

My Challenge to Conventional Views in Science

“Books written about the solar system before the advent of the space age could as well have been written in Latin or Greek, so dated do they appear to a contemporary reader.”

Zdenek Kopal - *The Solar System* (Oxford University Press, 1973)

In my published books, notwithstanding often repeated allegations, no physical law is ever abrogated or “temporarily suspended”; what I offered in them is primarily a reconstruction of events from the historical past. Thus I did not set out to confront the existing views with a theory or hypothesis and to develop it into a competing system. My work is first a reconstruction, not a theory; it is built upon studying the human testimony as preserved in the heritage of all ancient civilizations—all of them in texts bequeathed beginning with the time man learned to write, tell in various forms the very same narrative that the trained eye of a psychoanalyst could not but recognize as so many variants of the same theme. In hymns, in prayers, in historical texts, in philosophical discourses, in records of astronomical observations, but also in legend and religious myth, the ancients desperately tried to convey to their descendants, ourselves included, the record of events that took place in circumstances that left a strong imprint on the witnesses. There were physical upheavals on a global scale in historical times; the grandiosity of the events inspired awe. From the Far East to the Far West—the Japanese, Chinese and Hindu civilizations; the Iranian, Sumerian, Assyrian, Babylonian, Hittite-Chaldean, Israelite and Egyptian records; the Etruscan, Attic and Roman theogonies and philosophies; Scandinavian and Icelandic epics; Mayan, Toltec and Olmec art and legends—all, with no exception, were dominated by the knowledge of events and circumstances that only the most brazen attitude of science could so completely disregard.

The scientific community starts its annals with Newton, paying some homage to Copernicus, Kepler and Galileo, unaware that the great ones of the sixteenth and seventeenth centuries searched through classical authors of antiquity for their great discoveries. Did not Copernicus strike out the name of Aristarchus of Samos from the introduction to *De Revolutionibus* before he signed imprimatur on his work? Did not Tycho Brahe find the compromising theory of the Sun revolving around the Earth—but Mercury and Venus circling around the Sun—in Heracleides of Pontus, yet announce it as his own? Did not Galileo read of the equal velocity of heavy and light falling bodies in Lucretius;¹ did not Newton read in Plutarch of the Moon removed from the Earth by fifty-six terrestrial radii and impelled by gravitation to circle around the Earth,² the basic

¹ Lucretius, *On the Nature of Things*, translated by C. Bailey (Oxford, 1924; earlier ed., 1910) Bk. II, lines 23ff.: “For all things that fall through the water and thin air, these things must need quicken their fall in proportion to their weights, just because the body of water and the thin nature of air cannot check each thing equally, but give place more quickly when overcome by heavier bodies. But, on the other hand, the empty void cannot on any side, at any time, support anything, but rather, as its own nature desires, it continues to give place; wherefore all things must needs be borne on through the calm void, moving at equal rate with unequal weights.”

² Plutarch, *Of the face appearing in the orb of the Moon*, translated by W. Goodwin, (Boston, 1898) 246f. “They who place the moon lowest say that her distance from us contains six and fifty of the earth’s semi-diameters, that is, that she is six and fifty times as far from us as we are from the centre of the earth; which is forty thousand stadia, according to those that make their computation moderately. Therefore the sun is above forty millions and three hundred thousand stadia distant from the moon; so far is she from the sun by reason of gravity, and so near does she approach to the earth. So that if substances are to be distinguished by places, the portion and region of the earth

postulate of Newton's *Principia*, and did not Halley read in Pliny about comets returning on their orbits?³ Then why does modern science disregard the persistent reports of events witnessed and recorded in many languages in the writings of the ancients and also transmitted from generation to generation by communities unable to write, by American Indians, by the people of Lapland, the Voguls of Siberia, the aborigines of tropical Africa, the Tahitians in the South Pacific?

Why is theomachy the central theme of all cosmogonical myths? Should not a thinking man pause and wonder why the ancients in both hemispheres worshipped planetary gods; why temples were erected to them, and some are still standing; why sacrifices, even human sacrifices, were brought to them? Why was Saturn or Cronos or Brahma the supreme deity to be replaced by Jupiter of the Romans, Zeus of the Greeks, Ormuzd of the Iranians, Marduk of the Babylonians, Shiva of the Hindus, Ammon of the Egyptians? Why did the planet Venus—Ishtar, Athene, Kukulcan of the Mayas or Quetzalcohuatl of the Toltecs—become the feared deity, as I saw it omnipresent in Yucatan, where I savored a few days this February, writing this paper? Why is this Morning Star shown in sculpture as a feathered serpent on the grandiose monuments of Uxmal and Chichen Itza, where temples were built, one upon the other, if not to commemorate the ages, the last of which was dominated by Huitzilopochtli, Ares of the Greeks, who protected the people of Troy, while Athene clashed with him protecting the Achaean host?

Why was Mars of the Romans chosen as the protector of Rome, the greatest empire after the Empire of Heaven (Livy), while Athene gave her name to the capital of Attica, as Tanis to Tunisia? Why were human sacrifices brought in this country by the Pawnee Indians only a few scores of years ago, every fifty-two years connected with the Venus calendar? Why did the Ancient Assyrians mark on tens of thousands of clay tablets, free from any mythological theme, astronomical observations, but all data from before -687 are in contradiction to known values such as the duration of the daily rotation of the Earth, the time of the vernal equinox—that by the way was repeatedly transferred, as was also the beginning of the year—the ratio of the longest and shortest days of the year, the length of the month and of the year and the motion of the planets? The legends and myths clearly point to an astral origin of all ancient religions.

The problem that occupied the minds of the Classicists, Meso-american scholars. Orientalists, and students of social anthropology and mythology, was not solved in any one of these disciplines separately. Like the early memory of a single man, so the early memory of the human race belongs into the domain of the student of psychology. Only a philosophically and historically, but also analytically trained mind can see in the mythological subjects their true content—a mind that learned in long years of exercise to understand the dreams and phantasies of his fellow man.

Thus I entered a field that should be at the basis of the natural sciences, not only of the human soul and of racial memories, and soon I observed that the divisions in science are but artificial. I

challenges to itself the moon, which by reason of neighborhood and proximity, has the right to be reputed and reckoned among the terrestrial natures of bodies." Cf. Isaac Newton, *Mathematical Principles of Natural Philosophy*, translated by A. Motte, 1729, revised by F. Cajori, Berkeley, 1946. Book III: The System of the World. Proposition IV, Theorem IV, p. 407: The mean distance of the moon from the earth in syzygies in semi-diameters of the earth is, acc. to Ptolemy and most astronomers, 59; acc. to Vendelin and Huggins, 60... and to Tycho, 56½..."

³ Pliny, *Natural History*, II. 23. "Some person may suppose that these stars [comets] are permanent and that they move through their proper orbits, but that they are only visible when they recede from the sun..."

had to cross barriers. How could I do otherwise? Upon the realization that we are unaware of the most fateful events in human history, I had before me the task of explaining this well-known phenomenon of repression, the realization of which could also become crucial to the survival of the victim of amnesia playing with thermonuclear weapons. But before that I had the task of confronting the humanistic heritage with the message of stones and bones—do geology and paleontology carry the same testimony? I went again from shelf to shelf, once more around the Earth, and the record from the bottom of the sea and from the top of the mountains, from the deserts, jungles, tundras, lakes, rivers and waterfalls, told the same story—documented in every latitude and in every longitude. This evidence is presented in *Earth in Upheaval*, which I kept free from any bit of testimony that can be classified as human heritage. The scenes of devastation, mass extinction of many species in circumstances that are by far in excess of what can be considered as local catastrophe, the simultaneous change of climate all over the globe thirty-four and twenty-seven centuries ago, the drop of the level of the ocean and many other phenomena observed, could not be accounted for but by paroxysms in which the entire Earth was involved.

A psychological situation provoked the change in the attitude of the scholarly world with the beginning of the Victorian age. The founders of the sciences of geology—Buckland, Sedgwick, and Murchinson (who gave the classification of formations used today); of vertebrate paleontology—Cuvier; and of ichthyology—Louis Agassiz—never doubted that what they observed was the result of repeated cataclysms in which the entire globe partook. Actually, Charles Darwin, observing the destruction of fauna in South America, was convinced that nothing less than the shaking of the entire frame of the Earth could account for what he saw. But the introduction of the principle of uniformitarianism by Charles Lyell, a lawyer who never had field experience, and the acceptance of it on faith by Charles Darwin, are a psychological phenomenon that I observed again and again. Exactly those who, like Darwin, witnessed the omnipresent shambles of an overwhelming fury of devastation on a continental scale, became the staunchest defenders of the principle of uniformitarianism, that became not just a law, but a principle that grew to a statute of faith in the natural sciences, as if the reasoning that what we do not observe in our time could not have happened in the past can in any measure claim to be philosophically or scientifically true.

Obviously, a motive is at play that makes appear as scientific principle what is but wishful thinking. For over a century after Copernicus man did not wish to believe that he lives on an Earth that travels, and Francis Bacon and William Shakespeare were not persuaded by that firebrand, Giordano Bruno, of the truth of the Copernican doctrine. Even much less man wishes to face the fact that he travels on a rock in space on a path that proved to be accident-prone. The victory of Darwin's evolution by natural selection over a six-day creation less than six thousand years ago made it appear that evolution, the only instrument of which is competition, is the ultimate truth. But by *competition* for survival or for means of existence, never could such different forms as man and an insect with many legs evolve from the same unicellular form, not even in the six billion years that replaced the biblical six thousand. Mutations were necessary, and today we know that by cosmic and x-rays, by thermal and chemical means—conditions brought about in the catastrophes of the past—massive mutations can be achieved.

The pre-1950 astronomy followed the same pseudo-scientific statute of faith, elevated to a fundamental principle, and made believe that the Earth and other planets travel the same paths for

the same six billion years, always repeating the same serene circling. Against this violation of the principle of empiricism in science stood my work. In it I rejected the postulate that the ancients, the Greek philosophers Pythagoras, Heraclitus, Democritus and Plato included (O. Neugebauer in *The Exact Sciences in Antiquity* wonders why Plato is considered anywhere a philosopher of any rank⁴) were childish in their claims of repeated world conflagrations, and that the ancients were almost imbeciles in their beliefs. The ancients, the canard goes, believed in the Earth placed on the back of a tortoise. Thus it is preferred to start science three hundred years ago, and my work was pronounced (by those who did not read it) as an act of destruction of the entire edifice of science erected by the giants of science since Copernicus.

I offered a series of claims that naturally followed from the reconstruction. In science they are usually called predictions, but I prefer to term them advance claims. Thus I claimed that Venus, due to its recent birth and dramatic though short history, must be very hot under the clouds, nearly incandescent, and gives off heat—it has not reached thermal balance; that it must have every massive atmosphere; that the atmosphere consisted largely of hydrocarbons but that if oxygen is present petroleum fires must be burning—thus explaining also the present massive carbon dioxide content of the atmosphere; that sulfur and iron (ferruginous pigment) must be present too; and that if the same catalytic process that took place on the Earth when it was enveloped by clouds of Venus' origin takes place in Venus' own clouds, they must consist mainly of organic material infused with sulfur and iron molecules. Further, I considered that Venus was disturbed in its rotation.

Venus was found over 750°K. hot—many metals are incandescent at this temperature—while the consensus of opinion among astronomers was 17°C., 3° above the mean annual temperature on Earth. Venus was found rotating slowly and retrogradely. The atmosphere was found very massive, 95 terrestrial pressures near the ground surface, and not reckoning with this possibility, the first Venera probes were crushed. The content of the clouds is still unsolved, but in a paper in the Winter, 1973-74 issue of *Pensée*, a journal dedicated to the reconsideration of my views, I elucidated that the spectral features in the ultraviolet, near infrared, infrared and deep infrared can be accounted for by organic matter, and so can the volatility and the index of refraction. Nitrogen gas, expected by all specialists to comprise as much as 90% of the atmosphere, was not found. The enigma of the very rich content of carbon dioxide below the clouds is solved if the combustion of hydrocarbons took and still takes place. I expect that the Venus Mariner X probe of this month will bring us nearer to properly evaluating the content of Venus' clouds. But the preliminary report already says that “the manner in which that planet was born and matured differed basically from that of Earth.” An editorial in the *New York Times*, commenting on the bands and streaks first discovered by Mariner X, spoke of an “uncanny similarity” to the bands “in the atmosphere of Jupiter.” It added that “it is a problem that poses a formidable challenge to astronomers.”

There are problems requiring study that were not discussed in *Worlds in Collision* because the origin of Venus belongs to the volumes dealing with the earlier catastrophes. How did Venus, in Latin, “the Newcomer,” escape from Jupiter four hundred times more massive?—and Lyttleton's work gives some idea; or how could Venus be so much heavier per unit of volume than Jupiter?—either it was expelled from inner parts of the giant planet, or gases like hydrogen entered into

⁴ O. Neugebauer, *The Exact Sciences in Antiquity* (Princeton University Press, 1952), p. 146.

chemical compounds of higher molecular weight. In *Worlds in Collision* I suggested that electrical discharges in the atmosphere of ammonia and methane in which Jupiter is rich, would produce hydrocarbons of heavy molecular weight—an experiment successfully performed ten years later by A. T. Wilson. Further, I envisaged fusion of elements—like oxygen to sulfur—in interplanetary discharges.

Orbiter and Surveyor probes of the Moon were followed by Apollo probes; and on the historic night of July 21, 1969, when Man stepped on the Moon, I made a series of claims in an article written at the invitation of the *New York Times*, and spelled out earlier as well in memos to the Space Science Board of the National Academy of Sciences. Strong magnetic remanence, I claimed, would be discovered in lunar rocks and lavas, though the Moon itself hardly possesses any magnetic field whatsoever. A steep thermal gradient would be found already a few feet under the surface. Thermoluminescence would disclose that the Moon was heated considerably only thousands of years ago. Hydrocarbons, preferably of aromatic structure, would be found in small quantities, but carbides, into which hydrocarbons would transform when heated, in substantial quantities; expressed radioactivity would be detected in lunar soil and rocks; and several more claims. Already following Apollo XI and XII the score was complete. But each of the discoveries—steep thermal gradient, strong remanent magnetism, recent heating of the lunar surface, carbides and traces of aromatic hydrocarbons, and rich radioactivity of the rocks and dust—evoked exclamations of surprise and at best some far fetched, ad hoc hypotheses. Magnetic anomalies, especially where interplanetary bolts fell, and huge enclaves of neon and argon 40 in lunar rocks, were also claimed by me in advance of the findings.

The Mars probes disclosed, as I had claimed in *Worlds in Collision*, a dead planet that went through enormous cataclysmic events, not unlike the Moon. The “canali” proved to be not the product of intelligent work, but rifts caused by twisting of strata. Like on the Moon, enormous craters resulted from bubbling, but some formations, especially surrounded with “rays,” resulted, in my view, from interplanetary discharges.

When last December [1973] I was invited to address the scientists of the Langley Space Research Center that prepares the June 1976 Viking probes to Mars, I was told of the program and shown the module. I found that my 1945 copyrighted view, printed also in *Worlds in Collision*, of the possible abundant presence of argon and neon in the atmosphere of Mars, then a very far-fetched idea, is now incorporated in the program of the 1976 Viking probes. Today, in one of the alternative atmosphere models (the other has nitrogen richly presented - the same alternative I discussed in *Worlds in Collision*), NASA anticipates as much as 33.3% argon in the atmosphere, but, in my opinion, too little—666 parts per million—neon. Actually, in 1969 I saw my assumption indirectly confirmed when after I expressed my expectation of rich inclusions of argon and neon in lunar rocks, such enigmatic inclusions were found. I based my expectation on the realization that in the eighth century before the present era Mars and the Moon repeatedly came into near-contacts.

I would speculate that the red color of Mars, due mainly to the ferruginous material acquired from Venus when the latter displaced it from its orbit (in the theomachy described in great detail in the *Iliad*), may partly be due also to an electrical effect in a neon-rich Martian atmosphere. I recommended in my lecture and consultation at Langley Space

Research Center several tests not found in their program as it stands now:

To study the electrical nature of the sandstorms, occasionally reaching the velocity of one hundred to two hundred miles per hour, in the rarefied atmosphere of the planet.

To search for strong remanent magnetism of rocks and lavas, not just to photograph soil particles attractable to a magnet. As just explained, iron particles will be found in abundance. In future probes anomalous remanent magnetism will be discovered near places where electrical bolts emerged or fell.

To search for expressed radioactivity of the rocks and regolith, especially near large circular formations that resulted from interplanetary discharges.

To investigate the thermal gradient, presumably rather steep, even if only at the depth of two or three feet.

To perform a thermoluminescence experiment on glass-like particles in the Martian soil which will disclose a very recent heating of the Martian surface; if it were not for the expected radioactivity on Mars, the proper result would be twenty-seven centuries for the last heating.

The logic that led me to these conclusions and suggestions was the same that made me make similar advance claims concerning the Moon before the lunar landings.

I understand that the program will be dominated by an effort to find out whether there is or there was life on Mars; organic materials will be searched for and I count with the possibility that traces of hydrocarbons may be found in the Martian soil, but almost all hydrocarbons must have turned into carbide rocks by heating; cultures of possible micro-organisms will be investigated for changes in color and for the production of gases.

In *Worlds in Collision* I compiled descriptions from many sources of a widely spread pestilence that accompanied Mars' close approaches; it is not excluded that Mars is richly populated by micro-organisms pathogenic to man. I suggested an inclusion of a microscope in the equipment of Viking and, if possible, of an electron microscope for the study of viruses. I do not discount the probability that the seasonal changes in the color of the Martian surface may be due to seasonal microbial or other low vegetative activity.

It is preferable to postpone the second Viking probe, now planned as identical with the first and following it by one month, in order to rework the program and to include the instruments needed for the test I enumerated.

When earlier, a year and a half ago, in August [1972], I was invited to lecture and consult at Ames Space Research Center (Division of Exobiology), I suggested also that microbial life able to catalyze can possibly be found in Venus' clouds, lower forms of insect life on Jupiter, and primitive plant life on Saturn, besides what I said now of Mars. So much for cosmology and also the evolution of life.

If I was completely at odds with the cosmogony that had the solar system without history since creation, I was also carrying my heresy into a most sacred field, the holy of holies of science—celestial mechanics. I had a chapter on the subject at the end of *Worlds in Collision*, but I kept those galleys from inclusion in the book and instead I included only one or two paragraphs—and the only italicized words in the book are found in them—namely: ‘The accepted celestial mechanics, notwithstanding the many calculations that have been carried out to many decimal places, or verified by celestial motions, stands only; the sun, the source of light, warmth, and other radiation produced by fusion and fission of atoms, *is as a whole an electrically neutral body*, and also if the planets, in their usual orbits, are neutral bodies.’ I showed how the events I reconstructed could have occurred in the frame of the classical celestial mechanics, but coming from the field of studying the working of the brain—I was the first to claim that electrical disturbances lie at the basis of epileptic seizures—I was greatly surprised to find that astronomy, the queen of sciences, lives still in the pre-Faraday age, not even in the time of kerosene lamps, but of candles and oil. It was, of course, known since Gilbert that the Earth is a magnet, and G. E. Hale discovered that solar spots are magnetic and that the Sun possesses a general magnetic field. But this did not keep Einstein, a few years later, from accounting for the Mercurial precession by a new principle instead of first eliminating the effect of the newly discovered solar magnetic field on Mercury’s movement.

I claimed the existence of a magnetosphere above the terrestrial ionosphere - it was discovered by Van Allen in 1958; I claimed that this magnetosphere reaches as far as the lunar orbit—it was discovered by Ness in 1964; I claimed that the interplanetary space is magnetic and the field centers on the Sun and rotates with it—it was discovered in 1960 by simultaneous observation of Pioneer V and Explorer X, one travelling around the Sun and the other around the Earth; I claimed that Jupiter sends out radio noises,⁵ and actually offered in writing in June 1954 to Albert Einstein to stake our protracted debate as to whether, besides inertia and gravitation, electromagnetic interactions participate in celestial mechanics: Does or does not Jupiter send out radio noises?—and Einstein wrote his note of disbelief on the margin of my letter. But on the 8th of April, 1955, nine days before his death, I brought to him the news that Jupiter noises were discovered by chance; those who detected them for long weeks disbelieved their find and the Jovian origin of the noises.

Lately I lecture frequently for physical and engineering societies and faculties, and I challenge those in the audience who believe that a magnetic body can move through a magnetic field without being affected by it to lift their hands. Can Jupiter with its immense magnetosphere move in the magnetic field centered on the Sun, if only of a few gammas, without being affected by it? Can the satellites of Jupiter plow through the magnetosphere of the giant planet without being affected by it? On no occasion I saw a hand raised.

Only a few weeks ago, preliminary reports in *Science* on the Pioneer X December flyby recorded a series of unusual electromagnetic phenomena involving Jupiter and its satellites. At about the same time we read of radio noises for the first time detected from a comet, as Kohoutek was approaching its perihelion. (Incidentally, contrary to the unanimous opinion expressed by astronomical authorities, with which I disagreed, Kohoutek did not develop into the greatest

⁵ I. Velikovsky, “On the Advance Claim of Jupiter’s Radionoses,” *Kronos* III.:1 (Aug., 1977), pp. 27-30.

celestial spectacle of the century.) The role of electromagnetic interaction between a comet and the Sun was another subject of my detailed discussion, oral and written, with Einstein.

With the discovery of quasars, magnetic binaries, black holes and colliding galaxies sending out agonized radio signals, the electromagnetic nature of the universe is no more in question. Space is not empty either. I feel like calling René Descartes from the Land of Shades to present his appeal, because as late as 1949, a year before the publication of *Worlds in Collision*, the verdict was, according to the philosopher Butterfield, that “The clean and comparatively empty Newtonian skies ultimately carried the day against a Cartesian universe packed with matter and agitated with whirlpools, for the existence of which scientific observation provided no evidence.”

But ten years later we read: “Gone forever is any earthbound notion of space as a serene thoroughfare . . . a fantastic amount of cosmic traffic (hot gaseous clouds, deadly rays, bands of electricity) rushes by at high speed, circles, crisscrosses, and collides.”

How could I produce this score of correct prognostications? Professor V. Eshleman of the Jet Propulsion Laboratory, obviously astounded, wrote on September 11, 1970, to a news-writer— “I am completely mystified as to how Velikovsky reaches his conclusions. It is almost as though he does it through will power alone. . . .” But could I, by will power alone, initiate Jupiter’s noises?

There is no mystery. My advance claims are a “natural fallout from a single central idea,” in the words of one student of the affair. Reading of my work is a prerequisite for understanding the way I reach my conclusions.

Yet not a few upheld the scientific method by absolving themselves from reading the book they discuss and occasionally suppress. These days one planetarium astronomer authoritatively pronounced my score of correct predictions as compatible with the law of averages and added that I would have been unfortunate if my score were any less. Seven years earlier the same planetarium astronomer was the mastermind in the refusal of the Franklin Institute in Philadelphia to permit the oldest astronomical association of America, the Rittenhouse Society, to convene at their traditional meeting place in the Institute when they invited me to address their members—a story that had many reverberations.

The behavior of the scientific community was and partly still is a psychological phenomenon. The spectacle of the scientific establishment going through all the paces of self degradation has nothing with which to compare in the past, though every time a new leaf in science was turned over there was a minor storm, and it is not without precedent that most authoritative voices in science usually served to discourage the trail blazers—think of Lord Kelvin, unsurpassed authority of later Victorian days, who rejected Clerk Maxwell’s electromagnetic theory, demeaned Guglielmo Marconi’s radiotelegraphy, and till his death in 1907 proclaimed Wilhelm Konrad Roentgen for a charlatan.

But it is without precedent that the entire scientific community should be aroused to very base actions of compelling, by organized boycott, the publisher of a book checked and rechecked before the printing to discontinue its publication, to destroy the entire stock, and to punish the editor of twenty-five years service by dismissal. This community offered a united front of academic and scientific societies, of faculties, of scientific and semi-scientific press against a

solitary figure whose only iniquity was to present views carefully arrived at in more than a decade of work, supplied with all references to enable the reader to check multitudinous sources, with never a jest or a harsh word against those with whom the non-conformist disagreed, with no new terms introduced, in lucid language, though foreign to me, never given to misunderstanding.

Now, after twenty-four years, and more than seventy-two printings in the English language alone, forty of which were in hard cover, my *Worlds in Collision*, as well as *Earth in Upheaval*, do not require any revisions, whereas all books on terrestrial and celestial sciences of 1950 need complete rewriting. The opposition and the indecent forms it took are a psychological phenomenon and cannot be explained by a mere desire to protect the vested interests. The forms the suppression assumed are so multiple and sometimes ingenious, but mostly crassly rough and often dishonest, that only having been trained in recognizing various forms of resistance with which analytical patients react when unwelcome truth is about to reveal itself, could I understand the unique spectacle which I observe now for a full generation.

If a sociologist endeavors to divide the guilt between the establishment and the non-conformist, and claims neutrality, then he did not learn to discern objectivity from neutrality. And if a professor of astronomy puts passages in my book which are not there and then makes the class of tuition-paying students roar by attacking those passages, this roar may still sound in his ears when there will be no merriment in it. In these antics, an experienced psychoanalyst recognizes a state of anxiety. “We are shaking in our shoes—but with laughter” wrote an early critic, Cecilia Payne-Gaposchkin of Harvard. Actually the astronomers of that university must have felt *threatened by the book* and even an entire generation later, acting as if in peril, a Nobel prize winner wrote to a high school girl to close *Worlds in Collision* and not to open it again in her lifetime, only to admit three years later to the editor of *Pensée* that he never himself read the book. Those who act almost suicidal should keep their fingers on the pulse of time.

In the behavior of the scientific establishment the desperate resistance that bedevils human society found its expression. As members of the human race, we are afraid to face our past. But as Santayana wrote, those who do not remember the past are condemned to repeat it and—this time, I am afraid, in a man-made thermonuclear holocaust.

My work today is no longer heretical. Most of it is incorporated in textbooks and it does not matter whether credit is properly assigned. My work is not concluded—I only opened new vistas. The young and the imaginative flock in an ever increasing stream. Numerous colleges and universities in this country hold courses or seminars on my work, include my books among the required readings and have theses on my ideas written for graduate degrees. Those who stopped thinking since graduating will claim authority, soon to find that they are left without a following. I may have even caused retardation in the development of science by making some opponents cling to their unacceptable views only because such views may contradict Velikovsky - like sticking to the completely unsupportable hypothesis of greenhouse effect as the cause of Venus' heat, even in violation of the Second Law of Thermodynamics.

This spring, besides this Symposium on my work, two more international symposia dedicated to the subject will take place without my having any part in initiating them. Those who prefer name calling to argument, wit to deliberation, or those who point a triumphant finger at some detail that they misinterpret, yet claim that my entire work ought to collapse, and boast of their own

exclusiveness as a caste of specialists—as if I claimed omniscience and infallibility and as if I wrote a sacred book that falls due to some possible error—are not first in their art. I shall quote Giordano Bruno, and one of the organizers of this symposium, Professor Owen Gingerich, Harvard's historian of science, is well familiar with Bruno's description of how his contemporaries used to conduct a dispute:

“With a sneer, a smile, a certain discrete malice, that which they have not succeeded in proving by argument—nor indeed can it be understood by themselves—nevertheless by these tricks of courteous disdain they pretend to have proven, endeavouring not only to conceal their own patently obvious ignorance but to cast it on to the back of their adversary. For they dispute not in order to find or even to seek Truth, but for victory, and to appear the more learned and strenuous upholders of a contrary opinion. Such persons should be avoided by all who have not a good breastplate of patience.”

After all, it really does not matter so much what Velikovsky's role is in the scientific revolution that goes now across all fields from astronomy with emphasis on charges, plasmas and fields, to zoology with its study of violence in man. But this symposium in the frame of the AAAS is, I hope, a retarded recognition that by name-calling instead of testing, by jest instead of reading and meditating, nothing is achieved. None of my critics can erase the magnetosphere, nobody can stop the noises of Jupiter, nobody can cool off Venus, and nobody can change a single sentence in my books.

“Velikovsky’s Challenge to Science”

A Symposium held by the American Association for the
Advancement of Science
on February 25, 1974 in San Francisco

Transcripts of the Morning and Evening Sessions, transcribed and edited by Lynn
E. Rose.

“My Challenge to Conventional Views in Science,” Velikovsky’s keynote address.

“Afterword,” a concise reply by Velikovsky to his critics, originally intended for
the planned symposium publication.

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 benefited about equally from this. In no case have any of the 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 the 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.S.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 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 Bear 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 on 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 the 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 is 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 to 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 the 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 to also 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 pull 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 so, many years ago. And I must blushingly admit that he has put a finger on a weak point in my statement, because what I have 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 capital. And to make it precise, what I mean is, if you take the probably most reliable eclipse we have now from antiquity, it's the 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 capital 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 use, *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 there 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 the 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 the field of statistic, 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 a 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 [the] 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 are so many days. You have the way to check, because if from fifteen of 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 Fortheringham 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 Fortheringham in Langdon-Fortheringham-Schoch. That’s one comment.

The second comment, you said something about Venus joining the ranks 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—Now, “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.

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.

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 Amizaduga 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 tables were a little earlier than Schiaparelli thought, but not much earlier. Certainly they are not of the time from the First Babylonian Dynasty. It would make no difference for the thesis that the 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 Incas 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 Amizaduga 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 [the] 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 a 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 books 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 Burghstahler 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 [a] number of things, and all of them went into fulfillment.

I claimed about Venus that it would 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 found 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 sulfuric 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 Burghstahler. 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 Burghstahler, 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 of 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 is 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 this 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 the 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 xii + 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 allowed, will not be available, will not 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 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 is 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 We. 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 you 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. Arnold 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 the 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, 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, the represent an argument from authority. There was not a single substantive point in your question. It was all, "Have your read X, Y, X, 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 éminence 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 has 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 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. These 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 other 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 in calculated [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 in calculate [that is, calculate them in]. They still go by pre-Faraday astronomy. Of course, Newton was not to blame. Evening 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 not have spoke 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, though 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 fabulation of folklore, that the story as it is need to be searched from one place to another place.

And though 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 deal 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 differently, but the theme is always the same.

Now, again, to put into my book [the] story that frogs were falling from the sky—not in his 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 it 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 wrote 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 stores 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 larvae 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 has 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 stated 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 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's 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? There is a possibility—

VELIKOVSKY:

Possible existence of hydrocarbons—

SAGAN:

How much?

VELIKOVSKY:

—in the lower atmosphere.

SAGAN:

How much?

VELIKOVSKY:

Not a 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 the 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 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 not 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 has made, and then I'll be glad to hear the rest of this discussion and, if I can, try to respond to that.

In his response thus 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 has 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 deductions 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 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 embellishments that do 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 person 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 are 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 portion 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 it seems to me to pose serious problems with Velikovsky’s hypothesis. How 1010 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 1010 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 has 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 as 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 the 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 king 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 less 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 to go to print soon, [it] will have all of 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. Though 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 brining 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 [of], 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 this 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 claims 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 these 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 at 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 Bearn 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 1023 ergs results in less than 1015 ergs at the Earth. Thank you.

AFTERWORD

This essay was completed early in April of 1976, for inclusion in the A.A.A.S.-Cornell volume Scientists Confront Velikovsky, and it is to the latter that such expressions as “this volume” refer. After writing the “Afterword” in accordance with the limitations of space and time laid down by the A.A.A.S.-Cornell people, Velikovsky decided that he would not participate any further in their volume, because of their unfair and restrictive requirements, and because of their long record of broken promises and rigged arrangements. -The Eds.]

It has been claimed in print that I “objected” to efforts “to publish the proceedings” of the Symposium (Owen Gingerich, “Science Year Close-Up”, *World Book Encyclopedia Supplement*, 1975, page 249), and also that at the Symposium “Velikovsky was clearly shown in error, which perhaps explains why he has seen fit to prevent the publication of the transcript of the meeting” (Reader's Forum, *Mercury: Journal of the Astronomical Society of the Pacific*, November/December, 1975, page 35).

None of this is true.

The tape recording of the Symposium shows that I effectively and forcefully answered my opponents. I have urged, since even before the Symposium was held, that the papers as delivered and the discussions as spoken should be quickly published, with minimal editing, or, better, with no editing at all, so that there would be a full and accurate historical record of what transpired. This record could have been supplemented by additional papers by each of the panelists, but any such new material should have been clearly designated as supplementary to the historical record of what was said on February 25, 1974.

This plan was not accepted, however, and it was decided by others that Professor Carl Sagan, one of the panelists at the Symposium, could revise his paper prior to publication. My own paper was intended to be printed exactly in the form in which it was distributed on February 25, 1974. Hence the delay in the preparation of materials for this volume has not been due to me, despite what Gingerich and others are saying. The principal delay in the preparation of material for this volume has been due to Sagan, who consumed nearly two years (late February 1974 to early February 1976) in revising and greatly expanding his paper. There are well over thirty pages of new material in the 1976 paper, I could not be expected to have prepared answers to additional arguments and assertions that I had never even seen until February 1976.

By the decree of the organizers of this volume, I am limited to six thousand words or about twenty typewritten pages to answer the papers of the panelists at the Symposium that was held on my work. I found such a limitation unfair, but I agreed, lest my rejection be branded as a refusal to meet my opponents on printed pages.

To “equalize,” each of the other panelists is offered three thousand words to add his counterarguments. The reader will excuse me if I do not answer these counterarguments here: I will not be permitted to see them before the volume is printed.

I will omit here the story of how the Symposium came to be; but the original offer to have equal numbers of panelists on opposing sides was not carried out; the chairman, having come under pressure from the higher echelons of the establishment for the very idea, wished to publish an advance statement, making known that no pursuit of scientific debate was really in mind, and he also made an opening statement at the Symposium itself on that score, thus subscribing to a bias in advance of the debate. With a biased chairman, with an unbalanced panel, and with the papers of those panelists attacking my theories not sent to me in advance of the seven-hour debate, I, in the opinion of the fourteen hundred in the audience, fared well — as indicated by the standing ovation that was accorded only to the iconoclast. But the scientific and semi-scientific press showed by its reports that it was orchestrated - the very sentences, and the very same errors of fact and number, appeared simultaneously in many reviews.

The press in a chorus singled out and repeated the “overkill” by Sagan in his paper of 57 typewritten pages. In the oral confrontation at the morning session he read only part of his paper; it was anything but an overkill — and the audience reacted accordingly. The organizers permitted him to work on his paper for two additional years, and what appears in this volume has grown to 87 typewritten pages submitted in February 1976 as a paper read in February 1974. This, too, I protested, but I decided that any strictures and retreats from an unbiased position, to which they were obligated, will, in the final count, not serve their interest, which is to do what the previous generation could not — to stamp out the heresy, by stampeding the media.

A careless reader of the papers of the panelists will come away with the feeling that the basic tenets of my work have been pulled apart. The careful reader, however, will observe that my opponents retreated on all major fronts, and even surrendered the cherished arguments that were much used in the past and also used even more recently against my work. *In this itself is a measure of victory.*

MULHOLLAND

Two astronomers confronted me on the panel: Professor J. Derral Mulholland, whose field is celestial mechanics, and Professor Sagan, whose field is planetary atmospheres, rock compositions, and biological conditions. Mulholland said:

... the celestial mechanics of Newton and Newcomb are no longer the ultimate measure. The celestial mechanics of 1974 is a living, vital science that admits of non-gravitational effects, of electromagnetic interactions. . . .

This is a retreat of unprecedented significance. My opponents of the 1950's would not permit the smallest concession regarding their dictum that only gravitation and inertia account for celestial motions. They put themselves on record in print.

Celestial near-collisions *could have happened*, according to Mulholland, who here sheds the uniformitarian dogmas. Giant tides, global earthquakes, changes of the direction of the celestial axis would have resulted. “There is no faith here; these are unavoidable consequences of the laws of motion” .

But whether all this happened depends on historical and archeological evidence that can be presented. He criticizes a few single cases like the case of Babylon that in the past changed its position in relation to the pole by several degrees. But my sources were Johann Kepler, who quotes Arabian geographers, and historians going back to Ptolemy. Every other change, like the changing ratio of the shortest to the longest day in Babylon and Egypt, I also discussed in some detail.

Mulholland asks: “Does Velikovsky's evidence provide reasonable proof that the axis shifted abruptly and catastrophically 27 centuries ago?” His verdict is in two words: “Absolutely not” .

No careful reader of *Worlds in Collision* would agree. All of the evidence from Greek, Roman, Babylonian, Sumerian, Chinese, Hindu, Hebrew, Egyptian, Mayan, and Toltec civilizations are dismissed in this casual fashion, hundreds of pages, thousands of references. There are also several chapters in *Earth in Upheaval* dealing with evidence of the changing position of the terrestrial axis.

Mulholland, who on the first page of his paper refers to how both Venus and Mars “erupted into the sky” as “two giant comets” , cannot be counted among careful readers. He did not refer to *Earth in Upheaval*; and all the evidence from deserts, polar lands, jungles, once habitable countries, are also disposed of in this two-word verdict, though he agrees and stresses that “if a planet-sized object were to pass close by the Earth” , the consequences would be the very events described in *Worlds in Collision*.

I thank Mulholland for saying “Velikovsky's challenge is not one to be decided on a basis of belief or unbelief.” Mulholland continues:

“He strives to build physically plausible solutions that involve testable ideas. He is not a mystic.” — and this when some prominent scientists announced that I belong in one group with palmists, astrologers, or believers in a flat earth. But when Mulholland says that the rotation of Mars refutes the theory of recent catastrophism, he has grasped a weak reed — is it not agreed that Mars lost a major portion of its rotational angular momentum?

STOKER

Professor Norman Storer was selected by the organizers of the Symposium to represent the sociological aspect of the Velikovsky controversy. He concluded that it would be a sign of objectivity to divide equally between myself and the establishment the guilt for what happened. Storer made the fundamental error of confusing *neutrality* with objectivity, and his efforts failed, as I pointed out in the evening discussion: one who maintains “neutrality” between a gross offender and the victim of the offense does not give an objective account of the realities; the account is biased in favor of the offender. Storer's paper is a whitewash of the offenses of the establishment.

Storer's was the only paper sent to me before the Symposium.

HUBER

Dr. Peter Huber is a professor of statistics (on the program he was billed as a professor of ancient history), his Assyriology being, as he told me, his hobby. Huber tried to bring out of one or two sources of ancient material 1) that the solar system was stable through historical times (since a certain solar eclipse could prove it), and 2) that Venus was observed in the sky earlier than the first near encounter of the protoplanet Venus with the Earth.

To the first point it would suffice to cite the opening paragraph of van der Waerden's article of 1951 (*Journal of Near Eastern Studies*, vol. X, p. 20). In the Assyro-Babylonian calendar of about -700 the vernal equinox was transferred by more than a month. Also the ratio of the day to the night at the summer solstice seems to have changed from 2:1 to 3:2. Similar changes took place in Egypt at the same time.

And of what value are reports of phenomena from Syria or the Far East that are now supposed to be eclipses, when a plurality of scholars long acquainted with the problem discount such claims or interpretations? In my debate with Princeton astronomer John Q. Stewart, in *Harper's*, June 1951, Stewart based himself on an article by Fotheringham concerning three historical "eclipses". In 1974 Huber declared me right, and Fotheringham followed by Stewart he declared wrong.

As to whether Venus had been seen before -1450, Huber refers to the so-called Ammizaduga tablets (Schiaparelli refers them not to Ammizaduga but to the seventh century). 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 months must be changed; the dates of the month must be changed; the intervals between disappearance and reappearance of Venus must be lengthened or shortened — all in order to prove his point that Venus moved then as it moves now. My own understanding of the Venus tablets does not require a thirty percent change of data — probably not even one datum — and the tablets show only that Venus did not travel on the orbit it travels now. (See *Worlds in Collision*, p. 198 ff.)

Huber also quotes a Sumerian text (its being in Sumerian does not attest its antiquity; like Latin, Sumerian was used for sacerdotal purposes for many centuries after the Sumerian civilization went down in destruction, leaving hymns and prayers to the feared planetary gods); and Venus in the text quoted by Huber is compared in its brilliance to the Sun itself.

What is more: in an earlier prepared review, to explain a series of facts otherwise unexplainable, Huber expressed the surmise that the solar system may have been visited or invaded by a new planet that caused havoc in nature and awe in man: "highly improbable, but *not to be excluded* capture of a rather large foreign heavenly body into the solar system in *historical* time. . . ." (My italics.)

SAGAN

As my opponent for the fourth tournament, the astronomical establishment selected Sagan. To answer his nearly 90 pages and nearly 30,000 words (1976 version), I am left with barely one-tenth of that amount, though an answer usually requires more space than an accusation, especially those that are bland and unsupported: I must first state what the charge was, then state what the truth is, what I really wrote, etc., and then present the evidence for what I said. In the 1974 version of his paper, Sagan had twice mentioned both the letter of Bargmann and Motz and the

letter of Hess, but in the 1976 version all such references have been deleted, even from the Bibliography. These deletions cannot have been in the interest of saving space, for Sagan allowed the length of his paper to grow by more than fifty percent. In their letter published in *Science* (December 21, 1962), Professor V. Bargmann, Department of Physics, Princeton University, and Professor Lloyd Motz, Department of Astronomy, Columbia University, called attention to the originality and to the correctness of my predictions of radio noises from Jupiter and of a very high temperature of Venus (they also mentioned my prediction of the existence of the terrestrial magnetosphere). Bargmann and Motz conclude: "Although we disagree with Velikovsky's theories, we feel impelled to make this statement to establish Velikovsky's priority of prediction of these two points and to urge, in view of these prognostications, that his other conclusions be objectively re-examined." In his open letter to me on March 15, 1963, Professor H. H. Hess, Chairman of the Geology Department, Princeton University, wrote: "You have after all predicted that Jupiter would be a source of radio noise, that Venus would have a high surface temperature, that the sun and bodies of the solar system would have large electrical charges and several other such predictions. 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 been proven to be false." The deletion of his earlier references to Bargmann and Motz and to Hess seems to be a part of Sagan's program to deny me credit for my record of correctness and originality.

In the two years during which his paper was brought into shape, Sagan was helped by such authorities as Thomas Gold and Philip Morrison among others, and therefore I am in the position of standing against the entire establishment, though greatly limited as to space and time, and blindfolded as to any additional counterarguments my opponents may bring, before I see the printed book. Unjust as such conditions may be for a scientific debate, I am not abandoning the project and will do my best under the circumstances, to the limits of what decency can tolerate, though my friends, also in positions of moral standing in the community of the scholarly world, advised me to abandon the project and if necessary give a complete and unbiased account with the help of several collaborators.

Sagan may mislead the reader by professing, in opening his paper, high principles and even magnanimity (benefit of doubt going to me); he declares also that no physical laws are inviolate if facts of experience or of experiment oppose them - by this echoing the words in the Preface of *Worlds in Collision*. He actually admits that all vituperations of his guild in the past 26 years were not supported by sound argument. Sagan concludes that a planet *could* have escaped from Jupiter; that a disturbance of rotation of the Earth *could* have happened; that the terrestrial axis *could* have changed its direction; that a bringing of the Earth to a rotational stasis even in less than one hour, would hardly be noticed by human beings and they certainly would not fly off into space (what he himself asserted a few years ago and what his friend Asimov still asserts); nor even would stalactites break off (Asimov's foremost argument till today).

Sagan makes more fundamental concessions; and above all, agrees that changes in the order of the solar system *could and must* have taken place. This last general statement was the only thing I communicated to Harlow Shapley that started the campaign of suppression which is not over even today.

All these basic statements were used by the generation of Shapley and Payne-Gaposchkin against my work. Sagan also is contemptuous of 25 years (1950-1975) argumentation of his guild: “I am surprised at how little of it there is.” “There is nothing absurd in the possibility of cosmic collisions.” “Collisions and catastrophism are part and parcel of modern astronomy” — and so already for centuries. There is nothing unorthodox about the idea of cosmic catastrophes, says Sagan. Then why was I, and my work, vilified for a quarter of a century?

Fortresses having been surrendered, makeshift fortifications are being raised. “What then is all the furor about?” — It is the time scale and the written ancient evidence. Sagan admits “I find the concatenation of legends which Velikovsky has accumulated stunning. . . .” From here starts the assault. Sagan follows me into many areas and cuts many Gordian knots.

Throughout his paper, Sagan repeatedly stresses that I accept some parts of ancient myths and legends and not other parts, and he wonders why I do not accept either all or else nothing. He suggests that my procedure here is arbitrary or capricious. But Sagan has not troubled to understand my procedure. He complains that I accept ancient legends about manna, but that I do not accept the scriptural account that manna fell in a double portion on Fridays and not at all on Saturdays. But I accept the ancient testimonies about manna of the Hebrews, ambrosia of the Greeks, and honey-dew of other peoples from around the Earth, precisely because there is testimony on this from many peoples from many parts of the Earth and because there are physical events (the near collisions between Earth and Venus) that could have led to such results. And I reject the report about manna falling in double portions on Fridays and not at all on Saturdays, precisely because that feature of the story does not have a plausible physical basis, is not testified to by other peoples, and is therefore to be regarded as an inaccurate elaboration by one people upon what actually transpired.

Whoever read the sections in *Worlds in Collision* on ancient calendars and calendar reforms occurring from Japan to India, to Persia, to Assyria and Babylonia, to Greece, to Rome, to Israel, to Egypt, and so on, would not have been misled by the simplistic theory that 360-day years were merely convenient approximations.

Sagan moves to cave paintings (where he finds only a picture of a supernova) and to ancient art generally and asks: “If the Velikovskian catastrophes occurred, why are there no contemporary graphic records of them?” As a novice in the field, Sagan should perceive that the great majority of ancient contemporary art is *dominated* by the theme of global catastrophes and celestial planetary deities in battle. In my lecture I referred to the Mayan, Olmec, and Toltec art — and whoever visits Yucatan knows that virtually *no other theme* exists in this art. No dynastic or military exploits, but battles between planetary deities, and sacrifices to them — almost to the exclusion of other themes. The cave man pictures animals in global conflict; serpents fighting planets are a frequent theme in cave and mural art; and in literary art — from the *Iliad*, to the Assyrian prayers, to the Old Testament, its prophets and psalms, to Hindi and to Icelandic epics — it is the all-pervading motif. So it goes in this domain, which is foreign to Sagan.

Sagan writes:

Other critical statements which are given extremely inadequate justification, and which are central to one or more of Velikovsky’s major themes, are as follows: the statement (page 283)

that 'Meteorites, when entering the earth's atmosphere, make a frightful din,' when they are generally observed to be silent; the statement (page 114) that 'a thunderbolt, when striking a magnet, reverses the poles of the magnet;' the translation (page 51) of 'Barad' as meteorites; and the contention (page 85) 'as is known, Pallas was another name for Typhon.' On page 179 is enunciated a principle that when two gods are hyphenated in a joint name, it indicates an attribute of a celestial body — as, for example, Ashteroth-Kamaim, a homed Venus, which Velikovsky interprets as a crescent Venus and evidence that Venus was once close enough to the Earth to have its phases discernible to the naked eye. But what does this principle imply, for example, for the god Ammon-Ra? Did the Egyptians see the sun (Ra) as a ram (Ammon)?

The Smithsonian Institution published in 1929 a volume on *Minerals from Earth and Sky*. George P. Merrill, Head Curator, Department of Geology, U. S. National Museum, contributed "The Story of Meteorites," in which he gives a long series of reports of loud explosions accompanying the fall of meteorites. Meteorites are a subject that belongs to Sagan's own field, but he does not know that they can make noise. For example, in Emmet County, Iowa, on May 10, 1879: "The sounds produced by the explosions incidental to its [the meteor's] breaking up were referred to as terrible and indescribable. . . . The first explosion, for there were several, was louder than the loudest artillery." This is only one of a number of illustrative cases described by the Smithsonian Institution. So silent when entering the atmosphere they are not, Sagan notwithstanding.

Sagan wonders that "a thunderbolt, when striking a magnet, reverses the poles of the magnet" . This explains the reversals in paleomagnetism (*Worlds in Collision*, pages 114-115). If Sagan has doubts, let him perform an experiment.

The word *barad*, being described as hot, could not be ice hail, but is properly interpreted by me as meteorite.

That Pallas and Typhon are the same I need not have supported with a note — any dictionary will tell this.

Ashteroth-Kamaim is mentioned on page 169 of *Worlds in Collision*. Neither there nor anywhere else in my writings, have I ever said that whenever there is a hyphenated pair of names for a deity, this "indicates an attribute of a celestial body" . The "principle" is Sagan's invention. Sagan asks — in connection with Ammon — did the Egyptians see the Sun as a ram? Yet Ammon was not the Sun, but, as is known from many sources, Ammon was the planet Jupiter. (Herodotos, 11:41)

Sagan next presents "Velikovsky's Principal Hypothesis" , and he purports faithfully to tell what it is. I will follow this for two or three pages, and the reader will have enough. Sagan says: "at the moment that Moses strikes his staff upon the rock, the Red Sea parts. . . ." Later, "after the death of Moses ... the same comet comes screeching back for another grazing collision with the earth. At the moment when Joshua says "Sun, stand thou still upon Gibeon; and thou Moon, in the valley of Agalon," the Earth . . . obligingly ceases its rotation. ..." He later says that I "attempt to rescue the old-time religion" . To tell of Velikovsky's principal hypothesis in this vein is nothing but purposely misleading.

In the story of the crossing at the Sea of Passage, I deliberately did not even mention Moses; and some 200 pages later (in the section, “The Subjective Interpretation of the Events and Their Authenticity”) I wrote: “The sea was torn apart. The people attributed this act to the intervention of their leader; he lifted his staff over the waters and they divided. Of course, there is no person who can do this, and no staff with which it can be done. Likewise in the case of Joshua who commanded the sun and the moon to halt in their movements. Because the scientific mind cannot believe that a man can make the sun and moon to stand still, it disbelieves also the alleged event.” (*Worlds in Collision* pages 306-307.)

In the Biblical story, Moses did not hit the rock with his rod at the Sea of Passage; the striking of the rod against the rock is from the story (not quoted by me) of finding water in the desert. Biblical scholarship is not Sagan's field. And I stressed that many Israelites did not succeed in crossing the Sea, and the large majority of them perished (according to Psalms and midrashic sources, actually 49 out of 50 during the Plague of Darkness), contrary to the account by Sagan of my “hypothesis.”

These are just examples of Sagan retelling my book. One of the major areas of disagreement between Sagan and myself has to do with the composition of the atmosphere of Venus. This was perhaps the principal topic of discussion at the Symposium, and it is my understanding that the organizers of this volume will be including a complete and accurate transcript of that discussion. The readers of such a transcript will see for themselves how I have already replied, both during the morning session and during the evening session, to Sagan's claims. Thus I will not explore this subject further here.

Among many other disputes that I have insufficient space to discuss are: orbital circularization; escape velocities from Jupiter; and rotational stasis as opposed to axial tilting. I have answers regarding these and other issues, but the publication of those answers will have to be in some other context where there is not such a severe limitation on space. The reader is also directed to my published writings, wherein are already contained the answers to all of Sagan's various arguments and assertions, if he had but troubled to look.

But there are two matters that must be dealt with before I close:

Sagan's denial of the originality of my advance claim regarding the heat of Venus, and his widely publicized calculation of the “odds” against my theory's being true.

Sagan repeatedly states that none of my advance claims was original and correct. He made this announcement in the press before the Symposium, but as an organizer and panelist he should not have prejudiced the outcome. He says that Rupert Wildt in 1940 already proposed that Venus under the clouds is hot, and that I presented my claim of the heat of Venus without telling of Wildt and Wildt's estimate. (Actually, I did not give anyone's estimates.) So what was Wildt's estimate, and upon what was it based? He was the originator of the greenhouse effect theory that would keep Venus hot, and he came to the conclusion that only the subsolar point of the surface of Venus is of the temperature of boiling water, or possibly up to 135 degrees Centigrade. Professor G. Kuiper later showed that Wildt erred in his evaluation of the albedo or reflecting power of the Venus clouds, and therefore the temperature because of the greenhouse effect would be definitely less.

In *Worlds in Collision*, I stated that the protoplanet Venus “was in a state of candescence” only a few millennia ago, and I enumerated my reasons:

Venus experienced in quick succession its birth and expulsion under violent conditions; an existence as a comet on an ellipse which approached the sun closely; two encounters with the earth accompanied by discharges of potentials between these two bodies and with a thermal effect caused by conversion of momentum into heat; a number of contacts with Mars, and probably also with Jupiter. Since all this happened between the third and first millennia before the present era, the core of the planet Venus must still be hot.

I proposed that “Venus is hot”; that the source of the heat is Venus itself, rather than the sun; and that the temperatures are high enough for hydrocarbons to “circulate in gaseous form” and for the planet to have been “in a state of candescence” only a few thousand years ago, which means that the temperatures would be hundreds of degrees higher than Wildt or anyone else had ever imagined. Wildt's greenhouse effect theory was not relevant to my theory, and there was no reason why I should have cited him.

What I did cite in *Worlds in Collision* was the literature on the thermal *balance* of Venus. Thus the temperature of the clouds was found to be nearly the same (actually, about -25 C.), both for the day side and for the night side. This was paradoxical, since spectral indications were that Venus rotated very slowly. (This was later confirmed by radar studies.) Why did the night side not cool off? My answer was that the heat of the clouds of Venus, both on the night side and on the day side, is from the planet itself, not from the sun. I said: “Venus gives off heat.” Later, in a paper entitled “Is Venus' Heat Decreasing?” I called attention to separate measurements, spread over a number of years, in which the temperatures for the cloud surfaces seemed to be decreasing. Indeed, from these results only one deduction can be drawn: Venus cools off. Despite my repeated challenge to institute a planned observation of the rate of this cooling off, I have seen no paper dealing with the problem as it deserves — with a full cognition of what deductions are to be made if Venus is really cooling off. But even from an unplanned, haphazard comparison of figures, the trend can be recognized — and the lowering of the cloud surface temperature reflects an even greater lowering of the ground surface temperature of the planet.

“Venus gives off heat,” as I wrote in *Worlds in Collision*. In other words, it sends off more heat than it receives from the sun; it is in a state of thermal imbalance. If it has traveled on its orbit for billions of years and all the time has been cooling off, staggering figures would result for a time a million years ago, and unnatural figures for a time measured in billions of years. If in the fifty years since the observations of Pettit and Nicholson in the 1920's, the cloud surface lost, say, 8° C., a simple arithmetical deduction would point to a loss of 1° C. in 6 years, which would represent a substantially greater loss on the ground, under the cloud cover and the lower atmosphere. I assume that calculation would show that the planet must have been largely incandescent only thirty-four centuries ago.⁶ Such research would lead to the result that Venus is a newcomer (what its name in Latin also means).

⁶ Such a calculation was actually made by physicist C. J. Ransom in 1972. The results obtained by Dr. Ransom indicate that the temperature of Venus 3500 years ago would be 1184° K - or fully incandescent (*Pensée II*, Fall, 1972, p. 18).

The grace with which the figures of various observers, through decades, were left without being tabulated is a psychological phenomenon - the preference “not to know,” if the knowledge threatens to convey a firm basis to an iconoclastic concept.

Sagan calculated that a chance of one against 30,000 was needed in order to make Venus hit the Earth in any given millennium, and to produce a series of collisions the chance is one against 1027, if such collisions are statistically “independent” . The problem of marksmanship was discussed by me in my debate with the late Professor John Q. Stewart, Princeton University astronomer (*Harper's*, June, 1951, page 64). I came to a completely different result:

“The image of ‘marksmanship’ is not well derived. The planets revolve in the plane of the ecliptic; if one should move on a stretched orbit, it would contact its neighbor planets. And if a comet with a tail 100 million miles long [actually, it is not excluded that the tail might be even several hundred million miles long] should move in the ecliptic, no good fortune would keep the [inner] planets from passing through its fabric; at its every passage inside the terrestrial orbit, the Earth would have a better than 60 to 40 chance of going through its tail or head.”

In his calculations, Sagan chops off the tail of Venus that sweeps the entire area, and assumes that “Velikovsky is talking about a grazing collision: the surfaces of Earth and Venus scrape!” But in my writings it is repeatedly emphasized that these near collisions were *not* “grazing” collisions: the “targets” were larger, by many orders of magnitude, than Sagan allows. On page 85 I said that “the head of the comet did not crash into the earth,” and on page 372 I said that planets, during a close approach to each other, are “cushioned in the magnetic fields around them ... an actual crushing collision of the lithospheres will be avoided.”

Sagan calculates how close the head of the comet Venus must have come to the surface of the Earth, on the assumption that tides 1600 miles high were raised. On page 72, I quoted a midrashic source: “The waters were piled up to the height of sixteen hundred miles, and they could be seen by all the nations of the earth.” In the very next sentence I indicated that this legend obviously is not to be taken literally: “The figure in this sentence intends to say that the heap of water was tremendous.” But Sagan, in search of bizarre premises from which to derive bizarre conclusions, takes the figure of 1600 miles literally, and bases his calculations upon it. The figures in his conclusions should not be taken any more seriously than the figures in his premises.

Sagan further assumes “that Velikovsky believes in several statistically independent collisions in a few hundred years” , despite my many explicit statements that the collisions were causally interrelated. For example, see *Worlds in Collision*, page 373; “Each collision between two planets in the past caused a series of subsequent collisions.” A ten-car pile-up on the expressway is a chain reaction: it does not consist of ten independent events. Sagan's inferences are unsound. He should know better than to apply to causally interrelated events laws of probability that are appropriate only for independent events.

In general, when we are speaking of the probability of historical events, we must proceed with great care and caution. Whether something did or did not occur must be decided on the basis of historical evidence, not on the basis of probability laws. Actually, if some historical event is described with great precision, and each characteristic of that event is treated as an independent factor, then the probability of such an event will be vanishingly small. If all the details of an

automobile collision are listed, then the probability that such an event could occur will be so low as to seem almost impossible. Yet collisions of automobiles have indeed occurred — and so have near collisions of planets.

Jupiter's Radio Noises

One of the major deductions from the study of ancient civilizations was the recognition that the planetary and cometary bodies are charged objects and the solar system itself is regulated not solely by the law of gravitation; that electromagnetic interactions must exist and where following the inverse square law must be unrecognizable in their effects on the calculations of celestial mechanics - charge can, so to say, be hidden in or masked by the mass. Thus the problem of Pluto influencing Uranus and Neptune more than its mass can account for is a case of a substantial charge on a small planet. But where the less pronounced electromagnetic inverse cube relations take place, like in Mercury's precession of its perihelion, divergences from the celestial computations are registered as anomalies. Mercury moves through a general magnetic field of the Sun that influences it more strongly than it influences the remoter planets besides the influence on it and on them of the magnetic solar spots and solar wind.

In catastrophic conditions, with two celestial bodies approaching one another closely, the electromagnetic interactions may become most pronounced - the cometary protoplanet Venus produced a display of discharges between its head and its trailing part when the orbital movement of the protoplanet was disrupted by the close approach to the Earth; in the latter, eddy currents were generated with the effects due to such phenomenon (see *Worlds in Collision*, "Epilogue"). Interplanetary discharges took place when Mars and Earth came into close contact (*Worlds in Collision*, "Synodus"). The projected volumes dealing with catastrophes preceding those that took place at the end of the Middle Kingdom in Egypt carry the titles "Saturn and the Flood" and "Jupiter of the Thunderbolt".

The planet-god Jupiter (Zeus, Ormuzd, Shiva, Marduk) was pictured with a thunderbolt because of the spectacles witnessed by the inhabitants of the Earth —like a discharge that was directed toward Venus when it approached its parental body (*Worlds in Collision*, "Blazing Star"), or when the Earth itself might have been the target, as the content of the volume "Jupiter of the Thunderbolt" will reveal.

The understanding that the solar system is not neutral in its components but possibly neutral as a whole led me to the conclusion that the charge of the Sun may be equal to the combined charge of the planetary bodies and that quite possibly in Jupiter is assembled the major portion of it; thus, being ca. 1000 times smaller than the Sun it is charged to a very substantial potential.

Its potential could have been greater in the past; certainly planetary bodies exchanging discharges neutralized themselves to some degree; Mars, for instance, must have been much more charged in the past before the events of the first half of the first millennium before the present era. The charge of the planet, I thought, may even be decisive in the position the planet occupies in the planetary system. I even considered theoretically a system in which gravitation is completely supplanted by electromagnetic effects with the charged planets traveling in the magnetic field of the Sun, itself being a charged body that by its rotation creates the magnetic field permeating the solar system; I also contemplated the existence of magnetic shells that would be the determinative of the planetary distances (Bode's Law).

Since 1941, I insisted that electromagnetic interrelations in the solar system cannot be ignored - this was the theme of my long debate, in writing and oral, with Einstein - from August 1952 to his death in April of 1955. At some point in our debate (in a letter written in June 1954) I offered to stake our debate on whether Jupiter sends out radio noises (of non-thermal nature, as I already claimed in my Forum Lecture of 14 October, 1954), to which he reacted skeptically, yet was greatly surprised when nine days before his death I brought to him the news (*New York Times* of April 6, 1955) that such radio noises were accidentally detected.

It has been long known that Jupiter possesses an angular momentum that is superior to the angular momentum of the Sun, even of the Sun with the rest of the planets combined. This appeared to me not without a definite role of charges accumulated in Jupiter.

Jupiter was believed to be a cold planet - since the 19th century it was thought to be covered by a frozen mantle of ices over ten thousand miles thick. To me, however, from the knowledge of its activities in ancient times, it did not appear as an inert gravitational body; I thought also of Jupiter as a dark star (*Worlds in Collision*, p. 373); but the radio noises that I expected it to be sending out I considered as of non-thermal origin and so I also expressed myself in the mentioned Forum Lecture. But whereas I expressed myself in October 1952: "The planet is cold, yet its gases are in motion. It appears probable to me that it sends out radio noises as do the sun and the stars. I suggest that this be investigated," in June 1954 in a letter to Einstein, I took a most definite stand: "Of course, I am a heretic, for I question the neutral state of celestial bodies. There are various tests that could be made. For instance, does Jupiter send out radio-noises or not? This can easily be found if you should wish." This claim was also vindicated in the announcement made by Burke and Franklin on April 6 of 1955.

The relevance of the orbital periods of Jupiter and Saturn to the sunspot cycle appeared to me, if real, based on electromagnetic interdependence. The highly charged Jupiter must create a powerful magnetosphere; it may even create magnetic shells, for distribution of its satellites, a thing not yet proven; but certainly the large satellites of Jupiter, and especially the innermost of the Jovian satellites, must be much affected by its magnetic field. Jupiter itself appeared to me to be of contrasting charges on various levels which would account for the potential difference observed in celestial battles by the ancients between the head and the trailing part of the Jovian progeny - protoplanet Venus (*Worlds in Collision*, "The Battle in the Sky"), the head having been expelled from Jupiter's deeper parts, the trailing part of debris and gases from a more superficial layer.

Thus discharges on Jupiter could be dictated by potential difference. The closest of the Galilean satellites must be acting as a target independent of whether a spark discharge actually takes place or a stream of charged particles is directed toward it and to a lesser extent toward other satellites (the fifth, however is only 112,000 miles mean distance from the planet). A purely gravitational relationship between Jupiter and its satellites appeared to me unthinkable; and on this phenomenon, in my estimate, the purely gravitational system of the World must stumble, as also on the case of the behavior of the comets when approaching, then circling the Sun in their perihelia a subject much discussed by me with Einstein in my effort to convince him of the fallibility of a purely gravitational system of the solar system (and of the universe in general).

The discovery of the Jovian noises (1955), and of the terrestrial magnetosphere (1958), claimed by me also in the Forum Lecture of 1953, and of the interplanetary magnetic field centered on the Sun and rotating with it (1960), and of the solar wind or uninterrupted streams of plasma (1960), made the purely gravitational system of the World untenable. Yet among astronomers, as late as 1971, the full significance of the fact for the understanding of the structure of the universe only very slowly finds its way, as can be exemplified by a paper by Prof. Ivan King, “The Dynamics of Star Clusters”, where no mention is found of any electromagnetic participation in the mechanics of the galaxies.

The realization that Jupiter, which participated in a vigorous way in the theomachy (celestial battles), is not inert and cold led me to the conclusion that Jupiter must be also hot under its cloud cover, at some depth. This afterthought made me also claim that Jupiter is hot in a discussion with Prof. I. I. Shapiro of M.I.T., well-known authority in astrophysics, who denied such a possibility. This claim was confirmed recently by probes of the temperature underlying the surface clouds.

This leads me to the necessity to discuss some other aspects of the recent history of Jupiter, which all ancient peoples of the World elevated to the role of the supreme deity, the role it took over from Kronos-Saturn. But such a discussion I will undertake separately and at some length.

Saturn

Of Saturn I intended, already for some two decades, to write in a volume “Saturn and the Flood,” in which, as the title discloses, I would endeavor to identify this planet as the prime cause of the greatest of all catastrophes in human memory—the universal flood, or Deluge. This part of *Worlds in Collision* was conceived and drafted together with the parts dealing with Venus and Mars, but the elaboration of details was postponed and other labors claimed my attention and I am still before work unfinished. I will, however, disclose in a few sentences what is the subject of that part of reconstruction of world history.

The age that man later called the Age of Cronos (Saturn) was remembered with nostalgia as the age of bliss. It was the earliest age of which man retained some, however dim, memories, but farther into the past, the dimness amounts almost to darkness. Saturn was also a more massive body than it is now, possibly of the volume of Jupiter,⁷ whereas now the proportion is approximately 7 to 13.

At a date that I would be hard put to task to identify even with approximation, but possibly about ten thousand years ago, Saturn was disturbed by Jupiter and exploded, actually became a nova. The solar system and reaches beyond it were illuminated by the exploded star, and in a matter of a week the earth was enveloped in waters of Saturnian origin.

Told in such brevity, the story sounds fantastic. I had the choice not to mention these events here at all or to refer to them and ask indulgence on the part of the readers for having said something unusual, and at the same time ask them to wait for a detailed narrative at some indeterminate time. I selected the latter. I have already mentioned that the major planets were in some way connected with the earlier cataclysms, one of which was the Deluge (*Worlds in Collision*, p. 373).

When in 1946 the manuscript of *Worlds in Collision* was first offered to the publishers (Macmillan and Co., New York), it contained the story of the Deluge and of the catastrophe that terminated the Old Kingdom in Egypt. But, at the suggestion of the reader for the publishing company, the book should have concentrated on one event; we compromised and presented in the published volume two series of catastrophes—those that took place in the fifteenth century before the present era and were caused by near approaches of Venus, and those which occurred in the eighth century before this era, and were caused by close approaches of Mars. The unused material was left for elaboration in two volumes: “Saturn and the Flood” and “Jupiter of the Thunderbolt.”

With this hardly even a summary, as told on this page, I should possibly dispel any misconception as to what is the design of my manuscript, too slow in the making. As to “predictions,” I could make several and I offer them cognizant of the fact that a prediction in science needs to be elaborated on the reasons that led to it.

I assumed, in the first place, that the planet Saturn must contain water to the extent that it is a “water planet.” It is also possible that water that enveloped the earth following the explosion of Saturn was at least partly formed by hydrogen combining with the oxygen of the terrestrial

⁷ Interestingly, for certain reasons G. Kuiper assumed in recent years that Saturn originally was of a mass equal to that of Jupiter. *Sky & Telescope*, March 1959, p. 259.

atmosphere—and there are indications that I intend to discuss in my book on the Deluge which point toward a sudden drop in oxygen content in the terrestrial atmosphere. But the fact that comets were observed consisting of water (ice), according to their spectral picture, permits the conclusion that water “ready-made” came from the planetary “nova.” Actually, in years subsequent to my concept of Worlds in Collision, water was identified as present on Saturn.

Further, I assumed that sodium chloride, or common salt, is an ingredient of the Saturnian atmosphere. Geophysicists have long wondered as to the origin of salt in the ocean. Sodium could have been derived from terrestrial rocks; but they are poor in chlorine. To some extent chlorine in oceans could have come from volcanic eruptions but it would require eruptions on an almost unimaginable scale to produce all of the chlorine locked in the salt of the oceans. The ancient traditions of Deluge refer also to the water arriving from space as salty and warm.

I have thought also of free chlorine (not combined into salt) on Saturn; but it is possible that vegetable life, at least, is present on Saturn, and free chlorine would interfere with vegetation; the reasons, though not compelling, for this assumption of vegetation on Saturn are also reserved for the detailed discussion. The tradition found in ancient texts refers to innumerable new forms of life in animal and plant kingdom following the Flood, which could have been solely a result of multiple mutations. But there exists in ancient lore an ever recurring association of seeds and new plant forms, with Saturn, Osiris, Tammus, Cronos, all of whom I understand as personifications of the planet Saturn.

In recent years I have chanced to read the view of Josif Shklovsky, a Russian astrophysicist, that a nova would be a source of cosmic rays even thousands of years after the explosion. Shklovsky and his collaborators offered the suggestion that at some past time the earth, or the entire solar system, passed through clouds of cosmic rays, resulting from a nova star, that caused the extinction of various forms of life on earth, dinosaurs and others. This thought found an echo in me because the same thoughts had been put on paper by me two decades earlier. But their assumption that cosmic rays may be discharged by a nova thousands of years after the explosion led me to think that if such is the case, Saturn may still emit cosmic rays, if, by now, only of low energy. Therefore when asked at some college gatherings what new “prediction” I would make, and desirous to tell something that in case of detection could not be ascribed to a lucky guess, I volunteered to suggest that there is a good chance that Saturn emits low energy cosmic rays. This on the assumption that the Russians were right in saying that a nova would still be sending out such radiation after so long a period.

Finally, Saturn must emit more heat than it receives from the Sun. Reasons for such conditions of Saturn are at least two: first, the residual heat of the catastrophe in which Saturn was derailed from its orbit; second, the radioactivity that resulted from the catastrophe must still be pronounced on Saturn. In addition, Saturn can be regarded as a star and may have some mechanisms that make our sun burn with intense light. Because the surface clouds of Saturn are cold and the distance of Saturn from the sun renders the heat from this source very limited, the conclusion was drawn that Saturn must be very cold, frozen to its core. We came to a different conclusion also concerning the temperature of Saturn below the surface cloud layer.

In 1966 Dr. K. I. Kellermann described in *Icarus* the surprising fact that Saturn, at the wavelength of 21.3 cm. shows a temperature of 90 degrees F., which cannot be explained by solar radiation. It will be found of still higher temperature.

The rings of Saturn are formations of less than ten or twelve thousand years old. They must consist largely of water in the form of ice, but since the ancient lore all around the world tells that it was Jupiter who put these rings around Saturn, they may have some other components, too. Since these lines were written, spectroscopic study of the Saturnian rings has revealed that they consist mainly of water in the form of ice (1966).

Sodium chloride and cosmic rays are two phenomena still waiting to be investigated. Therefore, when I presented to Dr. H. H. Hess in his capacity as chairman of the Space Board of the National Academy of Science, a memorandum (dated September 11, 1963), subsequently submitted also to Dr. Homer Newell in his capacity as Director of NASA, I included these lines concerning Saturn:

“Saturn. Tests should be devised for detection of low-energy cosmic rays emanating from Saturn, especially during the weeks before and after a conjunction of Earth-Jupiter-Saturn.”

“Chlorine should be looked for in the Saturnian spectrum of absorption.”

Mercury

Mercury, the planet closest to the sun, is like Venus, a morning and evening star. But whereas Venus circles the sun in 224.7 terrestrial days, Mercury completes its orbital revolution in 88 days. Being so close to the sun it is rarely visible. Copernicus never saw it in the murky sky of Pomerania and wrote of it in his *De Revolutionibus* from what he learned in Claudius Ptolemy, the by then fourteen century old authority. Mercury is smaller than Venus and its mass was computed to be less than one eighteenth of the mass of the earth, whereas Venus is more than four-fifths of it. Mercury's diameter is by one half larger than our moon's diameter. Its orbit is a rather stretched ellipse whose perihelion, the point closest to the sun, and aphelion, the furthest point, are in the approximate ratio of two to three.

As the moon is locked with one side to the earth, its primary, so, and for the same reason, Mercury was thought to be locked permanently with one side to its primary, the sun. It was estimated that when the planet was in the process of formation, the sun must have caused in it tides, and this, in turn, must have exerted a tidal friction, and breaking of axial rotation. Thus the planet, so close to the sun for billions of years, must be permanently locked with one face to the sun.

In 1845 Adams and, independently, Leverrier, calculated in advance of its discovery, from perturbations of Uranus, the existence and the position of Neptune, thus supplying the world of physics and astronomy with what was (and often still is) regarded as the best confirmation of the scheme in which only gravitation and inertia direct the run of the celestial bodies. But in the same year Leverrier also calculated that the perihelion of Mercury advances in the direction of the planet's motion; it is the precession of the perihelion, or what is the same, a slow rotation of the long axis of the Mercurial orbit.

Laplace, who preceded Leverrier by half a century, acquired fame at the age 23 by showing that all kinds of irregularities in the celestial motions that have the appearance of "running down" and were so viewed by Newton himself who thought that Divine intervention is needed from time to time to rewind the mechanism, all these irregularities are not of a kind that accumulate, but are temporary, are actually swings or oscillations that after certain intervals reverse their direction and that therefore the celestial mechanism will never need rewinding.

Mercury's anomaly was obviously continually accumulating, and therefore not of an oscillating nature, not a swing. The anomaly was actually very minute. The observed precession amounts to 570 seconds of the arc in a century; of this amount, over 530 seconds of the arc of precession was attributed by Leverrier to the action of the planets perturbing Mercury; but some 35 seconds of the arc were unaccounted for, a figure increased by later investigation to 43 seconds. Since Mercury revolves in 88 terrestrial days around the sun, it makes more than 400 revolutions in a century and the anomaly amounts to as little as circa one tenth of a second of an arc of unaccounted precession at each revolution. How small this angle of deviation is one may perceive if one imagines a penny, 1.9cm, nearly three-quarters of an inch in diameter, viewed without magnification from a distance of about thirty miles. But so proud was the world of the mathematics of the first half of the nineteenth century, with its achievements, that such an unaccounted discrepancy in the Mercurial motions was paraded to show the acumen of science.

Leverrier, who predicted the existence of Neptune, a planet on an extreme orbit, thought that the residue of the Mercurial precession would be accounted for if yet another planet, still undiscovered, revolves inside the Mercurial orbit; because of the proximity of the sun it would not be easily observed, but Leverrier thought he had detected it. No confirmation came in the decades that followed. Other conjectures were made, such as a surmise that the mass of the Sun is not uniformly distributed, or that the Sun is a slightly “loaded” body; but there was nothing to support this particular claim apart from the fact that the anomaly of Mercury needed to be accounted for. Thus Leverrier in the same year 1845, by discovering Neptune confirmed the gravitational theory of Newton, and by discovering the anomaly of Mercury he cast doubt on the theory’s infallibility.

Seventy years after Leverrier calculated the anomaly, Einstein offered his explanation of it in his General Theory of Relativity (1911-1915) Ten years earlier he had published his Special Theory of Relativity (so named when the General Theory was adduced). In the Special Theory (1905) he deprived space and time, or their units, of the attribute of constancy – a second or a meter on a body moving in relation to an observer is no longer exactly a second or a meter, and he attributed constancy to the velocity of light, independently of whether the source of light is or is not in motion in relation to an observer. In the General Theory Einstein tackled the nature of gravitation. Space not being endowed with the attribute of constancy, Einstein visualized it as curved in the presence of a mass.

For the General Theory of Relativity Einstein offered three observational cases as proofs. The Mercurial anomaly is almost exactly what his theory would presuppose of a planet that moves in the curved space caused by the proximity of the huge mass of the sun. The next observational evidence accountable by the General Theory was the shifting towards the red (red shift) in the spectrum of light emanating from the sun, compared with the light of laboratory sources, a phenomenon in Einstein’s explanation resulting too from bending of space by the presence of heavy mass (sun).

The third phenomenon would be in light emitted by a star and passing near the solar disk (bending of the ray).

Einstein did not make “three predictions” for the validation of the General Theory of Relativity as it is often said; Sir James Jeans in his article on Relativity in the Encyclopedia Britannica refers in such terms to the three phenomena:

Einstein, knowing the mass of the sun, found himself in a *position to predict absolutely* what the motion of the perihelion of Mercury must be. It was found to be 42.9” a century, a figure which agreed with observation to well within the limits of errors of the observation... The theory makes one further prediction which admits of experimental test: The light received from a calcium atom situated in the intense gravitational field near the sun’s surface ought to be of slower period, and therefore of redder colour than the similar light emitted by terrestrial atoms... W. S. Adams found an actual shift of 0.32 A. It is hardly possible any longer to doubt that the spectral shift predicted by Einstein really exists...

A star or other massive body distorts the continuum [of space] in its neighborhood... in the neighborhood of such a body a ray of light does not travel in a straight line; it is deflected by the gravitational field of the body... None of the expeditions had of course measured the deflections of the stars actually at the sun's limb; most of the stars were several diameters away from the limb, the observed deflections being corrected so as to bring them to the limb. The deflection of stars at all distances were found to agree well with the predictions of Einstein's theory.

Actually in a paper published in 1911 Einstein, claiming redshift in solar light writes in a footnote:

L. F. Jewel (*Journal de Physique*, VI (1897), 84), and especially Ch. Fabry and H. Boisson *Compt. rend.* 148 (1909), 688-90) have actually established noticeable shifts of fine spectral lines from the sequence(?) here calculated, but have ascribed them to the effect of pressure in the absorbing layer.⁸

As to the Mercury's anomaly, it was announced by Leverrier in 1845 and often discussed since. Thus only the bending of light passing near a mass was in the category of prediction. A paper was printed by Soldner in the *Bode's Annual* but Einstein evidently did not know of that paper. Soldner calculated that following Newton's concepts of light as a stream of particles the ray of light passing near the Sun is deflected by a small angle; Einstein, however, claimed a deflection twice as large.

Every textbook on astronomy used to relate that Mercury is locked with one and the same face in relation to the Sun as the Moon is in relation to the Earth; tidal forces must have produced such effect.

With one side turned to the Sun and the other facing the cold space, it was estimated that Mercury must be as extremely hot on the lit side while the temperature on the other side must be very close to absolute zero.

Space probes have obtained the surprising result that the non-illuminated side of Mercury is comfortably warm, actually is 60 degrees F., or of room temperature. In order to explain such phenomenon it was assumed that Mercury, thought to be without atmosphere, actually has one consisting of gases of heavy atoms; the atmosphere could carry the temperature from one side of Mercury to the other. Mercury had been thought to be void of any atmosphere because the small planet could not keep the molecules of gases from dissipating into space; first, lighter gases, but then also heavier would need to be lost to space; but in view of the observed temperature on the night side of Mercury, the assumption was made that heavy gases must have still survived on it. Great was, however, the surprise when Nicholas Kozyreff, investigating Mercury on presence of an atmosphere, announced the detection of hydrogen, the lightest of all gases. This was in sheer conflict with all theoretical computations. In the effort of finding the cause of the Mercurial temperature on the side turned away from the Sun, a new riddle that instead of explaining a phenomenon needed its own explanation, and this was not forthcoming because Mercury, millions or billions of years on its orbit, could not preserve an atmosphere of hydrogen.

⁸ "Über den Einfluss der Schwerkraft auf die Ausbreitung des Lichtes," *Annalen der Physik*, XXXV (1911).

In further search of the cause of Mercurial thermal “anomaly,” the evident thing was undertaken and the planet was investigated by radar. There was another surprise lying in wait. The planet was rotating. This, too, was in conflict with the theoretical computations. Mercury had to be locked with one face to the Sun. But it is not. The rate of rotation was found to be once in 58.65 days, whereas one orbital rotation of the planet equals 88 terrestrial days. The heated state of the night side of Mercury appeared to have now an explanation, though a more careful analysis must show that rotating once in 58 days, Mercurial surface temperature must drop far below 60 degrees F. It is, for instance, observed that the surface temperature of the Moon warmed by the Sun precipitously falls when during lunar eclipses Earth interposes itself between the Sun and the Moon—and the duration of an eclipse is counted in minutes, not days, as in the case of Mercury’s rotation.

With the discovery of the Mercurial rotation, not sufficient to explain the thermal question, the question of why Mercury is not locked with one face to the sun became a matter of new perplexity. The observation was made by a team of Cornell University scientists. Thomas Gold speaking for the team announced that Mercury could not have been stationed on its orbit for long—400,000 years was, in the opinion of Gold, the longest stretch of time that could be allowed for Mercury to remain unlocked. On the assumption that the solar system is six or nine billion years old, 400,000 years represent only 1/10,000 of the time since the planets, following the accepted view, obtained their positions and acquired their rotational rates—and this is the upper limit. Neither the tidal nor the nebular theories can square with the newly discovered fact.

Mercury is beset by riddles: it should not have a hydrogen atmosphere, but, if Kozyrev is right, it has such atmosphere. It should not rotate, but it rotates. It should have the night side much cooler than 60 degrees F. but it has this temperature.

Actually all three unexplained phenomena point toward an adventurous past, a past counted by thousands of years, but not by millions. Mercury has heat of its own, not just reflected heat of the Sun; Mercury has still an atmosphere of hydrogen, the last vestiges of a more extensive halo and trail (caduceus) seen by our ancestors in the fourth or third millennium before the present era; Mercury rotates because it is on its orbit for only several thousands of years. It is on a stretched orbit—a relic of its recent arrival at its present orbital path. As to the last point, I would reserve an opinion because magnetic forces near the Sun need to be calculated in any motion of the planet. These forces are most probably also responsible, in our understanding for the precession of the perihelion of the planet, and Leverrier’s discovery of this precession does not require a geometric curvature of space.

Mercury, Hermes of the Greeks, was thought to keep well his secrets. The ancient writings not intended for circulation but for the study of the initiated only were called hermetic books. In our days Mercury disclosed four secrets: first that it is warm on the darkened side; then that it has a hydrogen atmosphere; next, that its axis is wobbling, and finally that it is not locked with one and the same face toward the Sun. Each of the four revealed facts is in conflict with accepted solutions. All together offer a solution—a planet on a new position since, in astronomical sense, recent times.

In the story as told in the volume *Worlds in Collision* the planet Mercury plays no role; however in the projected volume about earlier events on the celestial screen, Mercury was a participant

and was not an idle spectator of the theomachy, the battle of the gods. It had an epoch of its own, or an act in which it was the principle actor, in the early historical times, in an age antecedent to the events in the solar system, dominated (as seen by man from the earth) first by Venus, then by Mars. But despite my not having introduced Mercury into the narrative of those later times (15th-7th century before this era) it could not remain even then as a completely inactive member of the planetary family. Especially if planets are charged bodies, the entrance of a new planet (Venus) into the system must have caused much havoc also on planets not in collision or near collision. One should think of the changes which the entire solar system would undergo and also keep in mind what the entrance of a new proton or electron would signify for an atom—the result could amount to the transmutation of an element.

The Romans as well as the Greeks pictured Mercury with wings, either on his headgear or at his ankles, and with an emblem, caduceus, twin snakes winding. The Babylonian name of the planet was Nebo, and he was an important deity, as the name of the mountain Nebo, on which tradition lets Moses die (Sinai, by the way, was consecrated to the Moon, Sin in Babylonian); Nebo in the names of the Kings Nabopolassar and Nebukhadnezzar testifies to its significance in the Babylonian pantheon as late as the seventh and sixth centuries. Equally pronounced was the role of Thoth, the planet Mercury of the Egyptian pantheon, the theophoric part of the name Thutmose or Tut-ankh-amen.

Mercury, or Hermes of the Greeks, was a swift messenger of the gods that speeded on his errand sent by Jupiter or Zeus.

In my understanding Mercury was once a satellite of Jupiter or of Saturn and under circumstances not understood by me, was directed toward the sun and caught there in an orbit still elliptical. It could, however, have been a comet passing near Jupiter and the entwined snakes of the caduceus may memorialize the appearance it had when seen by the inhabitants of the Earth. There are indices that point toward Mercury's involvement in the catastrophe that is described in Genesis as the confusion of the builders of the Tower of Babel, something that in modern medical terms seems like a consequence of a deep electrical shock.

The claim is that Mercury travels on its present orbit only since some five or six thousand years. This view conflicts with both standard alternatives—of nebular and of tidal theories of the origin of the planetary family and with the assumption that the planets occupy the same orbits since billions of years. Since the early days of modern science, actually since Aristotle, it was considered undisputable that since the origin of the solar system, Mercury has been moving on the very same path. The study of ancient texts convinced me that there was nothing to this belief besides wishful thinking: the entire solar system was repeatedly rearranged. Mercury does not occupy its orbit since six billion years—the assumed age of the universe (which by the way was repeatedly re-assessed from 2 billion when I started my studies till by now 10 and 12 billion years are occasionally heard).

Already before the publication of *Worlds in Collision* I considered (and let it be set in print) a system of the world in which the sun, being a charged body in rotation, creates a magnetic field; the planets, being charged bodies, move in that magnetic field and are compelled to proceed on their orbits; to this phenomenon I gave the name “circumduction” (see my *Cosmos Without Gravitation*, 1946), borrowed from J. Kepler. I considered Mercury's precession, discovered by

Leverrier in 1846, as resulting from such an effect, and, possibly, from a growing charge on Mercury (besides its not having completely settled after the celestial “battles”). I considered Einstein’s use of Mercury’s precession as an ad hoc argument for the General Theory of Relativity (certainly not a prediction, as James Jeans wrote in *The Encyclopedia Britannica*).

In my debate with Einstein, already early, in a letter written in August or September, 1952, I drew his attention to charges and consequences for Mercury, traveling in the extended corona of the sun. I returned to this also later in our correspondence.

Dr. Dicke came up with an oblate sun as a partial cause of the Mercurial anomaly. I drew his attention to the fact that he disregarded the by then discovered solar plasma and the magnetic field centered on the sun and permeating the solar system. He gave me a strange answer: “That is something we have to disregard.”

In my paper at the San Francisco Symposium, “Velikovsky’s Challenge to Science,” I once more drew attention to the problem and its consistent evasion in discussions of the General Theory of Relativity. Even in the days of Einstein he must have known of the general magnetic field of the sun, discovered by Hale a few years before Einstein used the argument for his theory; the magnetism of the solar spots was discovered earlier by Hale. Einstein corresponded with Hale on other matters.

As a matter of methodology it appeared to me improper that Einstein selected the case of Mercurial anomaly (precession of the perihelion) for the support of the General Theory of Relativity, without eliminating first the possible effect of the solar magnetic field on the precession of Mercury.

According to Newton an inverse cube effect when superimposed on an inverse square effect would result in a precession. A regular dipole magnetic field would produce an inverse cube effect when superimposed on an inverse square effect, due to gravitation.

The general magnetic field of the Sun was made known by G. E. Hale in 1912 at the time when Einstein was construing his General Theory. The magnetic property of solar spots had been discovered at the beginning of the century by the same Hale.

On the 14th of October, 1913, Einstein wrote to Hale on the issue of another of his advance claims, actually the only one that could put claim to this definition. In his letter he inquired whether there was a possibility to observe in broad daylight, very close to the rim of the sun, some fixed star, this with the help of the powerful telescope that Hale built (Mt. Wilson 100-inch telescope). It was a naive inquiry; however, it was suggested to Einstein by another physicist in Zurich and he followed the advice—the idea was that if the answer were positive there would be no need to wait for a full solar eclipse for observing whether the sun (or any large mass) deflects a ray of light from its rectilinear path. Writing to Hale, Einstein showed much respect—but where he had to take into account Hale’s great discoveries, he omitted to do so. Only by excluding the possibility that magnetic fields deflect a ray of light from rectilinear passage, would Einstein have cleared the way for offering an explanation based on a new principle in science.

In my understanding that goes back to the forties, the Sun being a rotating charged body creates a magnetic field that stretches far into interplanetary space. This field rotates with the Sun on which it is centered; at the distance of any planet, the field travels the length of the planetary orbit in the same time it needs for one axial rotation, or one turn of the Sun on its axis.

Mercury is a charged body and it moves in the solar magnetic field that rotates swifter than Mercury proceeds on its orbit.

In August 1952 I started my long debate with Einstein on the question whether inertia and gravitation are the only forces responsible for all the movements of the celestial clock, or whether electricity and magnetism, to whatever extent, need to be considered, too. I put the problem of Mercury squarely before him on this issue. I wrote:

Now the visible streamers of the sun that conveyed to Hale the idea that the sun is a magnet reach a long way towards Mercury, almost half the way. Was the electromagnetic state of the sun ever considered as the cause of the anomaly? The effect of the electromagnetic action must have been reckoned, and possibly excluded, but not disregarded.... Also the fact that the sun radiates at the expense of splitting (or building-up) of atoms was never followed through to the inevitable conclusion that the sun is a charged body in motion. At least the action of the magnetic spots of the sun with a field intensity reaching four or five thousand gauss should have been, if only once, taken into computation for its influence on planetary motion, Mercury in the first place, if only for the purpose of showing it as ineffective.

When, nine years later, Prof. H. H. Hess, upon being appointed, or elected, chairman of the Science Space Board of the National Academy of Sciences, wished to hear from me some suggestions for the activities of NASA (National Aeronautics and Space Administration), I offered, on September 11, 1963, a program for a series of investigations; concerning Mercury I wrote:

The cause of the precession of the perihelion should be re-examined in the light of the presence of a magnetic field of solar origin and solar plasma through which Mercury ploughs. An artificial satellite with a perihelion close to the sun could be tracked as to the precession of its perihelion.

Since I wrote this suggestion for experiment more than twelve years have passed. I have not heard or read of such a satellite having been dispatched.

At the symposium "Velikovsky's Challenge to Science" organized by the AAAS in San Francisco in February 1974, in my paper, entitled "My Challenge to Conventional Views in Science," I returned to the problem of the electromagnetic nature of the solar system and of the universe in general, and said concerning Mercury's anomaly:

It was, of course, known since Gilbert that the Earth is a magnet and G. E. Hale discovered that solar spots are magnetic and that the Sun possesses a general magnetic field. But this did not keep Einstein, a few years later, from accounting for the Mercurial precession by a new principle instead of first eliminating the effect of the newly discovered solar magnetic field on Mercury's movement.

If I was completely at odds with the cosmogony that had the solar system without history since creation, I was also carrying my heresy into a most sacred field, the holy of holies of science, to celestial mechanics. I had a chapter on the subject at the end of *Worlds in Collision*, but I kept those galleys from inclusion in the book and instead I included only one or two paragraphs—and the only italicized words in the book are found in them—namely: “The accepted celestial mechanics, notwithstanding the many calculations that have been carried out to many decimal places, or verified by celestial motions, stands only if the sun, the source of light, warmth, and other radiation produced by fusion and fission of atoms, *is as a whole an electrically neutral body*, and also if the planets, in their usual orbits, are neutral bodies.” I showed how the events I reconstructed could have occurred in the frame of classical celestial mechanics, but coming from the field of studying the working of the brain—I was the first to claim that electrical disturbances lie at the basis of epileptic seizures—I was greatly surprised to find that astronomy, the queen of sciences, lives still in the pre-Faraday age, not even in the time of kerosene lamps, but of candles and oil. It was, or course, known since Gilbert that the earth is a magnet and G. E. Hale discovered that solar spots are magnetic and that the Sun possesses a general magnetic field. But this did not keep Einstein, a few years later, for accounting for the Mercurial precession by a new principle, instead of first eliminating the effect of the newly discovered solar magnetic field on Mercury’s movement.

Thus I did not omit once more to challenge the accepted view that Mercury’s anomaly serves as confirmation of Einstein’s concept of space curved in the presence of a mass, independently of whether Einstein was right or not in the theory itself. But if the Mercurial precession has a different cause than that which Einstein envisaged, the absence of the effect expected by him could not but be damaging to his theory of the nature of gravitation.

It did not take long after the symposium in San Francisco and the Mariner X probe passing upon passing and surveying Venus, approached Mercury.

Even from a great distance the photographs of Mercury taken by the unmanned probe showed a surface that attested to a very stormy past of the planet and as the probes came closer, the features grew in detail. It revealed itself as a battered world. Its surface features were never before observed by a telescope from the Earth; but after the scientific world accustomed itself to the Martian photographs of American and Russian space probes, there was no outcry of surprise anymore, though this planet closest to the sun was the least known as to its surface features. But the explanations applied to Mars and Moon for the phenomenon of cratered surface, namely, that these celestial bodies are in travelling, Mars more, the Moon less, in the zone of the asteroids that supposedly by collisions with Mars and the Moon have caused these features, could not well be applied to Mercury, out of reach of almost all asteroids. And there were other features on the Mercurial surface that bespoke a violent past.

Very shortly after the February, 1974 symposium, Mariner X, passing near Mercury, established to the great surprise of all scientists, that it possesses a magnetosphere. Since it rotates slowly, in my opinion the magnetosphere results from the speedy relative motion of the space satellite and Mercury on its orbit. On the second passage, and third, of the satellite, the existence of the magnetic field around Mercury (magnetosphere) was confirmed. Now it becomes possible to abstain from considering the effect of the Mercurial magnetosphere traveling with the planet through the magnetic field lines centered on the sun.

“The accepted celestial mechanics, notwithstanding the many calculations that have been carried out to many decimal places, or verified by celestial motions, stands only if the sun, the source of light, warmth, and other radiation produced by fusion and fission of atoms, is as a whole an electrically neutral body, and also if the planets, in their usual orbits, are neutral bodies.” (Worlds in Collision, Epilogue, p. 387). “In the Newtonian celestial mechanics, based on the theory of gravitation, electricity and magnetism play no role.”

The precession of Mercury, the planet closest to the sun, is claimed by the General Relativity theory as one of the proofs of the curvature of space around mass; but since Mercury moves close to the charged sun and actually in the outer reaches of the solar corona, the magnetic field of the sun must act on its motion; therefore the claim of the relativity theory needs reexamination as to its validity. (Already Laplace showed that should a celestial body attracted by its primary as inverse square of distance be subject to another attraction that changes as the inverse cube of distance, a precession by that body would result.)

Things axiomatic need to be repeated again and again over a score of years; the omission to take into account physical realities and calculate their effects should not be placed solely at Einstein’s door; in over sixty years since the publication of the General Theory nobody was disturbed by this situation and in merely a score of years since the space investigation started, with by now probably a thousand artificial satellites having been launched, an experiment intended to observe the behavior of a satellite on the Mercurial orbit and on an orbit perpendicular to it have not been performed or even planned.

An electromagnetic effect must be incalculated in the celestial mechanics, whether its action equals to a substantial part of the gravitational attraction, or to only a minute part: the precision of the celestial motions and the advance knowledge of planetary positions to a small degree of a fraction of a second of the arc, raises the question as to the part the electromagnetic interrelation must account for.

The discovery by John H. Nelson of certain dependence of the radio transmission and reception on the relative position of the planets (March 1951 issue of *RCA Review*) points in the same direction of an electromagnetic interdependence of planetary bodies. If an electromagnetic effect is present between these bodies, the exact masses of the planets must be recalculated, in order to leave also for the newly detected forces a role, small, however yet detectable, in the phenomenon of perturbation, or attraction of a planet by another.

The Ocean SEDIMENTS

Poseidon, lord of the Ocean, was the first to come to my defense. A basic assumption of geology for the past century has been that, though the sea may encroach on land by covering coastal areas with shallow water, the continents and the oceans are primeval; what is now ocean was always ocean and the continents were always land masses, independent of whether they do or do not move slowly, as a certain theory (continental drift) proposes.

In *Worlds in Collision*, the permanency of land and sea was denied. In the presence of external forces, with attendant pulling and shearing, land submerged into the depths of the sea, and sea bottom rose to become land. Prior to certain catastrophes, earlier than those described in *Worlds in Collision*, the highest mountain ridges of the Himalayas must have been under sea, as the fossil content of their rock formations testifies.

Stupendous meteorite showers occurred in the past, and the red clay on the bottom of the sea must have iron and nickel content of meteoric origin. Speaking of the cataclysm that closed the period known as the Middle Bronze II (Middle Kingdom in Egypt), I wrote in *Worlds in Collision* (p. 48):

“One of the first visible signs of this encounter was the reddening of the earth’s surface by a fine dust of rusty pigment. In sea, lake and river this pigment gave a bloody coloring to the water. Because of these particles of ferruginous or other soluble pigment, the world turned red.”

In paroxysms of nature, especially during the catastrophe of the fifteenth century before the present era, ash fell on land and sea.

“Following the red dust, a ‘small dust,’ like ‘ashes of the furnace,’ fell ‘in all the land of Egypt’ (Exodus 9:8), and then a shower of meteorites flew toward the earth. Our planet entered deeper into the tail of the comet. The dust was a forerunner of the gravel.” (*Worlds in Collision*, p. 51).

The ash must be still found on the bottom of the ocean, its final repository.

The Earth was “in a vise” — in the grip of external forces, which altered the terrestrial rotation; the sphere was twisted, and the Atlantic ridge and African rift are only two of the visible signs of the strain to which the Earth was subjected.

“The earth groaned: for weeks now all its strata had been disarranged, its orbit distorted, its world quarters displaced, its oceans thrown upon its continents, its seas turned into deserts, its mountains upheaved, its islands submerged, its rivers running upstream — a world flowing with lava, shattered by meteorites, with yawning chasms, burning naphtha, vomiting volcanoes, shaking ground, a world enshrouded in an atmosphere filled with smoke and vapor. Twisting of strata and building of mountains, earthquakes and rumbling of volcanoes joined in an infernal din.” (*Worlds in Collision*, P. 97).

In *Earth in Upheaval*, I discussed the problem in two chapters, “Poles Displaced” and “Axis Shifted”. In “The Bottom of the Atlantic” and “The Floor of the Seas” I discussed sedimentary rock: it was not deposited evenly through the geological ages but erratically, most rapidly following natural disturbances on land. Further, the sedimentary layers were displaced in global catastrophes. Thus, it follows that the relative thicknesses of the sedimentary layers are not true indices for measuring the age of the oceans.

With such heretical ideas, my work flew in the face of accepted notions in oceanography and marine geology.

The book, *Worlds in Collision*, though already three years in the hands of Macmillan, was not yet off the press when Maurice Ewing, the Columbia University marine geologist, published an account of an expedition to the Atlantic Ocean and the mid-Atlantic ridge. This ridge runs north-south the entire length of the ocean. More than one surprise was in store for the expedition.

Whereas its members expected to find a uniform layer of sediment, the bottom of the ocean revealed no such uniformity, and I quoted from the record of the finds (*Earth in Upheaval*, p. 101: M. Ewing, “New Discoveries on the Mid-Atlantic Ridge,” *National Geographic Magazine*, Vol. XCVI, No. 5 [November 1949]):

“Always it had been thought the sediment must be extremely thick, since it had been accumulating for countless ages... But on the level basins that flank the Mid-Atlantic Ridge our signals reflected from the bottom mud and from the bedrock came back too close together to measure the time between them... They show the sediment in the basins is less than 100 feet thick.”

The absence of thick sediment on the level floor presents ‘another of many scientific riddles our expedition propounded’.” The bottom of the Atlantic Ocean on both sides of the Ridge must have been formed only in recent times.

But even more unexpected was the find of beach sand at a great depth and far away from any land. “One [of the ‘new scientific puzzles’] was the discovery of prehistoric beach sand . . . brought up in one case from a depth of two and in the other nearly three and one half miles, far from any place where beaches exist today.” One of these sand deposits was found twelve hundred miles from land.

Ewing recognized the uncomfortable dilemma: “Either the land must have sunk two to three miles, or the sea once must have been two to three miles lower than now. Either conclusion is startling. If the sea was once two miles lower, where could all the extra water have gone?” I shall return to the problem of the fallen ocean level, which I consider to have been the result of rapid evaporation due to catastrophic heating.

Five months after the publication of *Worlds in Collision*, another marine expedition — led by Professor Hans Pettersson, director of the Göteborg Oceanographic Institute (Albatross Expedition of 1947) — made a preliminary report of the findings of its fifteen-month exploratory voyage. Writing in *Scientific American* (August 1950: “Exploring the Ocean Floor”), Professor Pettersson spoke of evidence of “great catastrophes that have altered the face of the earth.”

“Climatic catastrophes, which piled thousands of feet of ice on the higher latitudes of the continents, also covered the oceans with icebergs and ice fields at lower latitudes and chilled the surface waters even down to the Equator. Volcanic catastrophes cast rains of ash over the sea.” Also, “tectonic catastrophes raised or lowered the ocean bottom hundreds and even thousands of feet, spreading huge ‘tidal’ waves which destroyed plant and animal life on the coastal plains.” Pettersson also found, in addition to the ash, a “lava bed of geologically recent origin covered only by a thin veneer of sediment.”

In the red clay on the bottom of the ocean Pettersson found “a surprisingly high content of nickel” (Pettersson, “Chronology of the Deep Ocean Bed,” *Tellus* 1, 1949). Nickel is not present in sea water and therefore could not have been deposited by water. “Nickel is a very rare element in most terrestrial rocks and continental sediments, and it is almost absent from the ocean waters. On the other hand, it is one of the main components of meteorites.” But the quantity of nickel in the clays in the bottom of the ocean was prodigious. Pettersson assumed very copious falls of meteorites in the geological past. He wrote in his account of the expedition, *Westward Ho with the Albatross* (1953), p. 150:

“Assuming the average nickel content of meteoric dust to be two percent, an approximate value for the rate of accretion of cosmic dust to the whole Earth can be worked out from these data. The result is very high — about 10,000 tons per day, or over a thousand times higher than the value computed from counting the shooting stars and estimating their mass.”

In other words, at some time or times there was such a fall of meteoric dust that, apportioned throughout the entire *assumed* age of the ocean, it would increase a thousandfold the daily accumulation of meteoric dust since the birth of the ocean based upon the estimated present potential rate of accretion; but since the shower of meteorites was most likely an event of short duration, measured in days or weeks only, the “thousandfold” must be changed to some astronomical figure — a figure also dependent upon ascertaining the correct age of the ocean.

In a subsequent publication (“Manganese and Nickel on the Ocean Floor” in *Geochimica et Cosmochimica Acta*, 1959, Vol. 17), Pettersson wrote: “Of all the elements found in deep-sea deposits few have a more puzzling distribution than the two ferrides, manganese and nickel.” Not only their high concentration, much higher than in continental rocks, but especially their vertical distribution appear “most enigmatic.” Pettersson concluded that “the former being largely due to sub-oceanic volcanic action, the latter [was] due to contributions from the cosmos.” It must have occurred by “an unusually heavy incidence from the cosmos.”

In a still more recent paper, Professor Pettersson discussed “The Accretion of Cosmic Matter to the Earth” (*Endeavor*, July 1960): “We found surprisingly large numbers of typical cosmic spherules in deep-sea sediments.” These magnetic particles (in diameter between 0.03 to 0.25 mm.) were not only found in very great numbers in the red clay of the oceanic bed, in the equatorial region of the Pacific, but also all over the world. In the Pacific, “their number varied from about one hundred up to several thousands per kilogram of sediment.” “In general the number of spherules is greatest in the more recent sediments.”

Pettersson observed ash on the bottom of the ocean, and such ash had already been observed by the famous expedition of the last century, that of H. M. S. Challenger (see Sir C. Wyville Thompson, *Voyage of the Challenger*) between the year 1873 and 1876. However, Pettersson failed to observe that the layer of ash is not just distributed here and there on the bottom of the oceans and therefore possibly attributable to volcanic eruptions, but is spread quite uniformly—and the account of an expedition led by J. Lamar Worzel, of Columbia University's Lamont Geological Observatory, brought out this fact. The expedition of the vessel Verna, made in 1958, covered 500,000 square miles of the southwestern Pacific and found white ash between about 750 miles north and 850 miles south of the equator.

Writing in the *Proceedings of the National Academy of Sciences* in its March 15, 1959 issue (vol. 45, pp. 349-355), Worzel made the surmise:

“Since the layer is fairly near the surface and is not discolored and contains nothing but the glassy ash material it must have been laid down fairly quickly.” It must have been deposited in a single act, over a short period, “perhaps within a year or so.”

“The white ash immediately suggests a volcanic origin and the proximity of the Andes suggests the source. However, the great extent of the ash and its shallow cores would imply such a great amount of recent activity for a short time that it may be difficult to ascribe it to the Andes.” “. . . It may be necessary to attribute the layer to a world-wide volcanism or perhaps to the fiery end of bodies of cosmic origin.”

Maurice Ewing, as director of the Lamont Geological Observatory, joined Worzel in describing and evaluating the layer of ash; and on the basis of the random detection of similar ash in other parts of the oceanic world, he wrote (pp. 355-361):

“A single ash layer of 5 to 30 cm. thickness over such a wide area must record a notable event in the history of the area. It could hardly be without some recorded consequence of global extent.

“A re-examination of the file of Vema echograms is now in progress. It shows that sub-bottom echoes, similar to those found in the eastern Pacific, have also been recorded in the South Atlantic and Indian Oceans, [as well as] the Gulf of Mexico.

“The remarkable uniformity of thickness of the Worzel ash layer within the large area which has been cored is additional evidence suggesting that the layer may well have great extent.

“... The total volume of ash must be so great and the mechanism of dispersal so effective that the possibility of world-wide coverage must be considered.

“... Such an event could hardly fail to produce a variety of significant effects global in scale conceivably a cometary collision.”

In the *New York Herald Tribune* of March 31, 1959, Dr. Worzel was quoted as saying that this ash may represent “the remains of a fantastic collision of heavenly bodies from outer space.”

A collision of the Earth with a huge comet was postulated or, at least, preferred to a huge and simultaneous eruption of a multitude of volcanoes, because of the evenness of the layer of white ash. Its position, very close to the surface, almost touching the water layer, makes it appear that the time elapsed since the deposit is very short, geologically speaking.⁹

But only five or six years earlier, the consensus of scientific opinion — and it was expressed in no indefinite terms by my critics—insisted that there never was any collision of the Earth with a comet; furthermore, if such a collision were to occur, there would be no noticeable results. After all, the Earth passed through the tail of Halley’s comet in 1910 and there was no major phenomenon to register, not even flashes of shooting stars (e.g., I. Asimov).

In order to cover the expanse of the oceans with Worzel ash—this is its given name—some more significant collision must have taken place than that which occurred during the approach of Halley’s comet in 1910. A phenomenon observed in the bottom of the oceans bespeaks a collision in which the Earth would have hardly proceeded undisturbed on its path.

RIFTS

In *Worlds in Collision*, it is claimed that the terrestrial sphere underwent great stresses—with resulting rifts and mountain formations—during the global catastrophism that occurred 3400 and 2700 years ago.

Professor T. Y. H. Ma of the National Taiwan University in Formosa published an article in the journal *Oceanographia Sinica* (Vol. II, No. I, September, 1955), in which he claimed a sudden shift in the oceanic bottom several times in the geological past. He found that changes in the sedimentary strata on the sea bottom must be attributed to “changes in latitude due to the sudden total displacements of the solid earth shell and the intermittent readjustments.” The last disturbance of the ocean bottom “ended only 2,600 years ago,” judging from the cores taken at the bottom of the Atlantic, while samples taken in the Pacific allow the displacement to be estimated at about “2,800 years ago.” These figures closely resemble the date of the last cosmic catastrophe fixed in *Worlds in Collision* as 27 centuries ago.

In 1960 Bruce C. Heezen of the Lamont Geological Observatory made known the results of an expedition that, in the previous months, had traversed all the longitudes and, going up and down the latitudes, had discovered a huge and strange formation twice encompassing the globe.

The structure has the form of a large and high ridge, split along its length by a deep canyon.

In a preliminary report published in *Scientific American* of October 1960, Heezen described it thus:

“It is a submarine mountain ridge that runs for 40,000 miles across the bottom of all the oceans and covers an area equal to that of all the continents. The existence of the mid-ocean ridge is a recent discovery of oceanography, and the mapping of it still far from complete. But the stretches that have been charted show a most curious aspect. Down most of its length the ridge is split by a

⁹ See also E. Anders and D. N. Limber, “Origin of the Worzel Deep-sea Ash” in *Nature* 184 (1959), pp. 44-45.

deep canyon, or rift, in which many earthquakes originate. The ridge is apparently the locus of a crack in the crust that runs nearly twice around the earth. The discovery at this late date of the mid-ocean ridge and rift has raised fundamental questions about basic geological processes and the history of the Earth and has even had reverberations in cosmology.”

The Earth was, for some agonizing moments of its past, in a vise; and its coupling action wrenched the Earth and welled up the ridge and split it with a deep rift. The mid-Atlantic ridge known from before is but a segment of the entire serpentine formation. The area of the ridge is so great that it was estimated to equal the area of the five continents.

In *Earth in Upheaval* (1955), I wrote of the shearing action to which the Earth’s crust was subjected when caught in force fields of extraneous origin. In *Worlds in Collision* (1950), I described the same occurrence as reflected in the sundials and water clocks of antiquity that certify to a changed length of the day on solstices, and thus to changed latitudes and a changed inclination of the terrestrial axis to the plane of the ecliptic (Chapter 7). The fact that the Moon does not circle the Earth on its equatorial plane and that this plane is inclined by over 23 degrees to the plane of the ecliptic - whereas the plane of the lunar orbit almost coincides with the plane of the ecliptic — made H. Jeffreys (*The Earth*, 2nd ed., 1929) speculate that the Earth was once, or several times, in a vise that turned its axis in a new direction; and I quoted him in the chapter “Axis Shifted” of *Earth in Upheaval*.

THE OCEAN LEVEL

The stress which resulted in the formation of the immense undersea rifts must have been accompanied by widespread volcanic activity, irruptions of the sea, and changes in the level of the land and in the bottom of the sea. The level of the ocean must have also changed suddenly as a consequence of such upheaval; and in *Worlds in Collision* (Chapter 4), I cited various sources in support of the fact that the sea bottom was heated and rivers and parts of the ocean evaporated ca. 1500 before the present era.

Professor Cecilia Payne-Gaposchkin, astronomer of Harvard University, wrote: “There is no evidence of a wholesale disturbance of the ocean level near 1500 B. C.,” or 3500 years ago (*The Reporter*, March 14, 1950). However, Professor Reginald Daly, geologist of the same university, had claimed since the 1920’s that “a recent worldwide sinking of ocean level” of twenty feet occurred “about 3500 years ago” (Daly, *Our Mobile Earth*, 1926, pp. 177-179).

Subsequent to the publication of *Worlds in Collision* and this first of a series of articles by Gaposchkin on the book, Professor Philip H. Kuenen of Leyden University made the following statement: “In thirty-odd years following Daly’s first paper many further instances have been recorded by a number of investigators the world over, so that this recent shift is now well established.” As to the time of this sudden drop of the ocean level, Kuenen wrote: “... the time can be fixed at roughly 3000 to 3500 years ago” (*Marine Geology*, 1950, p. 538).

In a paper that Dr. Rhodes Fairbridge of Columbia University read before the International Oceanographic Congress on September 7, 1959, he brought evidence from many parts of the world that 6000 years ago the oceans rose forty-five feet; he even expressed the belief that the Great Flood described in *Genesis* is an echo of that oceanic rise.

Dr. Fairbridge found in many places along the eastern coast of the United States, from Maine to North Carolina, drowned forests which had lived 2830 years ago, with a possible error of 200 years. This points to the 8th century before the present era. In *Worlds in Collision*, Part 2, are described global catastrophes of the eighth and beginning of the seventh centuries (-776 to -687) which, while being worldwide, were less violent when compared with the one that occurred in the middle of the second millennium, ca. 3500 years ago, or earlier ones. Such submerged forests are found all around England and Wales and are described in *Earth in Upheaval* (1955), pp. 185ff.

Volcanic activity on the bottom of the oceans and seas must have been stupendous; likewise island building. On the latter we have the testimony of earlier centuries passed on in the writings of classical authors. For example, the origin of many islands as well as changes in the coastline of the Mediterranean are recorded in Pliny's *Natural History*. But, in *Worlds in Collision* I did not cite this and many other ancient chronicles, having presented only a fraction of the historical material I had before me; and again, the material I had before me and left unused is but a fraction of what is to be found in the ancient literature of the world. In *Earth in Upheaval*, however, I was careful not to include any historical or literary material at all, the work being built on the records of modern geology and paleontology.

CONCLUSION

The oceans as we know them are not tens of millions or hundreds of millions years old, as the accepted view assumes. In a sequel to *Worlds in Collision*, dealing with the catastrophic events preceding the second millennium before the present era, I shall discuss the origin of the oceans and shall try to show that their expanse grew greatly after the event known as the Universal Deluge, when cosmic water descended on Earth following the disruption of Saturn.

If this unsupported statement sounds unbelievable, the reader may rest assured that I shall underpin this thesis with as much essential documentation as I did my thesis of the youthful Venus, a newcomer to the planetary family. The provenance of the water will also explain the origin of chlorine in sea water — a problem that plagues marine geologists. For, while the land could provide sodium through erosion by rain, terrestrial rocks do not contain the requisite quantity of chlorine and are quite poor in that element. Some chlorine could have been added from volcanic eruptions but not as much as is needed to form the salt content of oceans and seas. The source of the greater part of the chlorine in oceans is of cosmic origin, and a few more words on this subject are contained in the pages of my book dealing with Saturn.

To the claims in my published work, the ocean responded with invariable support: the sediment on the bottom was not formed uniformly; the nickel content of the red clay in the sediment is of meteoric origin — cosmic dust that rained furiously on the Earth; the Worzel ash also came from cosmic sources; the Heezen ridge and rift are signs of the external torque applied to the Earth, probably more than once; the violent displacement of marine sediment layers, the changing level of the sea, coastal beach at great depths—all speak of catastrophic events temporally so close to us that our minds refuse comprehension.

H. H. Hess and My Memoranda

On August 25, 1972, three years elapsed since the death of Professor Harry Hammond Hess. He died of a heart attack while presiding over a meeting (convened at Woods Hole, Massachusetts) of the Space Science Board of the National Academy of Sciences. The Board had the task of overseeing the activities of the National Aeronautics and Space Administration, with its multi-billion dollar spending. At the Woods Hole meeting Hess had intended to discuss the role of thermoluminescence (TL) tests in the lunar programs, an issue I had discussed with him.

When I moved from Manhattan to Princeton in the early summer of 1952, I became steeped in library work for *Earth in Upheaval*, and the library of Guyot Hall (Princeton's geology department) was a place I frequented. Already known for my *Worlds in Collision* and the discussion it provoked, I caused some curiosity among the numerous faculty members of the department. I do not remember my first contact with Hess, but from our first meeting something in both of us attracted each other.

Hess was the chairman of the department. Once when I mentioned the Vening Meinesz submarine expedition for gravitational measurements in the Caribbean in the 1930's, during which, paradoxically, a positive anomaly was regularly detected and the greater it was the deeper was the sea, or the less mass there was, Hess surprised me by telling that he participated in that expedition.

Another highlight of his career took place during World War II. In command of a naval vessel in the Pacific with certain exploratory assignments, he utilized the opportunity to explore the bottom of the ocean in a certain area. Under the water he discovered flat-topped mountains, which he named "guyots," honoring the late Princeton professor of geology, Arnold Henry Guyot (1807-84),

By the end of the war, Hess was retired from active duty with the rank of a rear admiral. In the university he taught mineralogy and crystallography, but marine geology remained his favored subject.

In November, 1955, *Earth in Upheaval* was published. Soon it was made required reading in paleontology under Professor van Houten at Princeton — along with an antidote: Loren Eiseley's *The Firmament of Time*. Hess several times during those years gave me the opportunity to address the faculty and graduate students of his department. Since from 1953 (when I spoke before the Graduate College Forum of Princeton University) to 1963 practically no college or university or scientific society extended to me an invitation to speak, those appearances at the behest of Hess meant much to me.

He gave me his published paper on guyots. Upon reading it I wrote a rather merciless criticism of his idea that the accumulation of sediment caused the submergence of the sea bottom and with it the submergence of the flat-topped guyots. In his response he showed graciousness.

By mid-1956 preparations for the International Geophysical Year were gaining momentum. On December 5, 1956, I gave to Hess a memo describing, in brevity, several projects for inclusion in

the IGY. (The Year, due to start July 1, 1957, would continue until the end of 1958.) There was not yet a Space Science Board, so I gave the memo to Hess in his capacity as chairman of the geology department. Hess sent the memo to Dr. Joseph Kaplan, one of the scientific organizers of the Year. The answer came from Edward O. Hulburt, another scientist in charge of the program, and it was addressed to the “chairman of the department of physics” at Princeton. The first of the suggested projects — to investigate the earth’s magnetic field above the ionosphere — had been, according to Hulburt, considered by the planning committee. (In my Forum Lecture [October 14, 1953] I had already claimed the existence of a magnetosphere above the ionosphere — the lecture was printed as a supplement to *Earth in Upheaval*.)

Three months after the beginning of the IGY the Russians startled the world by launching the first Sputnik (October 4, 1957), opening the Space Age. I was then on a visit to Israel, my second since I came to the States in July, 1939.

Although Hulburt referred to the plan of measuring the strength of the magnetic field above the ionosphere as considered for the program, the fact is that the discovery of the van Allen belts, the main achievement of IGY, was not anticipated or considered: when no charged particles were registered at a certain altitude, van Allen of the University of Iowa was startled, but one of his co-workers suggested that possibly the recording apparatus was jammed by too many charged particles; the apparatus was modified and the belts were discovered. At the beginning they were featured in the form of two halves of a doughnut; only much later was it recognized that the half on the anti-solar side is stretched far out. But in my memo as also in the Forum lecture, I visualized a magnetosphere reaching as far as the lunar orbit.

Another claim made in my Forum Lecture of 1953 — namely, that Jupiter could be a source of radio signals — was already confirmed in the spring of 1955. I never came out with “claims confirmed” until I read in the *New York Times* that nobody ever thought of Jupiter as a source of radio noises before they were discovered by chance. I turned to Lloyd Motz, Columbia University astronomer, and V. Bargmann, Princeton University physicist, both of whom were entrusted by me with the script of my Forum Lecture soon after its delivery. They wrote a joint letter to *Science*, which published it in the December 21, 1962 issue, concurrent with the yearly convention of the American Association for the Advancement of Science, publisher of *Science*. It almost coincided with the first reports of Mariner II, which had passed its rendezvous with Venus a week earlier, on December 14. The high temperature of Venus was confirmed.

This last announcement was made by Dr. Homer Newell for NASA in February, 1963. The presence of hydrocarbons in the clouds surrounding Venus was also announced as confirmed — this on the basis of the work of Dr. L. D. Kaplan (Jet Propulsion Laboratory): only compounds containing the radical CH (polymerized) could lend to the 15-mile thick cloud the same properties at the -25° F temperature at the top of the cloud and at the $+200^{\circ}$ F temperature at the bottom of the cloud separated by 45 kilometers of lower atmosphere from the sizzlingly hot ground surface of the planet.

I wrote an article, “Venus — A Youthful Planet,” and sent it to the editor of *Science*. I found it back in my mailbox less than 48 hours later, returned unread.

I discussed the case with Hess, and he decided to offer it for publication in the *American Philosophical Society Proceedings*. As a member of the society he was entitled to sponsor a paper by a nonmember. The paper was submitted, and its fate was related by *Yale Scientific Magazine* (April, 1967, p. 8): “The paper was discussed at the editorial board meeting of the Society and caused prolonged and emotional deliberations, with the Board split between those favoring the publication and those opposed to it. For several months a decision could not be reached ... the decision was made, in order to safeguard the very existence of the Board, to delegate the decision on the article to three members of the society, not members of the Board. Their names were not disclosed but on January 20, 1964, Dr. George W. Corner, Executive Officer of the Society and the editor of the *Proceedings*, informed Dr. Hess that the decision had been made to reject the article.

“Subsequently it was also rejected by the *Bulletin of Atomic Scientists*. In that magazine in April, 1964, an abusive article was published by a Mr. Howard Margolis, attacking Velikovsky and his work. The editor of the *Bulletin*, Dr. Eugene Rabinowitch, in a letter to Professor Alfred de Grazia, editor of the *American Behavioral Scientist*, offered Velikovsky an opportunity to reply with an article ‘not more abusive’ than that of Margolis, or, instead, to have some of his views presented in the *Bulletin* by some scientist of repute. Then Professor H. Hess submitted the article “Venus — A Youthful Planet,” to Dr. Rabinowitch. The latter returned it with the statement that he did not read Velikovsky’s book, nor the article.”

In July *Harper’s* printed an article by Eric Larrabee calling for an “agonizing reappraisal” of my work. Menzel of Harvard College Observatory, who not so long previously had revoked his earlier estimate of Venus’ temperature as much too high, now wrote in *Harper’s* that “hot” is a relative term and liquid helium is hot in relation to liquid hydrogen. As to my claim concerning the magnetosphere, Menzel argued that since I claimed that the magnetosphere reaches as far as the lunar orbit, I made a wrong prediction. The magnetosphere, he said, does not reach more than a few terrestrial radii, whereas the moon is 60 terrestrial radii distant.

Hess was adversely impressed by the attitude of the scientific community toward me and my work; still subscribing to the accepted uniformitarian doctrine, he had sympathy for my independent stand. He wrote a letter that was intended for public record and which Doubleday incorporated in its “Report on the Velikovsky Controversy,” printed in the Book Review Section of the *New York Times* (August 2, 1964).

While a debate was going on in several issues of *Harper’s*, the Australian physicist/cosmologist, V. A. Bailey, joined the fracas and accused Menzel of pre-space age thinking.

Hess, now president of the American Geological Society and chairman of the Space Science Board, suggested that I put together a program for space investigation. I responded without delay; the memo of September, 1963, resulted.

About that time de Grazia published a special issue of the *American Behavioral Scientist* dealing with the reception of my work. When he came to see me, Hess came too.

Once or twice I asked Hess to organize a panel of members of various faculties of Princeton University that would investigate what was right and what was wrong in my theory and what was

proper or improper in the attitude of my critics. Before he decided whether to follow this course (perhaps, expecting a negative attitude by faculty members, he tarried), an initiative came from Dr. Franklin Murphy, at that time chancellor of the University of California at Los Angeles. He asked UCLA's geophysicist, Professor Louis Slichter, to organize a committee for the same kind of inquiry I had proposed to Hess. Murphy's initiative, however, foundered and the story needs to be told separately. It embraced the period from January to November, 1964.

In January, 1965, Hess took the initiative to organize the Cosmos and Chronos Study and Discussion Group, and he placed in the Bulletin of the University an announcement of the first open discussion. Originally we planned a debate on evolution based on the uniformitarian principle vs. evolution based mainly on cataclysmic events. My opponent was to have been Princeton professor of biology, Colin Pittendrigh. "There was a mutual respect between us (earlier he had visited me and also inscribed to me a biology text which he co-authored with G. G. Simpson, my early antagonist), but Pittendrigh insisted that the problem of extinction in the animal kingdom should not be a part of the debate. I could not see how the two parts of the evolutionary problem — the evolution of new species and the extinction of the old — could be separated in a meaningful debate. It appeared that the friendly relations between us were in jeopardy. Hess, without fanfare, offered to be my opponent.

The debate took place in the auditorium of Guyot Hall and fared well. Next, Professor Lloyd Motz came from Columbia University to debate me on astronomical subjects. The third open debate was between me and philosopher Walter Kaufmann of the Princeton faculty. Other study groups spontaneously organized themselves on various campuses. The story of the first four or five years of Cosmos and Chronos and what changes in the structure of the organization I had to demand is a story by itself;

In the fall of 1966 I spoke in the new auditorium of the Wilson School of Princeton University, under the aegis of the Princeton chapter of the American Institute of Aeronautics and Astronautics. The lecture was described by Walter Sullivan, science editor of the *New York Times*, in his column of October 2, 1966. As he described it, he first visited Hess to find out whether Velikovsky is a person of integrity. Hess assured him of my complete integrity and added something about my memory, ascribing to me more than I deserve.

An unusual memory was actually one of Hess' own characteristics. Things spoken or letters read were remembered by him years later. Once, when I exhorted him to reread a chapter in *Earth in Upheaval*, he replied that he knew the book by heart. His many very large tables that served him as desks were covered with stacks of papers, but it seemed that he could always find the necessary document; he was helped by a devoted secretary, Mrs. Knapp, who, it seems, also relied on his memory.

Despite his heavy schedule (he never stopped teaching crystallography), Hess was available for many a demand on his time. I remember the case of an uneducated but dedicated man who, living in Michigan, collected many rocks, obviously burned, and wrote me regularly of his belief that the lake was scooped out by an asteroid impact. He mailed me, at intervals, boxes with stones. I sent some of them to a scientist at the University of Pittsburgh whom I knew, and brought some others to Hess. The former did not answer; the latter took a few of them to investigate their possible meteoritic nature.

Hess ascribed the reversal of the rocks' magnetic orientation to a spontaneous process in the minerals, as he had claimed in debate with me at my occasional lectures at the geology department. But when he finally realized that such spontaneous reversals could not occur simultaneously in rocks of various compositions, he volunteered to tell me that he was wrong.

When, years after my first memo of December 5, 1956, he read or heard a paper concerning the reversal of the direction of winding in fossil vines and shells from both southern and northern hemispheres, he was pleased to let me know that the claims the IGY would not investigate were confirmed by independent research.

In 1967 I gave him a memorandum on radioactivity hazards for astronauts in several localized areas of the moon and Mars, results of interplanetary discharges. Dr. Homer Newell of NASA sent the memo to scientists on the staff who he thought would be the ones to consider the subject. By that time Hess and I started to call one another by our first names.

In 1968 Hess was named by the Italian government and Academy of Sciences the recipient of a major prize (in monetary value, approaching the Nobel prize) for his old work on the guyots. Despite all the distinctions he received, he remained a quiet and humble man. I never heard him speak in a loud voice. He did not pull or push and, which was unusual in the academic atmosphere of the time, he was sought out for his fairness.

Not long before his death he purchased a new home. Until then he had lived in a university house on Fitzrandolph Street. The house, built with its gables like a chalet, was occupied by Woodrow Wilson when he was president of Princeton University. At one of my rare visits, Hess drew my attention to the book cases built at Wilson's behest.

The last and possibly the most exciting event was quickly approaching. Hess, usually shy of publicity, made himself available to the press to state his belief that water in quantity would be found under the lunar surface. I remember how he showed me a winding rill or rift photographed on the moon and wished me to agree with him that it was caused by running water. I discussed with him my views, namely that the moon was once showered by water of the universal Deluge, but that all of it or almost all of it dissociated before the later cosmic catastrophes. The face of the moon we see was formed in those later catastrophes.

On May 19 I wrote down a few of my advanced claims concerning the moon and handed it to Hess' research assistant, who strongly supported the view that large water reservoirs lay under the moon's surface. Hess said to me, "this time you will be wrong." Until then, closely following my record, he found that all my expectations ("predictions") turned out to be true. Once, on our way from Guyot Hall to our respective homes, he ascribed my record to intuition. When I asked which of my claims does not follow from my thesis, he replied, "noises from Jupiter." He was right, but only to the extent that I have not yet published the story of the earlier cataclysms, promised in the final chapter of *Worlds in Collision*.

The events surrounding the first manned landing on the moon had a dramatic urgency, and they, too, need to be recorded separately. My two telephone conversations in which I tried to obtain Hess' support for thermoluminescence tests of lunar core extracts, as also envisioned in my

article in the New York Times on the evening of the first lunar manned landing, can be read in the correspondence.

I saw Hess once more — he was with his secretaries and assistants, preparing for the Woods Hole meeting. He was not in a cheerful mood — that morning the news came that hydrocarbons (petroleum derivatives) were discovered on the moon, but no water yet. (Now, almost three years later, signs of the one-time presence of water have been detected.) He was, it appeared to me, gloomy.

About half a year earlier he had suffered a heart attack. He was always a chain smoker. The load of work, the excitement of the last few weeks, and possibly a discouragement, but quite probably his premonition that he would not be able to witness the entire lunar program of many landings, must have weighed heavily on him.

On the morning of August 26, 1969, I picked up a newspaper at the Princeton Junction railway station and saw Hess' friendly face on a page carrying a eulogy.

The day the university arranged a memorial service in its chapel, I was delivering a lecture to the faculty of the Ocean County College. I spoke of Hess.

On October 21, exactly three months after the first landing on the moon, at my initiative, the geophysical department (the new name for the geology department), together with the Cosmos and Chronos Study Group, arranged a memorial lecture at the auditorium of Guyot Hall. The opening part of my lecture, "From Sputnik to Apollo XI," was dedicated to Hess.

In Hess' passing I lost the only member of the scientific elite who demanded a fair treatment for me and my work. When in November the assistant to the president of the university came to see me, I spoke of Hess and could not hide the tears in my eyes. For the rest of 1969 I felt depressed.

Of people who were prominent in their fields and who, since the beginning of my work and through the years showed me more than casual interest and sympathy, I name Robert Pfeiffer, orientalist and Biblical scholar (d. 1958); Horace M. Kallen, philosopher and educator; Walter S. Adams, astronomer (d. 1956); Albert Einstein (d. 1955); and Harry Hess, who died in his sixty-fourth year, three years ago. Kallen alone of all of them is alive, having these days reached the venerable age of ninety, still active as writer and lecturer, with time having dimmed none of his mental abilities.

They were few, but each of them was great as a human being.

Velikovsky to Hess - December 5, 1956

Hess to Velikovsky - January 2, 1957

Hulburt to Hess - January 18, 1957

Hess to Velikovsky - March 15, 1963

Velikovsky to Hess - September 11, 1963

Velikovsky to Hess - March 14, 1967 (Memorandum)

Velikovsky to Hess - May 19, 1969 (Memorandum)

Velikovsky to Hess - July 2, 1969

Velikovsky to Hess - August 7, 1969

On Prediction in Science

In order to bring into proper focus the significance of correct prediction in science, I offer at the start a short survey of the most celebrated cases, and it is not by chance that almost all of them come from the domain of astronomy. These cases are spectacular and, with one or two exceptions, are well known.

The story of scientific “clairvoyance” in modern astronomy starts with Johannes Kepler, a strange case and little known. When Galileo, using the telescope he had built after the model of an instrument invented by a Danish craftsman, discovered the satellites circling Jupiter, Kepler became very eager to see the satellites himself and begged in letters to have an instrument sent to Prague; Galileo did not even answer him. Next, Galileo made two more discoveries, but before publishing them in a book, he assured himself of priority by composing cryptograms, not an uncommon procedure in those days: statements written in Latin were deliberately reduced to the letters of which the sentences were composed, or, if the author of the cryptogram so wished, the letters were re-assembled to make a different sentence. The second way was chosen by Galileo when he thought he had discovered that Saturn is “a triple” planet, having observed appendices on both sides of Saturn, but not having discerned that they were but a ring around the planet, a discovery reserved for Christian Huygens in 1659, half a century later. Kepler tried to read the cryptogram of letters recombined into a non-revealing sentence, but did not succeed. He offered as his solution: “Salute, fiery twin, offspring of Mars” (“Salve, umbistineum geminatum Martia proles”). Of this, Arthur Koestler in *The Sleepwalkers* (1959) wrote (p. 377): “He [Kepler] accordingly believed that Galileo had discovered two moons around Mars.” But Galileo did not discover them and they remained undiscovered for more than two hundred fifty years. Strangely, Koestler passes over the incident without expressing wonder at Kepler’s seeming prescience.

As I have shown in *Worlds in Collision* (“The Steeds of Mars”) the poets Homer and Virgil knew of the trabants of Mars, visualized as his steeds, named Deimos (Terror) and Phobos (Rout). Kepler referred to the satellites of Mars as being “burning” or “flaming”, the same way the ancients had referred to the steeds of Mars.

Ancient lore preserved traditions from the time when Mars, Ares of the Greeks, was followed and preceded by swiftly circling satellites with their blazing manes. “When Mars was very close to the earth, its two trabants were visible. They rushed in front of and around Mars; in the disturbances that took place, they probably snatched some of Mars’ atmosphere, dispersed as it was, and appeared with gleaming manes” (*Worlds in Collision*, p. 230).

Next, Galileo made the discovery that Venus shows phases, as the Moon does. This time he secured his secret by locking it in a cryptogram of a mere collection of letters—so many A’s, so many B’s, and so on. Kepler again tried to read the cryptogram and came up with the sentence: “Macula rufa in Jove est gyratur mathem etc.” which in translation reads: “There is a red spot in Jupiter which rotates mathematically.”

The wondrous thing is: how could Kepler have known of the red spot in Jupiter, then not yet discovered? It was discovered by J. D. Cassini in the 1660’s, after the time of Kepler and Galileo. Kepler’s assumption that Galileo had discovered a red spot in Jupiter amazes and defies every

statistical chance of being a mere guess. But the possibility is not excluded that Kepler found the information in some Arab author or some other source, possibly of Babylonian or Chinese origin. Kepler did not disclose what the basis of his reference to the red spot of Jupiter was — he could not have arrived at it either by logic and deduction or by sheer guesswork. A scientific prediction must follow from a theory as a logical consequence. Kepler had no theory on that. It is asserted that the Chinese observed solar spots many centuries before Galileo did with his telescope. Observing solar spots, the ancients could have conceivably observed the Jovian red spot, too. Jesuit scholars traveled in the early 17th century to China to study Chinese achievements in astronomy.

Kepler was well versed in ancient writings, also knowledgeable in medieval Arab authors; for instance, he quoted Arzachel to support the view that in ancient times Babylon must have been situated two and a half degrees more to the north, and this on the basis of the data on the duration of the longest and shortest days in the year as registered in ancient Babylon.¹⁰

Jonathan Swift, in his *Gulliver's Travels* (1726) tells of the astronomers of the imaginary land of the Laputans who asserted they had discovered that the planet Mars has “two lesser stars, or satellites, which revolve about Mars, whereof the innermost is distant from the center of the primary planet exactly three of [its] diameters, and the outermost Five; the former revolves in the space of ten hours, and the latter in twenty-one-and-a-half; so that the squares of their periodical times are very near in the same proportion with the cubes of their distance from the center of Mars, which evidently shows them to be governed by the same law of gravitation that influences the other heavenly bodies.”

About this passage a literature of no mean number of authors grew in the years after 1877, when Asaph Hall, a New England carpenter turned astronomer, discovered the two trabant of Mars. They are between five and ten miles in diameter. They revolve on orbits close to their primary and in very short times: actually the inner one, Phobos, makes more than three revolutions in the time it takes Mars to complete one rotation on its axis; and were there intelligent beings on Mars they would need to count two different months according to the number of satellites (this is no special case — Jupiter has twelve moons and Saturn ten), and also observe one moon ending its month three times in one Martian day. It is a singular case in the solar system among the natural satellites that a moon completes one revolution before its primary finishes one rotation.

Swift ascribed to the Laputans some amazing knowledge—actually he himself displayed, it is claimed, an unusual gift of foreknowledge. The chorus of wonderment can be heard in the evaluation of C. P. Olivier in his article “Mars” written for the *Encyclopedia Americana* (1943):

“When it is noted how very close Swift came to the truth, not only in merely predicting two small moons but also the salient features of their orbits, there seems little doubt that this is the most astounding ‘prophecy’ of the past thousand years as to whose full authenticity there is not a shadow of doubt.”

The passage in Kepler is little known—Olivier, like other writers on the subject of Swift’s divination, was unaware of it, and the case of Swift’s prophecy appears astounding: the number

¹⁰ The reference is found in the collected works of Kepler (*Astronomica opera omnia*, ed. C. Frisch, vol. VI, p. 557) published in 1866.

of satellites, their close distances to the body of the planet, and their swift revolutions are stated in a book printed one hundred and fifty years to the year before the discovery of Asaph Hall.

Let us examine the case. Swift, being an ecclesiastical dignitary and a scholar, not just a satirist, could have learned of Kepler's passage about two satellites of Mars; he could also have learned of them in Homer and Virgil where they are described in poetic language (actually, Asaph Hall named the discovered satellites by the very names the flaming trabants of Mars were known by from Homer and Virgil); and it is also not inconceivable that Swift learned of them in some old manuscript dating from the Middle Ages and relating some ancient knowledge from Arabian, or Persian, or Hindu, or Chinese sources. To this day an enormous number of medieval manuscripts have not seen publication and in the days of Newton (Swift published *Gulliver's Travels* in the year Newton was to die), as we know from Newton's own studies in ancient lore, for every published tome there was a multiplicity of unpublished classical, medieval, and Renaissance texts.

That Swift knew Kepler's laws, he himself gave testimony, and this in the very passage that concerns us: ". . . so that the squares of their periodical times are very near in the same proportion with the cubes of their distance from the center of Mars" is the Third Law of Kepler.

But even if we assume that Swift knew nothing apart from the laws of Kepler to make his guess, how rare would be such a guess of the existence of two Martian satellites and of their short orbits and periods? As to their number, in 1726 there were known to exist: five satellites of Saturn, four of Jupiter, one of Earth, and none of Venus. Guessing, one could reasonably say: none, one, two, three, four, or five. The chance of hitting on the right Figure was one in six, or the chance of any one side of a die's coming up in a throw. The smallness of the guessed satellites would necessarily follow from their not having been discovered in the age of Newton. Their proximity to the parent planet and their short periods of revolution were but one guess, not two, by anybody who knew of the work of Newton and Kepler. The nearness of the satellites to the primary could have been assumed on the basis of what was known about the satellites of Jupiter and Saturn, Io, one of the Galilean (or Medicean) satellites of Jupiter, revolves around the giant planet in 1 day 18.5 hours (the satellite closest to Jupiter was discovered in 1892 by Barnard and is known as the "fifth satellite" in order of discovery; it revolves around Jupiter, a planet ten thousand times the size of Mars, in 11.9 hours). The three satellites of Saturn discovered by Cassini before the days of Swift – Tethys, Dione and Rhea – revolve respectively in 1 day 21.3 hours, 2 days 17 hours, and 4 days 12.4 hours. (Mimas and Enceladus, discovered by Herschelin 1789, revolve in 22.6 hours and 1 day 8.9 hours.) The far removed satellites of Jupiter were not yet discovered in the days of Newton and Swift.

It remains to compare the figures of Swift with those of Hall: there was no true agreement between what the former wrote in his novel and what the latter found through his telescope. For Deimos, Swift's figure, expressed in miles from the surface of Mars, is 18,900 miles; actually it is 12,500 miles; Swift gave its revolution time as 21.5 hours—actually it is 30.3 hours. For Phobos, Swift's figures are 10,500 miles from the surface and 10 hours revolution period, whereas the true Figures are 3,700 miles and 7.65 hours. Remarkable remains the fact that for the inner satellite Swift assumed a period of revolution, though not what it is, but shorter than the Martian period of rotation, which is true. However, Swift did not know the rotational period of Mars and therefore he was not aware of the uniqueness of his figure. If he were to calculate as an

astronomer should, he would either have decreased the distance separating the inner satellite from Mars - a distance for which he gave thrice its true value - or increased its revolution period to comply with the Keplerian laws by assuming the specific weight of Mars as comparable with that of Earth. But Swift had no ambitions toward scientific inquiry in his satirical novel.