morning session. We will have the evening session; then we'll discuss it at greater length.

But let us go to the question of the Venus clouds. I claimed about Venus number of things, and all of them went into fulfillment.

I claimed about Venus that it will be found incandescently hot when it was thought that it is not much above the terrestrial annual mean temperature.

I claimed that Venus was disturbed in its rotation.

I claimed that Venus has a very massive atmosphere at the time when my opponent and critic, the Royal Astronomer of England, Spencer Jones, claimed that Venus has less atmosphere than Earth, and, as you know now, there are about ninety, maybe ninety-five atmospheric pressure close to the ground.

Now, as to the composition of the clouds, let us say the first thing this. The question of recentness of Venus is solved by the question of the origin of Venus' heat.

Professor Sagan clings to an unsupportable statement that this heat could have been a result of greenhouse effect. We will discuss this. Already many authorities –

VOICE:

That's not the question.

VELIKOVSKY:

Already many authorities put it clear: it could not.

Now, in the last issue of *Pensée*—which, by the way, will be found at the door of this hall, where representative of that Student [Academic] Freedom Forum organization has a table—I was given the opportunity to answer Professor Burgstahler, chemist of University of Kansas—[aside, to Lorraine Spiess] I wish number VI—as to the constituency of clouds.

I never put it that clouds must be composed of hydrocarbons. [Notice that this statement already makes the "specific" part of Sagan's question irrelevant.] I have, however, claimed that Venus had hydrocarbons three and a half thousand years ago, and some of the deposits of petroleum on Earth came from Venus' clouds, or trailing part of it.

But I also introduced this statement by words, "I assume." I also said under what circumstances they can be looked for and where: in the deep infrared, and probably not at the top of the clouds, because, as heavy molecules, by physical law they will not be *there*.

But then again, Burgstahler came up, in this article of his, review of the literature, with the idea that more probable sulphuric acid diluted in twenty-five percent of water reflect the conditions in various parts of the spectra.

I answered, and the answer in here in *Pensée* instead of quoting my answer, which can be read, on page 31 is a table that answers Sagan.

SAGAN:

It does not.

VELIKOVSKY:

The table is not my words. The words are of Burgstahler. As to the refractive index, as to the volatility, as to the ultraviolet spectrum, as to the near infrared, as to infrared, and as to deep infrared. In no occasion is any word of mine.

And there is also a statement of Burgstahler, added to my article: he "appreciate . . . Velikovsky lucid discussion . . . I appreciate . . . of my article," of his article, "and especially the provocative tabular presentation of the spectral comments drawn from it." [Burgstahler's complete statement was: "I appreciate Dr. Velikovsky's lucid discussion of my article, and especially the provocative tabular presentation of spectral

comments drawn from it." He then acknowledges Velikovsky's priority in explaining the yellowish coloring of Venus, and mentions the possible "compatibility of sulfuric acid clouds with the sustained presence of appreciable amounts of hydrocarbons, especially in the lower regions of the atmosphere."]

Now, the question was put to me, which of the organic molecules has the refractive index of 1.44. Let me say this, the entire problem started with an article by Professor Plummer, of University of Massachusetts, who published on the fourteenth March, of 1969, in *Science* magazine, an article questioning the presence of hydrocarbons in the clouds of Venus. I answered this article; however, [I have] not reworked it to the desire of the reviewers for *Science*, and it was printed now here in *Pensée*.

The question was of the refractive index, who claimed what. Plummer claimed water. Sagan claimed water. I claimed there is no water, because the refractive index is not of water.

Sagan was proven wrong, because 1.44 not refractive index of water, which is 1.33, approximately, ice and water. And today exactly this statement of mine is repeated by a number of scientists: Plummer was wrong, *Sagan* was wrong, because of refractive index.

Now comes *Sagan* and asks me, where is the refractive index of organic molecules? Here is statement of organic chemist, who is Professor Burgstahler, and I have with me two or three statements more, of Professor Harris, organic chemist, whose specialty [it] is, of Furman University in South Carolina, and another statement, of Professor Bush, of the North Carolina University in Charlotte, both working on the spectrum of infrared of organic molecules, stating that *many organic molecules have infrared index of 1.44*. And I have another statement, from a resident of this area, Dr. Ballinger, who works as research chemist on organic material for the Exxon Company of California, and the statement is again the same.

And besides, what is the question? Plummer, for example, investigated —

MULHOLLAND:

We've forgotten by now.

VELIKOVSKY:

What is the question? Plummer investigated seventeen organic molecules, not on their refraction index. There are hundreds of thousand of organic molecules, either hydrocarbons or carbohydrates. They were not investigated. And there are *many and many* that have the refracting index of 1.44.

KING:

May I ask you to terminate your answer now?

VELIKOVSKY:

Well, this is the answer. I believe I answered completely.

KING:

It was a very complete answer. [laughter, applause] We have on record your reference to page 31 of *Pensée*, and Dr. *Sagan's* remark that that does not satisfy his question. Let's leave it at that. We have two hours to discuss things in the evening. [Notice that King is still unaware that *Sagan* is leaving.]

Now, we have three more speakers on our program. The next two speakers are going to talk on different subject matter but in a similar vein, and the way I am going to organize the program is that I will ask Dr. Mulholland to give his talk, and hope very much that he will stick to the twenty-minute limit, and

after that we will have Dr. Sagan immediately, and following that we'll have a chance for some more discussion, which I hope will be brief. Remember, we have two full hours for discussion this evening, and we have one more speaker after both Mulholland and Sagan.

So let me introduce the next speaker, Professor J. Derral Mulholland, of the University of Texas, in Austin, who is a celestial mechanic whose name is almost synonymous with high precision. [laughter]

MULHOLLAND:

[Mulholland's preliminary remarks, not included in his paper, were as follows:]

Before I am asked the question, I would like to point out that I first read Dr. Velikovsky's work in 1950 in *Collier's* magazine when I was sixteen years old, and I have read that same work [sic] three times since, the most recent yet this year. [What *Collier's* printed was the equivalent of six magazine-size pages that were "Excerpted and Adapted by John Lear from *Worlds in Collision* by Dr. Immanuel Velikovsky"; Velikovsky objected to the way *Collier's* treated his book, since he had agreed only to serialization, not to condensation, and the planned third installment of Lear's condensation was never printed. *Worlds in Collision* itself contains xiii + 401 pages.]

I found it very entertaining when I was sixteen, incidentally, and I still do.

[Mulholland's paper, entitled "Movements of Celestial Bodies—Velikovsky's Fatal Flaw," was presented at this point.]

Thank you. [applause]

KING:

As I announced previously, we'll move on immediately to the next speaker, and I wish to amend something that I said earlier.

Unfortunately, Dr. Sagan will not be allow-, will not be available, will not be able to be with us this evening, on account of a previous commitment out of town.

I'll call on Professor Carl Sagan, of Cornell University, to talk on "Venus and Velikovsky."

SAGAN:

[Sagan's preliminary remarks, not included in his paper, were as follows:]

Thank you, Professor King.

I first started working on this paper, that I have here, on the invitation of Stephen Talbott, the editor of *Pensée*, who invited me to give a critique of Velikovsky's views about Venus, which I started to do, but then discovered that it's very difficult to keep one's focus only on Venus, because Velikovsky's perspective is extremely broad. And so what has come out is a manuscript called not "Venus and Dr. Velikovsky," but something called "An Analysis of 'Worlds in Collision,'" which is much too long to read here, and especially in the interests of time I'm going to just go through a fraction of it, something like a third of it. I don't know what Mr. Talbott will do when I talk about him about the manuscript.

Well—

[Sagan's paper, now retitled, "An Analysis of 'Worlds in Collision,'" was presented at this point. The decision to put *Worlds in Collision* in quotation marks rather than in italics was Sagan's.]

Thank you. [applause]

KING:

Thank you very much, Dr. Sagan.

Although I found your ten points immensely interesting, as chairman, trying to keep this meeting running, I feel as if I've been visited with the ten plagues. [laughter]

We are going to have to make a change in the schedule. It is obvious that discussion at this point is necessary. The time is already seventeen minutes to one. We are required to be out of the room at one o'clock or shortly afterwards.

And I must apologize to Professor Michelson, to be last speaker, that we must postpone his talk until the evening meeting. He has graciously agreed to do this, in order that we can have some discussion, which I imagine will be largely between Dr. Velikovsky and Dr. Sagan. [laughter]

I am sorry, Dr. Michelson, in my incompetence in manipulating people in the presence of ideas. [laughter, applause]

May I ask for one or two questions from the audience, in the hope that the questions will be brief, and the answers equally brief. Question.

BASS:

I have four brief questions that I wish to ask. [laughter]

KING:

You have been recognized to ask one question. Choose one of them, please.

BASS:

Where's Mulholland? Is Mulholland going to answer?

MULHOLLAND:

Yes.

BASS:

Yes. Yes, Mulholland. All right. Are you familiar with the published work of J. G. Hill's Yale Ph.D. thesis, 1970, Michael W. Ovenden, *Nature*, 1972, and *Vistas in Astronomy*, in press, *Celestial Mechanics*, in press, and several other journals, in press, A. H. Wilson of the University of Chicago—by the way, Michael Ovenden is a fellow of the Royal Astronomical Society—A. H. Wilson—

MULHOLLAND (?):

And a friend of mine, I might add:

BASS:

— a dynamical astronomer, of — Also, are you familiar with the works —

MULHOLLAND [?]:

We should say yes and just sit down.

BASS:

— of the three leading celestial mechanics in the world from the point of view of rigorous mathematical proof, which exceeds that even of physical experiments —

MULHOLLAND:

Would you like to give your opinion as to who those three are before I say yes?

BASS:

[Bass has continued to speak, but was drowned out by Mulholland's question.] . . . and I refer, of course, to V. I. Arnol'd of Moscow, [J. K.] Moser of New York University, and Carl Ludwig Siegel of Göttingen, because these four gentlemen—I can give you the page references of their journal articles—have published explicit statements which show that almost everything you said was superficial, and they diametrically refute many of your leading points. [applause]

VOICE:

Well.

KING:

. . . [inaudible] . . . brief answer.

VOICE:

. . . [inaudible] . . . controversy . . .

VOICE:

That's not a question.

KING:

This was a speech, not a question.

MULHOLLAND:

As I passed up here, somebody said that's a controversy, not a question. I will answer very briefly. Yes, I am familiar with most of those works, and no, I do not agree with you that they confute anything that I said. [applause]

KING:

Thank you for your [brevity?].

SAGAN:

Also, they represent an argument from authority. There was not a single substantive point in your question. It was all, "Have you read Mr. X, Y, Z, or Q?"

KING:

One more question from the audience.

QUESTIONER:

I have a very brief question for Dr. Sagan. Following the recent Pioneer X encounter with Jupiter, there was a wire services story in which there was a quotation attributed to you that there were hydrocarbons in the atmosphere of Jupiter that were precipitating "like manna in the wilderness." I wonder if— [laughter]

SAGAN:

This is another idea due to Rupert Wildt in 1940, about ten years before 1950. [laughter] Rupert Wildt, in fact, turns out to be the *eminence grise* of this subject matter, having thought of, but for the correct reasons, all of Velikovsky's principal arguments which are used to justify his thesis *post hoc*, almost all.

And it was Wildt who had correctly identified methane in the atmosphere of Jupiter, and Saturn, in the 1930's, and he proposed that other simple hydrocarbons were to be found there, which indeed turns out to be correct. In fact, just in the last few months, acetylene and ethane have been found in the atmosphere of Jupiter, in small quantities.

We have done laboratory experiments in which we duplicate the methane, ammonia, hydrogen, and probably water, which exist in the atmosphere of Jupiter, supply energy sources to it, and find that a large range of organic compounds are produced, including the precursors of amino acids. For this reason we think that Jupiter is of substantial interest for pre-biological organic chemistry, and I do think that organic matter is dropping from the skies of Jupiter like manna from heaven. It's on Earth where I have difficulty understanding manna from heaven. Jupiter makes perfect sense.

KING:

The two previous talks were directed largely to Dr. Velikovsky, and I think he should be the next one to comment on them.

VELIKOVSKY:

I think that Professor King made the right decision, and I thank Professor Michelson for agreeing to speak in the evening.

Actually, Professor Michelson was selected by the organizers of this Symposium to discuss the subject of celestial mechanics, requiring advanced knowledge in mathematics and physics. He is an international authority in his field, and I am pleased to say that I will yield to him to answer many things that I would have answered to Professor Mulholland.

However, one thing I wish to say. All what Professor Mulholland mentioned here was based again on the assumption that nothing had happened and could not have happened in the past, and therefore it must have begun as it goes. But this is not a law; this is a principle—

MULHOLLAND [overlapping]:

I'm sorry, that's not true. That was no assumption. Those were observations.

VELIKOVSKY:

Yes.

MULHOLLAND:

Data, not assumptions.

VELIKOVSKY:

One of my data was that electromagnetic phenomena do participate, to whatever extent, in the celestial mechanics, and under catastrophic circumstances to much greater effect than, of course, a normal condition.

The discovery, for example, of Professor Danjon, Director of Paris Observatory, that made sensation when he announced it, in the summer of 1960, at Helsinki, about the change in the rotation of the Earth, if only in milliseconds, following a flare, a regular flare on the Sun, was unbelievably by those who attended the International Geophysical Union session. But then it was confirmed, in Helsinki again.

So these electromagnetic phenomena were entirely not included [that is, calculated in, included in the calculations], but when now the celestial mechanics is presented in textbooks, the authors, like Clemence and others who are great authority in the field, have excused themselves, saying they knowingly omit phenomena that certainly do exist, but they do not calculate [that is, calculate them in]. They still go by pre-Faraday astronomy. Of course, Newton was not to blame. Even I will read a sentence from Newton, because he was farsighted. He saw the phenomena which I—well—had long battle for with astronomical society. I was considered outcast exactly for, more than for anything else, for claiming that, besides inertia and gravitation, also electromagnetic forces and fields do participate, and on one of my letters, the late Einstein wrote, "Yes," this was the main cause of the great agitation against you.

Now, as to Professor Sagan— [laughter, applause]

VOICE:

That's good. Right there.

VELIKOVSKY:

—let me quote one single sentence from his new book. In his new book he says, "Jokes are a way of dealing with anxiety." [laughter, applause] And this is exactly what I said in my lecture. I wrote it before I read his book. I bought it only here, in San Francisco.

Well, you hear jokes. It is easy to put in a book something what is not there, and then make it a joke. I believe this is an action of a person who defend a position that is undefendable. [applause]

I would have not spoken on this subject now, but I heard that Professor Sagan will not attend the evening session, when we would have more time to discuss the matter, and since he is not prepared, or made advance—well—agreement on being somewhere else, so this Symposium already being prepared for more than half a year, so how advance could it have been? I would like to confront him in the evening, and I have with what to confront.

Nevertheless, to put into my book the story about Moses opening the sea, or Joshua asking the Sun to stop still, and then at the nick of a moment here coming the comet and do what Joshua or Moses asked, where I clearly said that these things are entirely fabrication of folklore, that the story as it is need to be searched from one place to another place.

And so Professor Sagan claimed that he is not versed in mythology or folklore, but he went into that area, and had some ideas. But I already discussed these ideas, I think to satisfaction of those who deals with question of mythology, because mythology has a reason in fact, a basis in fact. It was not just carried from one population, from one island to another. The story were told very differently, but the theme is always the same.

Now, again, to put into my book story that frogs were falling from the sky—not in this lecture here, but according to a tape recording of a lecture before tuition-fee-paying students at Cornell—that frogs were falling from the sky, and this [was] what Velikovsky said, and I said exactly the opposite, that frogs were the brood of the Earth, because the quotes in the Bible is exactly to this.

He said also that mice were falling from the sky. Now, well, mice? Well—You need to know the Ten Plagues. There was no Mice Plague among the Ten Plagues. And certainly warm-blooded animals did not fall from there.

I even did not claim that flies came with Venus. I put in that way: it could be; it is anybody[']s guess. So the idea of contamination of the Earth goes back to the beginning of the century, and you can find it in work of a Swedish geo-physicist of that age.

Now, again, as to the life on Venus, and the Venus clouds—

By the way, the story of the frogs falling from the sky was also a matter of discussion on the third of December when Jupiter probe, Pioneer X, passed by, and [there] was a press conference, and there was a confrontation between [Sagan and] Professor James Warwick, whom I never met, who demanded a fair treatment to me, claiming for me the advance claim of Jupiter noises. Now, well, this is one of the cases where Velikovsky made generalized statements. Jupiter noises, so clear as this, and who else said it?

So again Professor Sagan said, what is Jupiter noises? Frogs were falling from Jupiter clouds. But in the book, just I quote it now, 1974, he claims that—well, some few things. One of the things is that on Mars there may be animals today, of the size of polar bears, they sleep thousand-year hibernation sleep, and they get their food by, well, eating or taking stones into their mouth and extracting water from the stones. Well, somebody who comes with those ideas should be very careful to criticize. [laughter] Well—Well—documented, from many civilization, idea of contamination of the Earth by some larvas coming with cometary tails, which I did not subscribe [to], but presented for discussion.

Now, again, let me ask about the correctness of prediction. In that new book I read that Professor Sagan claimed for himself such clear predictions in 1963 that Venus is very hot, and that Venus had many atmospheric pressures, and he claimed that he said it already in 1962.

Well, possibly he said it in 1962, but I have with me an article in *Science* from 1961, where he claimed that if the atmosphere is 600, and it was already started by Professor Meyer in 1956. As soon as Jupiter noises were found, all planets were subjected to tests. Venus was found producing certain radiation, and this was not of the same length as from Jupiter, so it was not of the same kind. It was thermal signals. Now, these thermal signals would be like 600 degrees. It was not believed that 600 degree could be right. Sagan believed that it could be right, 600 degrees, but he said if the surface temperature is 600 degree, Venus would then be approximately four atmospheric pressures, and this is *Science*, and this is twenty-fourth of March, '61.

Now he claims in his new book, that in '62 he was such a great prophet that he claimed already fifty pressures. Well, from one year to another —

VOICE:

He's not perfect.

VELIKOVSKY:

No.

VOICE:

He's not perfect.

VELIKOVSKY:

He is not perfect. [applause]

Now he is opposing hydrocarbons on Venus. But I will quote some authorities concerning hydrocarbons on Venus. For example, here is an authority who says that about possible existence of some hydrocarbons in the lower atmosphere. Will you agree with this statement, Professor Sagan?

SAGAN:

Well, what was the statement? That there's a possibility —

VELIKOVSKY:

Possible existence of some hydrocarbons —

SAGAN:

How much?

VELIKOVSKY:

—in lower atmosphere.

SAGAN:

How much?

VELIKOVSKY:

Not question of how much.

SAGAN:

Yes, it is a question of how much. In fact, that's the *theme* which cause the most difficulty in this area. Remember [?]-

VELIKOVSKY:

There are at the end of *Worlds in Collision* two section, dealing with physical condition on Venus. In one I dealt with the constituency of the clouds and atmosphere, and I explained where, if there are hydrocarbons, to look for them; I said also how hydrocarbons could have been created from methane and ammonia. And this was confirmed ten years later by experiments, exactly this how it was done.

I claimed also later, in 1951, how hydrocarbons could be changed into carbohydrates, and this was in debate with Stewart that I mentioned before, in June, '51, of *Harper's*.

Now, again, second section dealt with thermal balance of Venus. And there I said if oxygen is still there, there must be hydrocarbon or petroleum fires. Now, you understand all right that if there is heat, as it is, and if there is oxygen, and if there are fires, hydrocarbon would not last. Actually, if it is still there, it would only be a time clock to find out how long the process is going on. The other way of transforming would be in[to] carbohydrates. But nevertheless, little or much, are hydrocarbons there? It is not the question of quantity; it is question of quality.

Do you agree with this statement, that I claim, that hydrocarbons could be there?

SAGAN:

Do I answer?

VELIKOVSKY:

Yes.

KING:

Would you please answer into the micro- —

VELIKOVSKY:

I would ask first this question, because immediately I will continue.

SAGAN:

You made a number of statements. Let me try to answer some of them.

VELIKOVSKY:

No, maybe I would continue, then you answer the others, but this I would ask.

SAGAN:

We are running out of time, and I am running out of remembering what your comments were. So how about letting me make some responses, and —

VELIKOVSKY:

Well, I wish to continue on this one question. [laughter]

SAGAN:

Well, why don't you let me answer, and then you can continue.

KING:

Please let him answer.

VELIKOVSKY:

No, because I am in the middle of an argument about hydrocarbons. [laughter]

SAGAN:

You're not in the middle of an argument if you don't let me answer.

VOICE [to Sagan]:

Say yes or no and sit down.

VELIKOVSKY [to Sagan]:

Well, if you wish.

SAGAN:

I'll be glad to respond. No, you see, it is not just a yes or no question. Let me say why.

VOICE:

Why not?

SAGAN:

I'll explain.

VOICE:

Then qualify it first, sir.

SAGAN:

Many of the difficulties with the Velikovskian approach is the absence of quantitative thinking. So it's not enough to say, for example, that I said there were going to be large magnetic effects, and [it] turns out that Jupiter has a magnetic field of six gauss or whatever. There is bound to be some residual magnetism everywhere. There is bound to be, just as in the Earth's oxidizing atmosphere there are today hydrocarbons. Methane is one part per million of the Earth's atmosphere. That has nothing to do with manna. It has nothing to do with any of this. If you look closely enough, you are going to find a large number of things.

Let me try to respond to a few of the remarks Dr. Velikovsky made, and then I'll be glad to hear the rest of his discussion and, if I can, try to respond to that.

In his response so far, there has been very little substantive commentary on my remarks, but, on the other hand, he hasn't heard many of them before now, so I don't object to that. [Actually, Velikovsky had heard almost all of them before.]

The idea of oxygen burning fires on Venus is very bizarre, because Venus would come from Jupiter. Jupiter has an excess of hydrogen. There can be no oxygen on Jupiter. It would all have been reacted with hydrogen to form water. Therefore, there should be no oxygen on Venus, and, indeed, there is none, as has been clearly shown by ground-based spectroscopic observations.

Dr. Velikovsky has criticized me for having changed my mind. I do not consider that to be a serious flaw. I think that it is precisely the ability to change one's mind which is the method by which science advances, and the unwillingness to change one's mind, the idea [an idea that Velikovsky has never presented!] that texts are canonical and need no revision in the light of twenty-five years of subsequent study, *that* I find more strange.

I do not consider this to be a debate between my theories and Dr. Velikovsky's theories. As I understood the function of this Symposium, it is merely to discuss Dr. Velikovsky's views in "Worlds in Collision."

To respond specifically to the remark he made, between 1961 and 1962 a significant change in our knowledge of Venus occurred. It was the question of whether the atmosphere was mostly nitrogen or mostly carbon dioxide. Nitrogen had been deduced there by default. We then realized that the spectroscopic reductions were in error. The atmosphere was therefore mostly carbon dioxide. Therefore, the specific heat at constant pressure was different. Therefore, the adiabatic temperature gradient was different, and, therefore, to get down to 650 or 750 Kelvin you had to go much further down the adiabatic gradient, and therefore you got to much higher pressures. And it is precisely because we learned something new that we changed our views, and by 1962 the views that several of us had proposed turn out to be correct.

Now, on the question of frogs, mice, toads [no one mentioned toads before, not even Sagan], flies, and other vermin from the skies, it is quite true that Velikovsky does not say that mice fell, nor, in this lecture, have I. [The words "in this lecture" were spoken with such rapidity as to be unnoticed by most of those in the audience.] It is almost true that Velikovsky says that frogs have not fallen. I say "almost true," because he quotes an Iranian text, in apparent approval, which Iranian text seems to show frogs from the sky. [The Iranian text and other such texts are discussed in *Worlds in Collision*, pages 183-187, which Sagan is totally garbling.] But he does not say that. He says "probably," or words to that effect. [Actually, Velikovsky's words were "must be," which are hardly to the same effect as Sagan's "probably."] It was the heat produced by this cometary interaction which caused indigenous terrestrial frogs to proliferate.

That's fine, but notice that Velikovsky is now asking to have it both ways. Some of the plagues come from space, and others do not. Now, what is the decision as to which ones to accept and which ones not to accept based upon? A consistent view would be to say either "I have believed the accounts in *Exodus*" or "I don't." But to say "I will choose to accept some and not others" is very strange. [These questions are ones that are answered in Velikovsky's writings. Even if Sagan has never consulted the written answers, he should be able to recall how Velikovsky answered these questions no more than fifteen minutes earlier in the discussion. Velikovsky repeated once again that "mythology has a reason in fact, a basis in fact." Velikovsky accepts those elements of the mythological stories that have a plausible physical explanation and that are independently reported by different peoples. The stories "are told very differently, but the theme is always the same." Local embellishments that have no plausible physical explanation "are entirely fabrication of folklore," and each "story as it is need[s] to be searched from one place to another place," if the common theme is to be found. See also Velikovsky's "Afterword," where he explains that he rejects any local embellishment that "does not have a plausible physical basis, is not testified to by other people, and is therefore to be regarded as an inaccurate elaboration by one people upon what actually transpired." Sagan's continuing need to describe his own garbled version of Velikovsky as "very strange" is itself very strange.]

Let me give one specific example.

KING:

With all due respect, –

SAGAN [overlapping]:

OK. One second.

KING:

–I think you're introducing new material rather than responding.

SAGAN:

No, I am trying to respond to the question about frogs and mice. [applause]

Exodus states that manna fell every day for forty years, with the exception of the sabbath. It did not fall on Saturdays. Instead a double ration fell every Friday. [laughter] It didn't actually say fell. It said appeared. But, using the Velikovskian verb, let's say fell. [The verb is not Velikovskian, but Biblical: Numbers 11:9 says, "the manna fell."]

Now this seems to me to pose serious problems with Velikovsky's hypothesis. How, 10 10 kilometers net path away from Earth, did the comet know to hold back on Saturdays, but to give a double ration on Fridays? [Here, again, Sagan displays no understanding of what Velikovsky's views are. The 10 10

kilometers is the approximate distance that Venus might have traveled during forty years. This, of course, has nothing to do with Velikovsky's theory, which is that various materials from Venus were transferred to Earth's atmosphere at the time of the Exodus. These materials were modified in Earth's atmosphere, and over a period of time precipitated *out of Earth's atmosphere*. Sagan's idea of a daily shipment from Venus to Earth, transported over the distance that Venus had covered since the Exodus (which was many times greater than the distance between Earth and Venus at any given moment), is entirely his own invention, and proves nothing, except that he is quite ignorant about the theory that he is attacking.]

So this is something that, of course, we see is absurd, so we do not invoke it. But why not? Why this preferential use of the fraction of *Exodus* which seems to match some preconceptions, and the avoidance of other things in *Exodus*?

If I had to choose—and we certainly don't have to choose, fortunately—but if we had to choose, is not the evidence almost as good for the God of Moses as for the comet of Velikovsky? [This rhetorical question is essentially the last sentence of Sagan's paper, which he had omitted when he read the paper.]

KING:

The time is almost ten after one. I will hope that Dr. Velikovsky can give his present answers in five minutes, and then postpone everything else until the evening.

... [inaudible] ...

VELIKOVSKY:

On one hand I am accused of having gone into too many fields. On the other hand I am accused of having not gone far enough, and not calculated everything to last detail. I left something for Sagan to do. [laughter]

As to the question of the energy required for explosion from Jupiter, I discussed this subject in a special issue of *Yale Scientific Magazine*, dedicated completely to the question of my thesis of Venus being a young planet. It was April, 1967, and there, with Professor Motz as my opponent, Lloyd Motz of Columbia University, I discussed and explained this subject.

It was not a kind of volcanic explosion. It was a fission of the planet being disturbed in a way how also British cosmologist, Lyttleton, describes in *Man's View of the Universe*—it's a popular work—1961, page 36, but also a year before in the *Monthly Notices* of the Royal Astronomical Society, in England, namely, how Jupiter had to come out of embarrassing situation by splitting in two unequal parts.

This of course Lyttleton put much farther in time, but the argument is even better if you know my arguments in the two volumes that precede *Earth in Upheaval* [meaning *Worlds in Collision*], describing the events concerning flood, universal flood, and other catastrophic events of the time. [Velikovsky is here referring to *Saturn and the Flood* and to *Jupiter of the Thunderbolt*, the two volumes that describe the earlier catastrophes, those that preceded "the last two acts of the cosmic drama" that are described in *Worlds in Collision*.]

As to the figures of mathematician and physicist, how they throw them! One lesson I had to give. Professor Straka, of Boston University, presented his piece, with calculation, with figures, to *Pensée*. It was printed in the second issue of *Pensée* dealing with Velikovsky. There are altogether ten, six already out, seventh go to print soon, [it] will have all these debates in it probably.

Now, in that occasion I took to give lesson to a mathematician. Read it. Read the figures, how they are put together, how [they] are brought before the lay public, and then read my answer.

I received a letter from Arthur Clarke in Ceylon. He says he would like to be present in the class of Straka, when students would bring that article into the class.

I don't claim to be a mathematician, and I leave this work to others, and I am happy that Professor Michelson, who started entirely uncommitted, not selected by me – not even asked I was whether I agree to selection of Professor Michelson. He will present to you in this evening – and I strongly advise you to be present – with complete answer to Professor Mulholland. So he is not a philologist, not an historian. He will not go into this field. But he will come with two great calculations that will be something in science to remember, of his own.

Now, as to question of manna and Saturday, you see another joke. Of course I didn't say in my book, as if in my book is spoken about manna falling six days in the week and not on Saturday. Of course I did not say this. Of course I did not say that the Israelites were much more fortunate than the Egyptians. At the Sea of Passage many of them perished. In the Plague of Darkness, despite the biblical statement, other rabbinical statements say that forty-nine of fifty Israelites perished during the Plague of Darkness.

So I stressed these points, this disagreement with the Bible. I am not a fundamentalist at all, and I oppose fundamentalism. So this bringing story of manna as if it is my story is, of course, not serving the purpose of scientific debate.

Now, as to the oxygen on Venus, I think Professor Sagan is just wrong. The Russian probes found small quantities of oxygen below the clouds. Not did not find. They found it. And they found that it is a hot, oxidizing atmosphere, and so it is referred to numerous time in the recent literature in America, too. So how not to know this, if Sagan serves also as editor of a magazine on planetary sciences?

Now, as to prediction in general, on this I stand: Nobody yet brought a wrong prediction of mine. Some thing is not yet completely confirmed.

The question of clouds on Venus, what it consists, is a question still of debate. But I asked something [of] Professor Sagan. He interrupted me, and he did not go into that question. And the question was whether he agrees with the idea that hydrocarbons are in lower atmosphere of Venus. He did not answer, but this was quotation from *his* article. [laughter]

Now, he also did not answer other questions, but let us say that he pretends that he did not claim me writing in my book about frogs falling from the sky, and mice, too. Now he says he didn't say about mice, but this is on the tape. The tape exists. [Sagan made these and other outrageous statements on March 28, 1973, in a widely publicized lecture on "Venus and Velikovsky."]

And about frogs, we have here, in *Pensée* number VI, also from a tape, discussion between Professor Warwick and Sagan on third of December, and Sagan say here, clearly: "Let me. Velikovsky explicitly predicts the presence of frogs and flies in the clouds of Jupiter," and here you heard that he says, no, he didn't say some things like this. But he said it only on third of December. So –

KING:

May I ask you, since it's a quarter after one, to stop?

VELIKOVSKY:

Yes, I am finishing with this. On this point I stop. I think that Professor Sagan, claiming water on the clouds, and there are none; claiming lower temperature, pressure, and it happened to be very high (of course subsequently he changed his view); and claiming now organic materials, and even life, in the clouds of Venus, and we heard here something contradictory to this, and this is another article of his. So if somebody has six days in the week for six opinions, he maybe sometimes be right, too. But with me, it happened so, that my claim were made long in advance of the findings.

And thank you. [applause]

KING:

May I thank Professor Michelson again for graciously allowing his talk to be postponed till the evening.

[aside] Yes.

I would like to make one . . . [inaudible]

. . . [inaudible]

QUESTIONER:

I would like to request that Professor Sagan be asked to continue his point of view.

VOICES:

. . . [inaudible]

QUESTIONER:

I present it to the podium. If one man made the sacrifice of allowing him to continue, I think he should make the sacrifice to attempt to stay here.

KING:

When I was describing the genesis of this Symposium, I mentioned that A.A.A.S. put this Symposium together out of a feeling that the work of Dr. Velikovsky was worth presenting at a public forum. What I did not mention at that time was that Professor Sagan is not only a vigorous defender of science, he is also a vigorous defender of scientific freedom, and the suggestion that we hold this Symposium came directly from Professor Sagan. [This is false; the suggestion that A.A.A.S. should hold such a Symposium was first put forward by Walter Orr Roberts. Roberts' idea was later "supported" by Sagan and others.]

The meeting is now adjourned.

* * * *

THE EVENING SESSION

GOLDSMITH:

How about now? Is this better?

My name's Donald Goldsmith. I'll be the chairman of tonight's session. We will have until ten o'clock, at which time, by the rules of the A.A.A.S. and the hotel, all the other things that have been worked out, to get the room ready for tomorrow, we'll have used up the time allotted to us, all too short— [filled up] the morning. We'll have a full discussion of all the points people would like to discuss. So that I'd like to urge you to be short in your answers, short in your questions. It would be nice if there were not enough people who had a lot to say, so that we could have a full, complete discussion. But I'm afraid that that will not be the case, and it'll be of extreme importance to use the time.

We'll start tonight with a talk by Professor Michelson, which he so kindly postponed until this evening in order to allow for the extra time that was used up during the morning session. And after he speaks, we'll go into a panel format, with the members of the morning discussion here, who will answer questions, I hope never speaking more than one at a time, or perhaps two or three at a time at a maximum. We have a microphone in the audience for those who wish to ask questions, make it easier, so that people won't have to get up and down here. And with luck we can have a reasonable exchange of views. With bad luck we'll simply run out of time and all go home a little bit disgruntled. So we'll first have a talk by Professor Irving Michelson of the Illinois Institute of Technology, who will speak to us on the topic of "Mechanics Bears Witness." Professor Michelson.

MICHELSON:

[Michelson's paper, entitled "Mechanics Bears Witness," was presented at this point.]

That's all I have. [applause]

GOLDSMITH:

Thank you, Professor Michelson.

Before we go to the panel discussions, we will have a brief discussion period concerning the talk which Professor Michelson has just given. I will take questions from the audience for a brief while. Let me first call on—Professor Mulholland?

MULHOLLAND:

I would like to point out, with respect to this last calculation here, which produced such remarkable results, in a correspondence between the energy required to flip the Earth over and the energy expended in a solar flare of great magnitude [Michelson had spoken of a geomagnetic storm, not a solar flare!], falls a little short when one realizes that the Earth, as seen from the Sun, represents rather less than ten to the minus eighth power of the total space into which the energy of that flare is expelled. Therefore, the 10^{23} ergs results in less than 10^{15} ergs at the Earth. Thank you.

HUBER:

How do you conserve angular momentum?

MICHELSON [regarding Mulholland]:

I'll let *that* go. I'll let that go. [In *Science*, March 15, 1974, page 1062, Robert Gillette implied that Michelson dismissed Mulholland's question because he had no answer. Michelson thereupon wrote a letter to *Science*, calling attention to the fact that Gillette had "omitted mention of the irrelevance of the outburst from the floor, to which I responded 'I'll let that go.' Those who heard my presentation as symposium panelist were aware that it deserved no other reply; your readers are entitled to know a bit more, having been given what Gillette told them." Michelson then explained that he had pointed out "that the energy required to turn the Earth's magnetic dipole through 180 degrees (interchanging positions of north and south poles) happened to be equal to that of a moderately strong *geomagnetic storm*." Mulholland's remarks about the tiny fraction of a *solar flare* that reaches Earth were thus completely beside the point. The final two paragraphs of Michelson's letter were as follows:

"In the discussion period someone who wanted to voice an 'objection' talked about the energy of a solar flare and the spatial attenuation at Earth's distance from the Sun - declaring that one of my numbers was therefore very wrong. The relevance of solar flare energy to the geomagnetic storm energy confined to the geomagnetic cavity surrounding the Earth is about as small as the Sun's distance from the Earth is large. At most, we can say that the sudden influx of charged particles from the Sun triggers geomagnetic storms - their energy is to the energy of the storm as the detonator energy is to the energy released by the bomb it activates.

"There had already been all too much acrimony, back-biting, and anger expressed in the Symposium - and too many long-winded replies to comments from the floor. For me to launch into a lecture explaining the difference between the Sun's solar flare and the Earth's geomagnetic storms to one who either knew it already or would never know it, while all others present wanted to get on to more meaningful discussion of real questions raised by my presentation, seemed inappropriate. I hoped that most others present knew this was my meaning in refusing to enter into heated or lengthy dialogue with an individual whose zealous opposition to Velikovsky outran his reason."

After some delay, *Science* decided not to publish Michelson's letter. But when they received a petition from thirty-five concerned scientists urging them either to refute Michelson's complaint or to publish his letter, they suddenly reversed themselves and published the letter.]

HUBER:

I wonder, how do you conserve angular momentum?

VOICES:

The question? Repeat the question.

GOLDSMITH:

The question is, how do you preserve angular momentum?

MICHELSON:

Angular momentum is certainly modified in this process. I don't know how long it would take or what the couple would be that would be acting. I don't think we have any basis to try to ask so much about the detailed process that that would require. I speak only of energy.

QUESTIONER:

I have something I'd like to say, and that is right now on this planet we're trying to have an energy crisis, [laughter] and we're trying to implement solar energy systems by which we can use to augment our energy crisis. Well, in part of my research I've come across some very interesting things in . . . [inaudible] . . . philosophy [?], which deal with the Sun, and some of the things described in talking about here. Well, actually what happens when we use solar energy is that the mass of the planet is increased simply by trapping the energy from the Sun here on this planet. It can't get back to the Sun . . . [inaudible] . . . That's one way by which the mass of the planet is increased, to answer your question. And the other way is that we are taking energy from the Sun and adding it as a charge to the Earth, increasing the charge of the Earth, drawing the Earth into the Sun. Now, three times, to my knowledge, in ancient Aztec civilization, these power networks were set up and three times they had a dangerous catastrophe. Possibly we're going to have to find some way by which the energy can be returned to the central sun of the galaxy, or the energy can be returned back to the Sun, or else, again, if man wants to worship a Sun god – sacrifice all men –

QUESTIONER:

Question.

GOLDSMITH:

Would the . . . [inaudible] . . . microphone? [applause] If we can only solve the energy crisis by returning the energy to the Sun that is given off, I'm afraid we're not good for much longer here. [laughter] So we'll have to take some more questions. In the back there.

QUESTIONER:

Yes, the Sun's surface [happens to be enough?] hotter than the filament of a vacuum tube, and the vacuum surrounding the Sun is much better than the vacuum in a vacuum tube. How do you propose to hang onto all these electrons, or do you propose a source to resupply . . . [as the Sun boils them off?] . . . [remainder inaudible] . . .

MICHELSON:

Dr. H. C. Dudley, professor of radiological physics at the University of Illinois, proposes [a] mechanism by which this charge is continually sustained on the Sun, involving [a] deuteron passing to a proton and neutron, another proton and electron neutrino going into a neutron and positron reaction, which he says occurs at 10^8 degrees absolute, followed by a decay of a neutron into a positron [meaning "proton"],

electron, and electron neutrino, [a] process involving a half life of twelve minutes. The greater momentum of protons resulting favors their diffusion into the solar wind. A total negative charge on the star would then be a function of rates of neutron production within the star's interior and the differential rates of diffusion of protons and electrons from the chromosphere into the solar wind. This is the explanation I have received. This is not my field. And I don't want to make any kind of judgment on this.

QUESTIONER:

. . . [inaudible] . . .

GOLDSMITH:

I think perhaps — one question at a time — far in the back there.

QUESTIONER:

Yes, have you considered that what you are proposing here is a particular model, you might say, of the electric structure of the solar system in that the Sun has a charge and each planet has, presuming they have a charge on a hypothetical basis — have you considered other possible structures such as a charge separation, or equal and opposite space charges, either within the Sun or perhaps extending farther out of the Sun, as possibly accounting for some of the phenomena referred to by Velikovsky?

MICHELSON:

I believe I am beginning to appreciate the importance of some of those charge separation effects, but I have not [studied?] them. No, I'm sorry.

GOLDSMITH:

Let us have one more question and then go into the panel phase of this. We'll take — the man here.

FOX:

It's inappropriate for me to make a speech here, but I think I have something that is germane to this whole Symposium, particularly Dr. Michelson's talk, which I feel personally to be the most restrained —

VOICE:

. . . the mike.

VOICE:

Can't hear.

GOLDSMITH:

I think the mike back there is working.

FOX:

OK. In brief, I think I have something that is germane to a Velikovsky Symposium—and in particular to Dr. Michelson's remarks regarding the existence of net charges on the bodies of the solar system—in some research of my own which has not yet been published, but which however has been submitted for publication. A revised paper is about to be mailed as soon as I return home, which is Vancouver, Canada. This research of mine is into the origin of the solar cycle. And using two methods of time series analysis, Fourier analysis and the method of superposition of epochs developed by Cree, I've found that in the sunspot record there are planetary periods. Now the tide-raising power of a planet at the solar surface is simply given by the formula M over r^3 . And it can be readily calculated for any one of the planets in the solar system. Taking the Earth as 1, we find that the tide at the solar surface due to Mars—this is the gravitational tide—has a value of .03, in other words, something less than one-thirtieth of the Earth's gravitational tide at the Sun's surface. Now the relative amplitudes of the planetary periods which I have found in the sunspot record indicate that the influence of the planets on the spottedness, if you like, of the Sun—the activity of the Sun—is probably not solely gravitational in origin. The amplitude of the Martian cycle which I have found—this is what I call the Martian fundamental—is twenty-five times what it should be on the basis of a purely gravitational explanation. This is all I have to say. I would be happy to give Dr. Michelson and Dr. Velikovsky a copy of the paper which I have submitted for publication. Thank you. [applause]

GOLDSMITH [to Bass]:

Well, OK.

BASS:

Yes, I have a comment on this next-to-last question. Is this original with you, Dr. Michelson, or has this been published previously? The kinetic energy of rotation being equal to one-half the hypothetical electrical potential energy?

MICHELSON:

No, this has not been published, no.

BASS:

This is your work?

MICHELSON:

Yes.

BASS:

Uh-huh. All right, speaking as a celestial mechanic, purely gravitational, I have always thought that the hypothetical electro-magnetic forces in the solar system were of tertiary importance, but I feel that you have brilliantly demonstrated the opposite and made me change my mind. And I'm trying to understand the formula, and I have a question here. I think I can understand it on the basis of gravitational celestial mechanics. There's a theorem known as the virial relationship which says that in a system of moving point particles the mean potential energy is exactly twice the kinetic energy, and your relationship here has exactly that form, and I had wondered if you had thought of it from that point of view.

MICHELSON:

Well, virial theorem relationships that give you a value—a fraction—ratio—two are those that appear when there is an inverse first power energy. The cases where we have equi-distribution, equipartition, are those in which, most commonly, both kinetic energy and potential energy are proportional to the square of displacement. And, yes, it is very much suggested that a virtual theorem demonstration would lead to this result, when we would know what kind of interaction there would be between the mechanical and the electrical fields of force here.

GOLDSMITH:

Dr. Michelson has asked me to ask whether Dr. —

MICHELSON:

Dr. Driscoll.

GOLDSMITH:

—Dr. Driscoll whom you mentioned has any comment to make on what was said about his work, if he is here?

If not, I suggest we pass into the panel format.

I'd like Drs. Velikovsky, Mulholland, Storer, and Huber to join us on the platform. I will add a little filler commentary while they're doing so.

I'd like to point out that this Symposium has been sponsored not only by the Astronomy Section of the A.A.A.S. but also by the Section on History and Philosophy of Science. It's for that reason that the co-organizers of the Symposium are both Professor King, the chairman of the Astronomy Section, and Professor Gingerich, chairman of the Section on History and Philosophy of Science, as well as myself, who is chairman of nothing in particular, except that I shall be here. [laughter]

[At this point there was a pause of several minutes while the panelists moved from the audience to the platform.]

GOLDSMITH:

. . . [inaudible] . . . more or less at the appointed hour.

I'm going to call for questions.

[gavelling] Here's how we shall do it.

Those of us up here on the panel are more than enough to fill up an hour and a half. In this case the audience can sit mute and admiring [laughter] and let the personalities clash.

But because of our slight commitment to involving the audience also, what I will call for is, roughly the order of the speeches this morning, questions from the audience, mercifully short, and we shall try to have equally short replies from the members of the panel who would be most affected by answering the question. For example, the first talk this morning was by Dr. Storer, "The Sociological Context of the Velikovsky Issue." I will entertain a question dealing with that, and presumably ask Dr. Storer or Dr. Velikovsky or someone else to reply, but if we get stuck on the first one we'll never go on to the others, so I repeat, brevity is what keeps us all from falling asleep from time to time. There must be a way so that we can express ourselves fairly well without using up the precious hour and a half that we have. The floor mike is working, for those who care to step to it.

If there is a question, then, related to Dr. Storer's discussion, I'll be happy—There's one from Dr. King.

KING:

May I ask very briefly what Dr. Storer thought of the Symposium? [laughter]

STORER:

Well, I was not certain how we're going to run this, but it did occur to me that if anybody had any question to ask me it would be that. [laughter] I think my principal conclusion is that scientific controversies will never be resolved in public debate. [laughter] The desire to persuade an audience leads one into rhetoric, into selective attention to what the other fellow said. This is a show almost. I don't mean to denigrate it, but really nobody's gonna say, "Hey, I was wrong, you're right," and go home, [and] be satisfied. I don't know that I have much more to say than that, right now. It's been an entertaining experience. [laughter]

GOLDSMITH:

There's a question, far in the back.

QUESTIONER:

I'd ask this of Dr. Mulholland, and Dr. Sagan—

GOLDSMITH:

A little louder, please.

QUESTIONER:

... [inaudible] ...

GOLDSMITH:

You can use the microphone for greater clarity.

QUESTIONER:

. . . [inaudible] . . . Dr. Sagan had changed his mind and was here, he might –

GOLDSMITH:

Well, wait a second. Are you saying Dr. Mulholland and Dr. Sagan? Because what I'm saying is, I had hoped that the questions would be directed at Dr. Storer, at least for the focus of his talk.

QUESTIONER:

Oh, I'm sorry.

GOLDSMITH:

Right.

QUESTIONER:

Will you take them in two parts?

GOLDSMITH:

Right. Sorry.

VOICE:

Here's one.

VOICE:

There's one, Doctor – yes, Dr. Rose.

ROSE:

With regard to Professor Storer and his talk, I would like to mention that Professor Horace Kallen, a distinguished American philosopher and former Dean of the Graduate Faculty at the New School for Social Research, and an editor of some of William James's unpublished manuscript, chosen by James, was in close touch both with Velikovsky and with Harlow Shapley in the years before 1950 and just after 1950. He wrote both as a participant in those events and as a critic of those events. On this February 16th Professor Kallen suddenly died, and on that very day he wrote a critique of Professor Storer's paper. I would like to read two sentences from that critique. Quote. "Here is a hurried scrawl done in the sleepless hours last night re Storer.

. . . Storer seems to me to *uphold* the established convention of his discipline, not to analyze the actual course of events which are the real." Unquote. [applause] I'd like to ask for any reaction. [laughter]

STORER:

Since that is not a question, I don't know that an answer is called for.

ROSE:

Well, my question is, do you have any comment?

STORER:

Oh. Well, the gentleman is or was free to his opinion. I don't quite see what sort of an opinion I could have about such a very vague statement.

ROSE:

Well, if I can state a little further what Kallen's position was. His position was that range and boldness—which were the terms he used in the material published in *Pensée*—are the things that constitute the essence of science. Without them there can be no progress. He thought that the work of Velikovsky, whether right or wrong, well illustrated just what scientific activity should be. And it seems to me that the adherence to norms, both by the scientists and by the sociologists of science, is precisely what Kallen was opposing, and he feels that your work adheres to that same norm.

STORER:

All right. You seem to be attributing a prescriptive tone to my discussion; there were some moral "shoulds" in there. I had hoped that what I had to say was the erection of a theoretical framework—certainly not proven beyond any shadow of a doubt yet—but one which seemed to make sense of why things went as they did since 1950. My object is not to dictate the way people should do things, but to try to describe them economically and with some theoretical accuracy. So if he disagrees that either I'm empirically wrong or that my assumptions are wrong, that's certainly still open for discussion, yes.

GOLDSMITH:

Dr. Velikovsky has a short comment?

VELIKOVSKY:

Horace Kallen, the dean of American educators and probably of last decades the most prominent philosopher in this country, died on the sixteenth of this month at the age of ninety-one, in full capacities. Just two weeks or three weeks before, a book of his appeared, *Creation, Imagination, and Logic*. It is a very precious book. Its subtitle is: *Meditation to the Eleven Hours*.²¹³⁵ It was the twelve hours. It was the most respected man whom I knew in America. Since the year I came here, 1939, I did not know any man whom I would respect to greater extent than Horace Kallen.

²¹³⁵*Creativity, Imagination, Logic: Meditations for the Eleventh Hour.*

Now to the lecture of Professor Storer. I have some comments. I have to make a parallel. To the right is Professor Michelson. As I mentioned in the morning, internationally known in his specialty, and even in some other fields, with many publication to his credit, professor here and in France, very busy man. He however found it necessary to travel to Princeton to meet several other scientists who work on the same problem of the celestial mechanics, who followed this problem for years, who came for that special purpose, end of November, from Texas and from Buffalo, like Professor Rose, Professor of Philosophy, who spoke last, University of State of New York.

Professor Storer, I thought, had also reason to come from closer distance—not from Chicago, but from New York—to Princeton, where I live. And I offered it through Don, our chairman here, namely, to give him a chance to know a little more about the sociological part of the story which was never published. I was not revengeful. I kept the wound bound, but I did not show them. My archive, however, is rich, and someday it will be the possession of scientific community and especially of historians of science.

So I offered for the benefit of this audience that the paper of Professor Storer should be written on the basis of what he could at least at a short visit one hour distance learn from printed material—there are some five thousand articles collected there on *Worlds in Collision* alone—and so much unprinted materials. Well, I regret to say that Dr. Storer did not show this equal interest, assuming that he would be maybe more objective not seeing this material and also excusing himself because of various administrative duties.

Now, in this article of his that he read in the morning, he tried to be what he himself believes neutral. But neutral is not objective. You cannot be objective between evil and the victim of the evil, neutral between the behavior of science—how it was and how it started from 1950 and continued till today, almost till today, till yesterday, better let us say, is something that need to be appraised not just by applying neutrality. Neutrality is not objectivity.

I believe that, in this context, there was enough safeguard on my part not to appear as a crank when I presented my book. In the first place I let it be read by a number of people who were at that time supposed to be able to discern whether there is scientific merit in it. It was Director of Hayden Planetarium, it was Science Editor of *Herald Tribune*, it was the same Horace Kallen, who was at that time Dean of the Graduate Faculty of the New School in New York, and several others. The first three whom I mentioned, their opinions were printed on the jacket by Macmillan. So anybody who approaches the book, and reading what was stated there, was already made aware [of] the opinion of those who read the book.

Those who have not read the book attacked the book. As somebody recently wrote—actually Juergens, Associate Editor of *Pensée*—that closed mind discussed unopened book. [laughter] And it's remained unopened. And those who discussed it were quoted by next one, the next one by next one, and so a certain attitude in scientific community was created toward me and my work which was not justified by the book.

I did not present it as a text book. I asked the reader in the introduction to judge for himself whether he is reading science or science fiction. So there was no way to mislead anywhere. Any statement I presented in the book had a footnote: reference, page, and verse. In these circumstances it was clear that anybody who believes that I have misquoted or that I have quoted out of context was enabled to go to the source and to find for himself. However, he had to go to several thousand sources, because so many footnotes are in *Worlds in Collision*.

And this also caused eight or nine publishers to refuse the book, because commercial publisher did not wish the book with so many footnotes. [laughter] They one after another wrote to me that only a supported publishing house can take over such a book.

GOLDSMITH:

Dr. Velikovsky—

VELIKOVSKY:

[at first to Goldsmith, inaudible]

Now, [laughter] I believe that, under the circumstances, I would really follow what you ask me. So I have more to say. And let the question come to the next speaker of the morning.

GOLDSMITH:

We'll hear one comment from Dr. Mulholland before we go on to talk number two, and try to conserve our time.

MULHOLLAND:

I think it's necessary to say at this point that I do not really believe that anyone who has been involved in this program believes that Dr. Velikovsky is a crank, in the usual sense of that term, that he is a crackpot. I don't think we believe that, and that is not at question. Some of us believe that at least some of what he says is wrong. But that's different. I would like to ask Dr. Velikovsky, recognizing that a wrong was done twenty-five years ago, does he believe in visiting the sins of the father upon the son? [laughter, applause] We are trying to right a wrong. But that does not make the scientific questions right or wrong. [applause]

VELIKOVSKY:

Were it really the sins of the fathers? The fathers, starting with the astronomers of Harvard University, at that time, some of them no more alive, were never really exposed by me by printing their letters to my publisher, where they, like at that time Director of Harvard Observatory, denied in print that he ever wrote such letters, and I have them in my possession.

I speak not about the fathers, I speak about the sons. And the sons are *new* generation. Not the Nobel Prize winner whom I mentioned in the morning who wrote only recently, couple of years ago, to a high school girl that she should close *Worlds in Collision* and never in her lifetime open it again. [Harold Urey, March 7, 1969, UREY to LINDEMAN] But this was a man of old generation. But here, Nobel Prize winner of new generation, and he put together only recently, maybe a year ago, speaking, I believe, in *Science* or some other magazine, the bad name that science received, was caused by the unusual interest that is developed for palmistry, astrology, and Velikovsky. [Laughter, applause; the reference is to Murray Gell-Mann, who spoke in *Physics Today*, (May, 1971, page 23) of "astrology, palmistry, and Velikovsky."]

And why further to go? Open this week *Newsweek*, and you will find expression of one of the panelists of today, who also put me together with—in his book that just appeared; again, not only in *Newsweek*—with scientology and something else, which is certainly not a proper association.

And many others. A.A.A.S., only a year ago about, printed an article by Warren Weaver, for many years associated with this organization, and he repeated the words of Dr. Gell-Mann, the Nobel Prize winner I mentioned before, about palmistry and astrology and Velikovsky, and the bad name of science. [See Warren Weaver, *AAAS Bulletin*, February 1972]

So the sin that the science obtained a bad name was not put to those who performed all what they performed, but to me, to my door. So it is not sins of fathers; it is sins that were not yet discontinued. I hope that this new start maybe will be in that new direction.

GOLDSMITH:

Well, [delayed applause for Velikovsky] without then being quite clear whether we're sinning further or sinning less by continuing [laughter], I would like to pass on to the discussion Dr. Huber gave. It had dawned on me that perhaps Dr. Huber and Dr. Velikovsky are the only ones here who know exactly what they were talking about. [laughter] You would think the chances are limited [?], is one thing I have to say, but unfortunately too it only takes two to have a discussion, and I call on—first, Dr. Huber I think is the first one with a comment on what he said. And then I'll entertain questions— one member of the audience?

VOICE:

[inaudible]

GOLDSMITH:

We've got Mulholland because his talk is coming later on. We've gone to that time to go on or never get anywhere.

You want to ask Dr. Huber a question?

QUESTIONER:

I wanted to ask a question of the whole panel. [laughter]

GOLDSMITH:

. . . at the end we can ask questions of the whole panel, at the end of our ride.

QUESTIONER:

My question is meant—let me ask it. [laughter]

GOLDSMITH:

I would prefer to go on with Dr. Huber, and—

QUESTIONER:

You may, you may.

GOLDSMITH:

You're not going to let me? [laughter]

QUESTIONER:

Please let me.

GOLDSMITH:

Go right ahead.

QUESTIONER:

I would like to see the panel address itself to the problem or question of so many of Dr. Velikovsky's advanced claims being validated, whether or not his comprehensive and underlying explanations and theories are true or—let's even forget they even exist—it's very important for me to know—how could a man be right so many times when everybody else was wrong? [applause]

GOLDSMITH:

I'm impressed by the fact that we've gone through the discussion this morning without accepting the fact that many members of the panel believe Velikovsky to be wrong, and many believe him to be right. It seems to be a fact that we're stuck with. I don't think that asking ourselves, how can we believe he's right when he's wrong, and how can we believe he's wrong when he's right, will resolve the question quickly when it couldn't be resolved in four hours this morning. Does that seem clear? [laughter, applause]

So—exerting all my prerogatives, then [laughter] I would like to ask Dr. Huber for a comment and Dr. Velikovsky for a reply on the subject of Dr. Huber's talk this morning, the ancient historical records.

HUBER:

What comment? [laughter]

GOLDSMITH:

You said that you had a comment.

HUBER:

Oh. [laughter] I really have to answer Dr. Velikovsky's comments on my talk. And here it's really the matter of the length of the month and those intercalary months. First of all, I realize that the many members of the audience did not quite get the point that Babylonian months are lunar months. The beginning of the month is determined by the first visibility of the lunar crescent. So the months are sometimes twenty-nine days long, sometimes thirty days long, irregularly. And the question is, whether there was a thirty-day month at any time. And indeed there are thirty-day months, mainly in the latest five hundred years before Christ. The Babylonians used thirty-day months for certain purposes in the planetary theory. When they calculated a planetary ephemerides seventy years or so ahead they used the formal months of thirty days because they didn't bother to calculate the exact length of the month seventy years ahead. They were wrong by never more than a half a day, because the month's length is either twenty-nine or thirty days. [hesitating] More than one day. And, these thirty-day months seem to have either migrated to India or maybe it was invented independently. The idea is rather easy to find. And so also in India you use thirty-day months for certain purposes. But that really belongs to the mathematical astronomy of late times, and it cannot really be used as an argument that there was a real month of thirty days. [No one used this argument.]

And then the other question about the intercalations. Because twelve lunar months add up to about 354 days, twelve months fall short of solar year. And every third year, approximately, you have to insert an

additional lunar month. This was done in older times in a quite haphazard fashion by royal decree. Sometimes there were two or three intercalations in a row. Sometimes there were none for many years in a row. And these intercalations were inserted either after the sixth month of the year or at the end, after the twelfth. Now, we have from old Babylonian times very many contracts which are dated in intercalary months. That some person lent some money to somebody at that-and-that date and it has to be paid back at that-and-that date and if it is not paid back he has to pay so-and-so much interest. And these are actual documents from this actual time, and these are preserved. They give definite evidence on the intercalations. And Velikovsky was just plainly wrong when he claimed there were no intercalations in Ammizaduga's times. [Huber is "just plainly wrong": Velikovsky *never* claimed this.]

GOLDSMITH:

[to Velikovsky, inaudible]

VELIKOVSKY:

Yes. [laughter]

First, I will respond to what was said, namely, about the month of thirty days and twelve months in a year without intercalating days into the year, which were added later. I am very proud of these chapters of mine toward the end of *Worlds in Collision*, because I succeeded to quote from practically every ancient civilization, from Peru, to Mexico, to Rome, to Greece, to Babylon, to Assyria, to Persia, to Hindu, to China, to Japan, and to Egypt, and to Palestine, Judea, and probably several more civilizations, always quotation not by myself, always by specialist, always expressing the same wonder that no intercalary days—the year was just this: twelve months of thirty days—for a period of time, which was discontinued at the beginning of the eighth century. Soon after that time, in all places, in all civilization, one or another reform was done, and five or five and a quarter days were added by all civilizations. The reform was almost simultaneously—at least during one and the same century. And expressions were not just about months—about the Moon, the Moon travels in thirty days. Half of the Moon is fifteen days. Such great errors as a half day every month, to do was never done.

Yesterday, I discussed the question with Professor Huber, and, discussing Ammizaduga tablets, I told him that I believe that Ammizaduga tablets—asccribed usually to the end of the First Babylonian Dynasty; King Ammizaduga was the last or before last—I said that I am inclined to come closer to the views of Schiaparelli and Hommel, who believed that these tablets were written not in the second millennium, but in the seventh century. In my opinion, however, between tenth and eighth century. And then we came to the question, why? I said to him, because this was the time when the year was of twelve months of thirty days each, not only the month, but the travelling of the Moon.

And, interestingly—this I bring in before coming back to Ammizaduga—you can read it in every modern book of ancient calendars, whether Parker about Egypt, or anybody else across the two continents of Asia and Europe—Egypt included—and the New World. You find always the same expression of wonder about the Moon movement, not just the month. Now to this I wish to say.

When I mentioned to Dr. Huber that because of this I believe Ammizaduga tablets were written so much later, and the insertion of so-called "year formula of Ammizaduga," as Hommel suspected, may have been an insertion of a scribe—and Dr. Huber believes that very many insertion of the scribe, practically innumerable inscription of the scribe, were placed in those tablets, but his view is only to explain how *is* it that tablets have so many wrong data that could not be accounted by astronomical position of the planets, movement of the Moon, and the seasons of the Earth.

At this moment, we came to the question, whether in book of Langdon and Fotheringham is reference to thirty days and no intercalary months. Dr. Huber said, "No." I said, "Yes." It was fortunate for me that Professor Rose brought with him from Buffalo the text of Langdon and Fotheringham. And here I read it from page 2:

". . . no allowance is made for intercalary months . . . The scribe assume[s] a constant value of 30 days for the month."

Could not be clearer.

Now on this let me say: calendars were changed repeatedly. Professor Mulholland, to whom I did not answer [in the morning] on his paper because it's too much to answer, yet he raised several question[s], like question whether the clocks could be transferred, Egyptian clocks that were found and do not represent the proper time, and whether it could be that Egypt changed its position by thousand kilometers, and so on. No, it is not just geographical change of the poles. It is more the astronomical change of the pole, and the astronomical change of the pole would cause this situation. And we have in Babylonian texts thousands of tablets, tens of thousands of tablets, dealing with astronomical subjects. When in 612 B.C. the library of the King Ashurbanipal of Assyria—he was already dead—was burned by onslaught of Medes, Scythians, and Chaldeans, the palace was destroyed; the library was saved, because the tablets were burned to stone. When Caesar caused the destruction of the library of Alexandria, and Alexander caused—before this—the destruction of the library of Petropolis [meaning "Persepolis"], they were burned, because here were hides, and here were papyri. But the clay tablets turned to stone, and they are in British Museum.

And nothing what is before 700 B.C. is acceptable to astronomers. At Brown University, Professor Neugebauer, who wrote several important books on the subject, just offers not to occupy ourselves with those tablets, because—despite very advanced mathematics, no mythology invested in proofs—the phenomena there described could not be justified by the present state of the world: the length of the month, the length of the year, the length of the seasons, the time of the vernal equinox. And by the way, in a textbook on astronomy, so-called *Mul Apin*—[in] which one [of the] tablets deal with the planetary motions and the other with the Earth—you read about changes in the time of the equinox. Equinox at one occasion was transferred, vernal equinox, by 30.4 days. At another occasion was transferred by nine days. And this is not only there. And when we find about the ratio of longest day in the year to the shortest day of the year, and we find it in Babylon, we do not accept these values because they are impossible. Three to one, there is no such thing. But the very same thing in Egypt. Neugebauer wonders here [about Babylon]; ten years later he writes another article, about Egypt, [and] wonders for the second time, not remembering that he wonders already once about Babylon on the very same subject. [laughter]

Well, this is the situation. It is very composite structure. It is not given to so simple thing as to say: Well, people have travelled maybe to the southern hemisphere and saw the southern stars that were represented in the ceiling of Senmut, who was architect of Queen Hatshepsut of the Nineteenth [meaning "Eighteenth"] Dynasty, and as I reconstructed in *Ages in Chaos*, contemporary of King Solomon. Now, it was not contemporary ceiling, but it was something very holy for them. They kept it from the past. What happened? It is not just as Professor Mulholland said, they went to the south, they saw the southern sky. No, the Egyptian sources, many of them, say, about the catastrophe, when the north and south changed places. And in eighth century, when second series of catastrophe took place, you have thousands of sources; in Greek authors alone you have hundreds of sources, references, to the change of the position of the terrestrial axis in relation to the sky. So it is not so simple to explain everything: the ancient did not know anything, they did not care, half a day does not count. No such thing.

When America was discovered in 1492 and later the Mayan calendars were studied, it was found that they are more exact than the Gregorian calendar that was introduced in Europe—and this which we

follow still—ninety years after the discovery of America. And Mayans were already at that time much closer to the true figures than we are today. So how to put on the ancient this kind of accusation? Who knows? They did not care, six days in a year! [laughter] What is on the ceiling, this was just traveling. The clock does not show right, maybe it was transferred from one place to another. But somebody has to read my books carefully in order to read it [the clock].

And despite the fact that Professor Mulholland started saying that he read my book first when he was fourteen years old—but I question whether he really remembers what he read [laughter] because he starts his paper by saying that Venus and Mars were described by me as two comets. Who read my book would not make this statement.

GOLDSMITH:

OK, and—[delayed applause for Velikovsky]

HUBER:

I have only a very short comment.

GOLDSMITH:

One short comment. One very short comment.

HUBER:

I would like to recommend to Dr. Velikovsky to read beyond page two of Langdon-Fotheringham-Schoch; the matter of the intercalary years is discussed by Fotheringham on page 61.

VOICE:

Where is your point?

VELIKOVSKY:

Very good. [laughter] But may I say to you one little thing. I believe it is very interesting. Professor Huber wrote, a year or two years ago, a review of a book on tablets of Ammizaduga, published in Istanbul. Now he comes with ideas, and please follow. I will read it in text, German; I will translate it into English. Page four.

GOLDSMITH:

Perhaps we could just do the English translation, [laughter] unless there is some strong opposition. [laughter]

VELIKOVSKY:

In order to be exact. Maybe my quotation will be criticized, so I give the original, too.

"Dazu kommen . . ." [applause]

He says why could not be this, why do these tablets show those wrong figures, and he says this:

"Dazu kommen noch hypothetische Fehlermöglichkeiten - wie der äusserst unwahrscheinliche, aber nicht auszuschliessende Einfang eines grösseren fremden Himmelskörpers durch das Sonnensystem in historischer Zeit - die hier ausser acht gelassen werden sollen."

"To this also come hypothetical sources of error – like the highly improbable, but not to be excluded capture of a rather large foreign heavenly body into the solar system in historical time – which should be left out of consideration here."

GOLDSMITH:

Doktor Huber, hopen Sie da eine kommentin machen? [laughter]

HUBER:

The only comment I have is that the probability is so small that I thought I could leave it out, and in case I was able to explain the text without that hypothesis. The same argument has been used by other people in this audience: if something has a very low probability, then you do not believe in it. And I do not believe in it, because the probability is, in my opinion, really too low, if you just estimate the mean time between two encounters in the galaxy.

Now at the end of this same review, what I say – what is it? – the last chapter, I'm translating into English, the last chapter of Weir's book:

". . . the Postscript contain many in part very interesting speculations about changes of the orbits of the Moon and Venus. I don't know how far these are correct. But I have to stress that the sources of errors A, B, and C1 – that is, copying errors, observational standard deviation, and clock-time errors – suffice to explain the observations. One has to assume about fifteen – that is, thirty percent – of scribal errors."

GOLDSMITH:

I'm going to pass on in the period . . . to do with Assyriology. One would like to be able . . .

DROWN:

I have a question for Dr. Huber. [laughter]

GOLDSMITH:

You do?

DROWN:

Oh, yes, strange as it may seem. On your fourth slide this morning, you had an artifact—I believe you said [it] dated from minus three thousand—showing Venus as an evening star. I was curious to know how do you read that artifact? Do you read it from top to bottom, left to right, as is common in western culture? And if so, do they have the same north-south orientation that we have in the western culture? And if that's the case, then the evening star showed up with the Sun setting in the east, and not in the west. [laughter]

HUBER:

Oh. That's, that's a good question. [laughter] These texts—some time in historical times, the direction of writing was turned around by ninety degrees. Now that photograph as I showed it is so that you have to read it from top to bottom, from left to right. There is one difficulty with these very old tablets. These are subdivided into cases. And the arrangement of the signs inside the cases is apparently random, which makes [it] *very* difficult to understand these tablets and to read these tablets. The actual order is of only very few signs—three signs in this case—in this last case. And the actual order of these three signs doesn't seem to matter—they just placed the signs where they are convenient. It's quite similar with Egyptian hieroglyphs. They are arranged where there is space.

GOLDSMITH:

You really think you have a short question?

RANSOM:

Yes.

GOLDSMITH:

Let's give it a try. We'll make it the last one before we go on.

RANSOM:

You made quite an extensive discussion of the tablets talking about the morning and the evening star before 1500 B.C.

HUBER:

Yep.

RANSOM:

Since this event could occur with an object having an Earth-crossing orbit, why did you make such a big deal of it?

HUBER:

I think I have a good answer on that. And oh, I think it's at least as convincing as the mythological arguments offered by Dr. Velikovsky. The answer is to be sought in the myths on Inanna's descent to the netherworld. I am convinced that the stay of Inanna in the netherworld has to do with the long period of invisibility of Venus at superior conjunction. I mean, the invisibility of Venus seems to be of the order of sixty days, but—The corpse of Inanna is hung on a stake, and it has to be sprinkled with the water of life sixty times. And I am personally convinced that this refers to sixty days. And if the period of invisibility is that long, it cannot be a comet.

GOLDSMITH:

OK. I am going to move on to Dr. Velikovsky's talk. Participants are free to talk, after, on these important subjects, which I'm having trouble following myself. Now since we are going to discuss Dr. Mulholland's talk after Dr. Velikovsky's, I'd ask for questions concerning Dr. Velikovsky's talk more than Dr. Mulholland's, if you see what I mean, at the present time, and we'll go on later to Dr. Mulholland's talk. With that in mind, there's a question here.

QUESTIONER:

It's a well-known fact today that, as I'm sure most of you in the audience know, the Moon rotates with a period which is precisely equal to its period of revolution. In the light of what is claimed to have happened to the Earth as a result of the close passages of the Earth and Venus, I would be interested in Dr. Velikovsky's explanation as to how this precise synchronization of the Moon's rotation and revolution could have continued through this holocaust.

One other question, of a like nature. I think it is generally accepted that the Great Pyramid of Gizeh was built before this close approach. The sides of the Great Pyramid are oriented—north, south, east, west—within, as I recall, about three minutes of arc, about the smallest angle one could expect the orientation to be if surveying was done with the naked eye. It seems a rather unusual coincidence that this north, south, east, west orientation could have come out of an Earth that had been thrown into such a chance disorientation by the close approach.

GOLDSMITH:

I'm sure these are questions Dr. Velikovsky has heard before, and will answer again.

VELIKOVSKY:

Concerning the pyramids, the question was asked first by Professor Stewart—John Stewart, astronomer, Princeton University, now dead—in debate that I had with him at *Harper's*, June, 1951. The question is this: How pyramids are properly oriented? Now let me say this. Suppose it will happen today that the Earth will change the position of its terrestrial axis, astronomical position, to a new direction, whatever it should be, even complete reversal, partial reversal, anything. The pyramids will remain properly oriented. Nothing will happen.

VOICE:

. . . the question.

VELIKOVSKY:

The question is, of course, what would be with the pyramids in case of twisting, and whether any building from before could remain standing, the question was asked me, too. Actually, I spoke at Yale University about six or seven years ago, and I believe the gentleman who at that time organized—he was student of architecture, now he is architect in Washington D.C. —and he peddled proposition that Velikovsky should be invited [by] all departments—history of science, science, anything; history, archaeology—none would respond. All were very interested, but none would respond. Then he reapproach his own department, architecture. Architecture [did] respond. Now what should I speak about? About the purpose and orientation of the pyramids. And when I arrived at Yale, they needed to transfer the lecture from the large hall in R building into the largest auditorium they had, and for two hours I occupied them with this talk. So I cannot occupy so long you. [laughter] And I will say only this. In case of geographical disturbance, and if this geographical disturbance in the position of terrestrial axis, which point I do not support any more in *Earth in Upheaval*, claiming that this must have been, because of terrestrial equatorial bulge, a temporary disturbance returning—after wobbling, which still exist to some extent, the rest of this wobbling, Chandler wobbling—to its previous position, geographically.

So I elaborated on my view in *Worlds in Collision* as a temporary disturbance, geographical one; astronomical being permanent and being caused without much energy applied necessary. Professor Michelson gave you some idea what forces would be needed to be applied. Professor Thomas Gold, of Cornell University, published two papers, one in beginning of 1960's in *Nature*, the other I believe in 1964 in *Science and Telescope*²¹³⁶, and he expressed himself even to that effect, that actually no force is necessary whatsoever to turn the terrestrial axis in a new astronomical position, if the Earth were a perfect sphere. If it were a perfect sphere, he illustrated his idea, which was a revision of that debate that went on last century between several scientists, like George Darwin, son of Charles Darwin, and Lord Kelvin, and others, that no energy would be spent; that an insect crawling on a spherical Earth would be able to turn it into a new position.

Now, Professor Mulholland comes from Observatory of Paris, where he spends most of the time, and to where I sent him some material when I found that he is there and have not the books with him. Professor André Danjon, director of that observatory, as I mentioned in the morning session, created a, well, sensation—sensation of disbelief, if you wish—when in the summer of 1960 at the annual meeting of the Geophysical Union, that happened to be at Helsinki, he announced that after a flare of the Sun that rotation of the Earth lost something like few milliseconds, which strangely the day thereafter started again to accelerate by microseconds. And this happened more than once. Idea came from Harvard—Menzel—maybe thermal phenomenon. No. Professor Schatzmann, collaborator with Danjon, published like Danjon in *Comptes rendus de l'Academie de France*, a calculation that it could not be a thermal effect; it must be an electro-magnetic effect.

GOLDSMITH:

I'll ask a comment from Dr. Mulholland before we go back to the audience.

²¹³⁶*Nature*, March 26, 1955, pp. 526-529, and *Sky and Telescope*, August 1961, pp. 100-101.

MULHOLLAND:

First of all, I would observe that I'm not at Monsieur Danjon's observatory, but -

GOLDSMITH:

Is that my correction, Dr. Mulholland?

VOICE:

Speak up, please?

MULHOLLAND:

[not answering Goldsmith]—but rather at Meudon, which is not the same place. [Nevertheless, the letterhead of the stationery on which Mulholland wrote to Velikovsky does read "Observatoire de Paris," the "Meudon" is underneath and in smaller type.] However, it needs to be said that Danjon was wrong about that, that the data do not show any such effect.

But I would like to make some further remarks about the entire business of Earth axes. First of all, it has to be recognized, in discussions of this nature, that there are three axes of the Earth under discussion here. And it's necessary to keep them separate in one's mind, because shifts of them are due to different causes and require different phenomena to drive these shifts. There is, first of all, the pole of rotation, which is what we customarily refer to as the north pole of the Earth, which is pointed roughly towards what we call sometimes the North Star. There is the pole of figure of the Earth, that is to say, the direction that is perpendicular to the equator of figure of the Earth, the equator of shape of the Earth. And then there's the pole of the magnetic field of the Earth. And I have the vague impression, reading Dr. Velikovsky's books from time to time, that there is not always a distinction made between these things. It is not necessary that when one of them moves they all move. *Not* necessary. And in fact, with respect to the changes in the magnetic pole, we know that the magnetic pole of the Sun reverses quite regularly without any effect whatsoever on the other poles.

Dr. Velikovsky, in his remarks just now, seems to have backed down on the question of moving the pole of figure of the Earth, and says instead that the pole of figure moved briefly and then moved back to its original position, which requires two separate events, not just one, and that what changed permanently was the astronomical pole, by which I assume he means the rotational pole, the one that one can determine from observing the stars.

I would simply like to make the point here that a change in the rotational pole will not affect how a clock works. And therefore the evidence that he adduces in *Worlds in Collision* for a shift of the pole does not concern this pole at all, but rather the geographic pole. In addition, he conveniently ignored my major point with respect to the clocks this morning, which is not that such-and-such a place moved one thousand kilometers, and such another place might have moved something-or-other else, but that two places that moved south and one place that moved north, by large amounts, are located in a geographically very small area, and that this is simply kinematically very difficult to imagine how it can be, just by turning the Earth over.

VELIKOVSKY:

May I answer?

GOLDSMITH:

Yes, go ahead. [?]

VELIKOVSKY:

First I wish to say that Danjon was not proven wrong. Just most recent literature proves him right, confirmed again, and quite a few articles on this score were in the last few months printed. This is number one.

As to pyramids, I wish to add—

Here—Professor Michelson gives me the reference—Gribbin and Plagemann, "Discontinuous Change in Earth's Spin Rate following a great Solar Storm of August 1972," *Nature*, 242, pages 26-27. So it's very recent. So he was not disproven, but proved.

Now as to the clocks, and—

Oh, no, first I wish to say one more word about the pyramids. Since I spoke at Yale came up a series of articles, namely in *Nature*, also I believe in *Science*, dealing with the new fact that actually the pyramid of Cheops—that always is the case of observation and discussion—is not properly oriented. Actually, the measurement go back to Flinders Petrie, who was both Egyptian archaeologist and surveyor by education. And then started the debate in the press, the last year, namely, the question was, what could cause this disorientation of the pyramid? It was assumed that this disorientation could not be the result of a bad surveying of the ancients. And the idea was suggested that so-called continental drift was the cause. Now, as to continental drift as the cause, calculations were made and rejected, namely, continental drift could not, in this short period of time, geologically speaking, produce the effect of the pyramid's being geographically now disoriented. So they are not entirely properly oriented. This is one thing.

Now, I had no chance to read all your paper that you gave me so kindly this afternoon. There is many things that I would like to make remarks about them. But now you, I notice, for example, the question of changes in latitudes. These changes in latitudes not necessary changing because of the geographical shift, but in inclination of the terrestrial axis. And I never confused magnetic axis and the axis of the spheroid, which the Earth is, or the axis of the rotation. Never. I wish to be shown a point where I confused. I know my work to that extent.

Now, to me is ascribed as if I claimed that Babylon changed its latitude by so many kilometers. I did not claim it. Who claimed it? From where is the source? I gave the source. I don't look [haven't looked] in my books so far. I remember it. The source is Kepler, Johannes Kepler. And in his *Opera omnia*, Volume Six—this I still remember, so my book is written over twenty years ago—and he refers to his source, and his source is Arabian astronomer—Arzachel by name—and again reference goes back to Ptolemy, Claudius Ptolemy, of the second century A.D., astronomer of Alexandria. So it is not my invention. They calculated, and writes Kepler: Babyl.—which means the ancient Babylon—must have been two and a half degrees away. How to explain, he does not know.

Well, I think that we are touching less important problems. The time [is] running short, and the greater problems in my work were: were catastrophe, or were not?

Professor Huber admitted me that he read parts of my book. Now with reading parts of my book, it's difficult to say, "I don't believe that this catastrophe happened." As long as it is parts and not all, maybe it is "eighteen minutes" lost, maybe it is hundred eighty pages lost, we don't know. [laughter]

The main problems are two, if we are speaking here in the frame of the Astronomical Section. The one: Historical sources, unanimously—not that there's just here or here some fantasy, some myth, some legend, some prayer—but all of it. If you don't read Babylonian, which means Akkadian, if you don't read

Mexican, if you don't read even Latin, but maybe you read Old Testament. And if you don't have Old Testament, next time, in a motel, find one, [laughter, applause] open the pages at least twice, better chance than fifty-fifty, that there would be story of catastrophes. [laughter] What they were filling all those pages? [laughter] What were filling those astronomers of Babylonia thousands of tablets with observations if nothing was necessary to observe? Something was on their minds. Something was very important. Why they were building these temples to the planets? Why were they bringing sacrifice, human sacrifice, to those planets? I asked this [in] the morning. This [is a] question that need to be thought through, not just brushed away. We lived in a careful world, everything was going on [that is, on-going], we came from children [?] quite plain, nothing happened, no catastrophe, no thing was in the past. We live in the scientific age of uniformitarianism, where anything that is not observed today could not [have] happened also in the past. This is our rule; according to this we study and learn and believe.

The second question is—and to this question there is plenty I could say—Venus is hot and Mars gives off heat. Dr. Sagan said this morning not. And I regret that Sagan threw in the sponge [laughter] after mentioning ten points, and, behaving like a boxer before they box, [laughter] but very soon was out of sight. [laughter]

GOLDSMITH:

I think here, Dr. Velikovsky, that these *ad hominem* comments—wouldn't want to get away from discussing your ideas.

VELIKOVSKY:

Well, he made some *ad hominem* comments in *Newsweek* of this week. With this I say that it's pity, because scientific subject should be dedicated, should be discussed. You are here, if you are constructive minds, to help to solve. I didn't offer you all solutions. Something was left for you to solve. Something for the next generation, too. But, nevertheless, already this explanation entered so many fields and returned with confirmation from so every field, that I believe there is a reason for scientific community to discontinue its psychological, well, resistance, which we in analysis call resistance, and start reading the book, contemplating the quantity of material, and say: historically proven; now, how can it happen physically?

Professor Michelson gives the example. He is a shining example. He could be wrong on the last point, speaking about that this could be the solar flare—something that would give this energy. No, it was not solar flare, it was something else. Something what the ancients saw and described, and not once described. All their literature is filled with it. Everything what came from the ancient literature speaks almost only about this. And I saw here Margaret Mead—and she was in South Pacific—and whoever was, like my old friend (dead when I came to this country), Boas, he was on the Pacific coast to the north. All what they hear, all what they know, it is about the disturbance of the Sun, about something happening to the length of the day and so on, and so on, and so on. It is not just pleasant stories, not just taken from one culture to another.

Now—

GOLDSMITH:

Dr. Velikovsky, do you think we could entertain a couple more questions from the floor?

VELIKOVSKY:

Please.

GOLDSMITH:

Before we get—see, there are twenty-five minutes left. There are a large number of people who still have a large number of comments. I'm going to start—there, I think, for brevity, brevity.

QUESTIONER:

First of all, I do find myself puzzling, and I would ask that type of a question. Is it possible that with the admittance of past wrongs—Is it possible that today's contemporary world, a methodology could be developed, one way or the other, on interdisciplinary approach, as other problems to solve, to at least go from here on? Have we reached that level? Can this conference come with that type of a positive statement? Or are we still, shall we say, skirting the overall issues, not finding any commonality yet?

GOLDSMITH:

It seems to me that the question is one that hangs in the air but cannot be answered by any individual.

QUESTIONER:

Well, in a positive way, just rephrase it. Where do we go from here? What is possible? And, is there a methodology possible with a scientific approach when you say quote, quote, whatever you want to call it?

GOLDSMITH:

Well, we'll get a comment from right behind you on what's possible.

QUESTIONER:

Yes, there is. [laughter] There is a very simple solution to all of this whole enigma that has plagued man for centuries. We get an answer some of the questions. I can tell you about some of the research work conducted by a very close friend of mine, Dr. Nicola Tesla. I never knew him in this body. But Nicola Tesla is listening, and he answers the questions through me. Nicola Tesla designed a synchronous motor. Well, that's what the planet Earth is. Throughout the period of time, over history, as a result of the Sun reversing its magnetic field, the Earth acts just like an armature. Then energy is stored, and at the end of a certain number of years, flipped over on its axis. That's in answer to the enigma. [laughter] Now, Dr. Velikovsky, a while ago, uh, I can't remember. Next, when I can remember what he said, I'll stand up and I'll relate the answer. [laughter]

GOLDSMITH:

Maybe it was Tesla.

QUESTIONER [continuing]:

I'm too overwhelmed.

GOLDSMITH:

OK. The question far in the back the, we'll – Yes. Please speak loudly.

BURKHARDT [the spelling may be wrong]:

I have a few things on this.

GOLDSMITH:

More loudly.

BURKHARDT:

There are a few things that this has brought up to me – one thing, I must say I admire scientists a great, great deal because they do seek truth. And I just have another thing or two. I'm Dr. Burkhardt. I've thought a great deal about this planetary system, the Sun and our whole solar system. I've come up with a lot of research concerning Polaris. And I'd like to present the thought that perhaps our Earth started falling – you might want to call it falling; in terms of relativity maybe it's climbing – but perhaps it's falling from Polaris, and as it falls, it falls at a speed in excess of the speed of light, which we might call Mach light; therefore this is the reason that light is more or less constant. And then I can also, I can leave it, at some time, if anyone wants to discuss it, go into the details of why light comes in waves and also why it's in particles. And get into photography with you. I think also that there are some other very brilliant stars in the solar system that could have something to do with the ellipse of the Earth and –

GOLDSMITH:

Is there any particular question that the panel should consider on this? Right.

BURKHARDT:

Well, the main thing is, I'd like to propose that if there's anyone interested in really sitting down and working on some of these theories, instead of trying to refute everyone else – I like to think that Einstein was one of the bravest thinkers of our time, and that the reason he was – [applause]

GOLDSMITH:

He wasn't bad, but –

PANELIST:

Call him who?

BURKHARDT:

– the reason he was is because he didn't keep his mind locked in a little box, that he dared to get out of that box, and I see many people here today doing that very thing.

GOLDSMITH:

OK. In the interest of time we'll congratulate ourselves quickly –

QUESTIONER:

I have a question.

GOLDSMITH:

– and move on to someone who seems to have identified himself already as the next questioner.

QUESTIONER:

The question is, Dr. Velikovsky, are you aware of the recently discovered anomalously high temperature of the satellite, I believe it's called Titan, of one of the major planets, I believe it's Saturn? And does your theory offer an explanation of this anomalously high temperature, which at first glance resembles the situation of Venus?

VELIKOVSKY:

[to Goldsmith] What is the question?

GOLDSMITH:

The question is—The question I will repeat. And I hope for a brief answer, although it's a complicated question, is: Are you aware of the recently discovered anomalously high temperatures of Saturn's satellite, Titan, and how does this fit in, if at all, with your theories on the planet Venus?

VELIKOVSKY:

In what the Greek called Theomachy, practically the entire solar system was in disorder, disarray. And actually if a planet with a charge will enter the solar system, I believe this should follow. Now in works like Homer's *Iliad* you have a very detailed description—so mythologically clad—of events taking place in the sky when the Achaean host makes the war against the people of Troy. And you see that many planetary bodies are involved. Moon is disturbed by Mars. And Venus is repeatedly in conflict with Mars. And not only these three bodies are in disarray.

I believe that the second important question, besides the question of disturbance, which could be shown by the fact that Mercury is on its place, astronomers calculate maximum four hundred thousand years, because of the temperature of the shadowed side. And this would be just—how much?—if four billion years the solar system is, and, four hundred thousand, it would be just one ten-thousandth of the history of the solar system.

And Venus is very hot. And Moon shows the great disturbance in the past, which I was given the chance to claim in advance of the landing on the Moon, when editors of *The New York Times* asked me to write an article for that special evening of twenty-first of July, nineteen sixty-nine, when Apollo 11 men stepped on the Moon, and I had the chance to claim what there would be found. And all what I claimed, everything came into fulfillment.

So, well, it is not, as the case is, I was regularly wrong: I was regularly right. I asked Professor Sagan to say which is the case where I was wrong. He started to speak about hydrocarbons. I showed he spoke himself about hydrocarbons. And this is not yet closed issue; according to the spectral figure, it must be most probably organic material in the clouds.

Now, so, Venus is hot, and Mars is hot and disturbed, and the Moon showed these mascons with which Professor Mulholland occupies himself, and I give him a simple solution. These mascons were caused in these close approaches described by ancient authors between Mars and Moon. And of course then Moon would be heated and it would be pulled, the mass, toward the surface, and this would create the mascons. Mascons states [that is, stands] for mass concentrations.

Now, the other question, of course, is—in astronomy speaking here—question of whether inertia and gravitation are the only forces. Let us put this clearly so. In solar system electrical and magnetic forces do participate. I could not know to what extent. I claimed more than zero. And I claimed that even a bank—two deposits are not in calculated [that is, calculated in], and the balance sheet is absolutely correct. I claimed that the balance sheet is wrong. And these are two deposits.

Now I wish to read one or two sentences, and before I read them I wish to exonerate Isaac Newton, because all his fault was to be born before Faraday. [laughter] Well. You know the sentence very often mentioned, "I do not frame hypotheses," about the nature of gravitation. And, strangely, the most interesting sentence, with which he ends the *Principia*—Book Three, "The System of the World," ends—and I wish to read it, because it's most profound sentence. And it is:

"And now we might add something concerning a certain most subtle spirit which pervades and lies hid in all gross bodies; by the force and action of which spirit the particles of bodies attack one another at near distances, and cohere, if contiguous; and electric bodies operate to greater distances, as well repelling as attracting the neighboring corpuscles; and light is emitted, reflected, refracted, inflected, and heats bodies; and all sensation is excited, and the members of animal bodies move at the command of the will, namely, by the vibrations of this spirit, mutually propagated along the solid filaments of the nerves, from the outward organs of sense to the brain, and from the brain into the muscles. But these are things that cannot be explained in few words, nor are we furnished with that sufficiency of experiments which is required to an accurate determination and demonstration of the laws by which this electric and elastic spirit operates."

I think this is a most profound prophecy in Newton.

Now—

GOLDSMITH:

Dr. Velikovsky, we're down to our last minutes, ten minutes or so. I wonder if I could get just a few more comments, one from Dr. Mulholland, and a couple from the audience, and then it'll be just about time to fold our tent.

MULHOLLAND:

Well, first of all—

GOLDSMITH:

Briefly.

MULHOLLAND:

Thank you.

Well, first of all, I would like to commend Dr. Velikovsky for exonerating Isaac Newton. That's very nice. However, I do wish that he extended the same courtesy to Kepler's dynamics and paid a little less attention to Kepler's LSD trips that he wrote so much about. Kepler is a very weak reed upon which to lean except in dynamics. However, that's not really what I wish to speak about. [laughter]

There were two items. First of all—

GOLDSMITH:

Brief, brief.

MULHOLLAND:

When discussing a subject such as this and bringing to bear fossil evidence of various kinds, which is very impressive, perhaps the evidence itself should be a warning against letting one's own ideas fossilize. Dr. Velikovsky insists on discussing the celestial mechanics of the turn of the century as though they were the celestial mechanics of today. And that is not true. He insists on discussing celestial mechanics as though we only talked of gravitational fields. But in fact the dynamics of systems involving electro-magnetic fields, charged bodies, atmospheres, and various other non-gravitational effects are well studied, not just studied theoretically, but experimentally. Every one of you out there, I hope, pays a certain amount of taxes. Many of you pay more taxes than our President does. [laughter] And I should take this opportunity to extend to each and every one of you who do pay your taxes, thanks, for helping to support the space program, which among other things is an experimental test-bed for exactly these dynamical ideas which Dr. Velikovsky would like to have you believe are unstudied.

Now, as for the question of the hot Venus. Present measurements show that Venus does not emit more heat than it receives. Furthermore, Venus is not cooling off, as Dr. Velikovsky has claimed in *Pensée*, Volume II, Number 2, [that is, IVR I]. There's an appearance there that Venus is cooling off, but that appearance is quite illusory, for the following reasons. He says:

"I offer the new proposition as another crucial test of my theory. Since "Venus gives off heat" . . . the drop in [the] temperature . . ."

Well, I can pass over that and pass to the substantive part:

"In the 1920's E. Pettit and S.B. Nicholson measured the cloud surface temperature and obtained ca. -250 degrees C for both sides . . ." [This was a misprint in *Pensée* IVR I for "ca. -25 degrees C."]

That's not true. C means Centigrade. What they found was -250 degrees *Kelvin*, which works out to -23 degrees Centigrade. [Mulholland is wrong: -23 degrees C = 250 degrees K; there is no minus Kelvin.]

[Laughter; many people talking at once]

GOLDSMITH:

There's something strange about what you said there, Derral.

VELIKOVSKY:

Is this the same?

VOICE [HUBER?]:

Plus, plus 250 degrees.

VELIKOVSKY:

What? This is the same?

VOICE:

He blew that one.

MULHOLLAND:

I'm sorry.

. . . [inaudible murmurings] . . .

VOICE:

250 degrees Kelvin is absolute. He blew that one. There is no -250 degrees.

VOICE:

[There is no minus?] K, of course. [laughter, applause]

MULHOLLAND:

Yes. I'm sorry . . .

I'm sorry, -250 degrees Fahrenheit. [Mulholland is wrong again: he should have said 250 degrees *Kelvin*, or else -9.4 degrees Fahrenheit; in any case, he is belying King's introduction of him as "a celestial mechanic whose name is almost synonymous with high precision."] I'm sorry. That's — The last in the string [no; Low and Gillett and Stein were the last], he quotes Strong and Sinton in 1956 as indicating, quote, "approximately 40 degrees C." That paper says "approximately -40 degrees C." [This is correct; another obvious misprint in *Pensée*.] So that over four measurements [meaning "three"?] the total change is very small [actually, it would be nearly five degrees Centigrade in thirty years, hardly negligible] and not monotonic. So we are not dealing with a change. We are dealing with determination errors, not Venus' heat decreasing.

VOICE:

[inaudible]

MULHOLLAND:

Yes, I marked that down wrong, that's right.

VELIKOVSKY:

To this I wish to answer. Professor Mulholland is unaware that Pettit and Nicholson, thirty-five years [it was actually about thirty years] after their original publication, on the basis of the very same material, recalculated and published a new report, in which they instead of -25 put -38 Centigrade for the [cloud] surface temperature [for the day side; their figure was -33 (that is, *warmer*) for the night side] of the Moon [meaning "Venus"]. And therefore, between the figures of Pettit and Nicholson of [1924-] 1928 and the figures of Strong and Sinton of — what was it? — 1956, was quite a drop in the temperature. Now, after Strong and Sinton, at that time I suggested that these experiments should be made because temperature from the clouds can be measured to great exactness. However, it is necessary that the experiment should be done by the very same experimenter, and from the very same latitude of Venus, and in the same circumstances as to solar flares, and so on. Now it so happened, and I suggested in eight years later to measure the temperature of the clouds again. Not that my suggestion was followed, but Low and Gillett and a third author [Stein] — exactly eight years later, which is five synodical periods of Venus — published a paper [the *publications* in question were separated by eight years, or five synodical periods, but the actual *measurements* were separated by about nine synodical periods], and they wrote enigmatically — [they] did not give the figures but they wrote — strangely our figures are much lower than the figures of Strong and Sinton. And we do not know the explanation. And I quoted it here. [*Pensée*, IVR I, page 51] So I believe that reason to repeat the experiments more carefully with this special purpose is, well, well-recommended.

Now to few other things that Professor Mulholland said, I wish to remark.

As to question of electro-magnetic phenomena in the solar system, since the beginning — we are now in '74 — since 1950, since beginning of the century, we in calculate [that is, calculate in, include in the calculations] these electro-magnetic phenomena. Not correct. Just properly, simply not correct. And what better if I would quote you, no by heart, but I have here couple letters of later date, but not by heart. A first letter in the eighteen-months' exchange of letters and long sessions, sometimes to midnight, between me and Einstein on this very subject. And the first letter sounded — in German it was, and it was typed; later were always handwritten — and it was that way:

"It is nicht die Annahme aber es ist the fact that on Grund dieser Annahme – what this means, die Annahme – dass nur gravitation, dass nur gravitation or inertia in the solar system participating, es ist ein fakt dass on Grund from Dieser Annahme wir können kalkulaten, wir können ausrechnen die Position des Himmelskörper to ein unimaginable exactness."

I put one or two English words. Sorry for this. Which means, he says:

"It is not the assumption that only gravitation and inertia participate in the solar system. It is a fact that on the basis of this assumption we can calculate the position of the planetary bodies to the greatest exactness."

Now here is a statement of Einstein, another one, if I have it – here it is – from my letter written in June, 1954:

"The real cause of indignation against my theory of global catastrophes is the implication that celestial bodies may be charged."

He wrote to the side, "Ja," which in the German means "Yes." This is the cause of the great indignation.

If you wish I will continue, but maybe not. I will not only say that on the same page I also offered:

"Of course I am heretic, for I question the neutral state of celestial bodies. There are various tests that could be made. For instance, does Jupiter send radio noises, or not? This can easily be found, if you should wish."

Nine days before his death, ten months after this letter, I brought him the news that these radio noises from Jupiter were discovered. And he was greatly impressed. Now in the meantime we had a much longer negotiation.

You mentioned also why do not I refer to Kepler. Let me refer to Kepler. [laughter]

MULHOLLAND:

How selective, and which point of Kepler?

VELIKOVSKY:

Well, I will tell you: the point that you raised. And here is a quote from Kepler. *Opera omnia*, Volume VI, page 345, [laughter] in Latin, as he wrote. [laughter]

"[Sol] trahendo et repellendo retinet, retinendo circumducit."

He claimed that magnetic – this was idea of Gilbert, but he followed – that magnetic interaction between Sun and the Earth is the reason of gravitation. Now he says,

"The Sun pulls and repels, and by this retains: and by retaining, circumduces, carries around."

This is Kepler.

Now I wish only to give a little credit to a man who was, so to say, at the cradle of this conference, Dr. Howard Greyber. Is he here?

VOICES:

[inaudible]

VELIKOVSKY:

He was here.

Well, in a review in 13 June in 1969 in *Science* magazine, he writes:

". . . while the observers make one breathtaking discovery after another, leading theorists cling to elaborate reworking of an old purely gravitational model for spiral galaxies that, after *four*" – it's italicized – "*four* decades, still does not fit the facts. Models in which the galactic magnetic field plays a role comparable to that of gravitation are almost ignored at this symposium" – the symposium of 1969 – "although"

they protest, and so on.

So you have the story. It is a new chapter being opened. It was not opened in 1950. It was opened in the year I. Newton wrote his book, with which he closed. But then it was forgotten. Laplace was man who calculated and actually brought celestial mechanics on strong foundation, and also brought the astronomy to the belief that no changes could have taken place. And it's the same Laplace printed an unusual article in which he claimed exactly all what is in *Worlds in Collision*. If somebody would read it, he will say that Velikovsky stole from Laplace. [laughter] Comet, charged Earth, the sea lost its beds, mountains rose high in the sky, and all civilization were destroyed, and more, and more, and more. It is Laplace. So this dichotomy between, on one hand, to write about this stability of the solar system, on the other hand to write that the Earth *was* in conflict with some cometary body that it almost brought it to the brink of destruction, is the work of Laplace.

So astronomy – and you are in the younger generation comparable with me – are you invited to go into open gates. Don't try to close them. And science is not a closed, one unit here, and one unit here,

department, divided in compartments, and one does not know what is going [on] in the next place. Generalization or synthesis in science is maybe the new approach to which I was to some extent, well, I [have] something done for this.

And maybe I finish for today by just quoting one or another sentence on this score, not of my hand, if I can find the quote. [laughter] Here it is. Here is August 31, August, 1973, *Science*. It is in a review.

"The supreme factor in clear writing cannot be communicated, and is not always learned in a lifetime of experience: it is wisdom. A science exists as a vast depository of facts and theories, from which must be chosen a few significant and central themes. To shape a subject in the mind requires more than technical knowledge: it demands that "desire and pursuit of the whole" called natural philosophy. The style is the man."

And again, Danielli, who moved here to California, just recently, after having succeeded to create life in a tube:

"Among our immediate necessities we must include the following: our present enormous need of generalicists [corrected immediately to "generalists"], who can cope with complex [corrected immediately to "complex"] system as a whole."

Well, you see by me stammering that I am already tired, and you are, too. And thank you. [applause]

GOLDSMITH:

Thank you for your cooperation, ladies and gentlemen. I'm afraid the evenings being what they are, tomorrow being what it is, it's time to go home. Thank you.

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