**The Causal Effect of Military Conscription on Crime**†*

(Short title = Military Conscription and Crime)

Randi Hjalmarsson and Matthew J. Lindquist

September 2018

*Abstract*

We study the causal effect of mandatory military conscription in Sweden on the criminal behaviour of men born in the 1970s. We find that military service significantly increases post-service crime (overall and across multiple crime categories) between ages 23 and 30. These results are driven primarily by young men who come from low socioeconomic status households and those with pre-service criminal histories, despite evidence of a contemporaneous incapacitation effect of service for the latter group. Much of this crime inducing effect can be attributed to negative peer effects experienced during service. Worse post-service labour market outcomes may also matter.

Keywords: Conscription, Crime, Criminal Behaviour, Draft, Incapacitation, Military Conscription, Military Draft.

JEL: H56, J08, K42.

† Corresponding author: Randi Hjalmarsson, Department of Economics, University of Gothenburg, Vasagatan 1, SE 405 30, Gothenburg, Sweden; randi.hjalmarsson@economics.gu.se

* We would like to thank Philip Cook and seminar participants at the 8th Annual Transatlantic Crime Workshop, the CEPR Labour Workshop, City University (London), the Helsinki Center of Economic Research, Linnaeus University, Maastricht University, the Swedish Institute for Social Research (SOFI), Research Institute for Industrial Economics (IFN), Society of Labor Economics 2017, the Tinbergen Institute, and University of Bergen for their helpful comments and suggestions. Hjalmarsson gratefully acknowledges funding support from Vetenskapsrådet (The Swedish Research Council), Grants for Distinguished Young Researchers. Lindquist would also like to gratefully acknowledge funding support from Vetenskapsrådet (The Swedish Research Council).

Young men in more than 60 countries around the world face the prospect of mandatory military conscription.[1] This occurs at a critical juncture in a young adult's life – when he is at the peak of the age-crime profile, making decisions about higher education, and entering the labour market. It is, therefore, not surprising that conscription remains hotly debated. In fact, a number of European countries have recently abolished it (France, 1996; Italy, 2005; and Germany, 2011), while others have had failed referendums (Austria and Switzerland in 2013). Most prominently, Sweden abolished mandatory conscription for men in 2010 and then, in an abrupt about face, reinstated it for males and females in 2017 (but with the intention of a much lower share of the population serving than previously). Yet, despite a growing body of academic literature, there is little consensus about the impact of this potentially life transforming event.

Our paper contributes to this debate by utilizing individual administrative records and a quasi-experimental research design to identify the causal impact of mandatory military conscription in Sweden on crime, both concurrent with (incapacitation) and after conscription. Our subsequent investigation of potential mechanisms produces several additional policy relevant insights. We argue that the Swedish case provides an important piece of the overall picture needed for a discussion of the (intended and unintended) consequences of modern-day mandatory military service. Further, we argue that other social programs, such as military-type "boot camps" for delinquent boys, juvenile jails, extensions of mandatory education, and labour market training programs, all share several important features with Sweden's mandatory military conscription. They may all affect outcomes through (i) incapacitation effects, (ii) human capital accumulation (or depreciation), and (iii) peer effects. As such, programs like these may also produce effects (both good and bad) similar to those uncovered in this paper.

Military conscription may affect both contemporaneous and future criminal behaviour through a number of channels. Contemporaneous crime may decrease if conscription

[1]See the CIA's World Factbook (https://www.cia.gov/library/publications/the-world-factbook/fields/2024.html) and http://chartsbin.com/view/1887 for a summary of this data.

incapacitates, i.e. keeps otherwise engaged and isolated from society, young men. On the other hand, conscripts are not under 24-hour supervision and can still commit crimes "after hours" and experience increased social interactions, which could result in an increased propensity to commit highly "social" crimes (a concentration effect).[2] If conscription does "incapacitate" potential criminals, then this could reduce post-service crime by putting conscripts on a new path of lower criminal intensity. Alternatively, the promotion of democratic values and obedience and discipline training that one receives may decrease crime by helping focus young men at this high risk age. Others argue that exposure to weapons and desensitization to violence, especially during wartime, may exacerbate criminal tendencies (Grossman, 1995).

Military conscription may also positively or negatively impact crime through its impact on education and the labour market (Becker, 1968). Crime could decrease if conscription extends a conscripts' social networks, is viewed as a positive signal of quality by employers, or improves his marketable skills (e.g. training as mechanics, cooks and medics), health, or physical fitness. However, crime may increase if conscription interrupts a continuous educational path, delays entry into the labour market, and reduces future labour market opportunities. Finally, exposure to a new peer group may have either positive or negative effects on criminal behaviour, depending on the relative characteristics of the new and old peer groups.

The existing research yields mixed results, with respect to both labour market and crime outcomes. Angrist's (1990) seminal study found that Vietnam draftees in the U.S. had lower earnings than non-draftees. Subsequent papers (Angrist and Chen, 2011; Angrist, Chen, and Song, 2011) find that this gap closes over time; by age 50, draftees are on par with non-draftees.[3] With respect to peacetime service, Imbens and van der Klaauw (1995) find lower wages for Dutch veterans, Grenet et al (2011) and Bauer et al (2009) find no impact on wages

[2] This parallels the school crime literature (Jacob and Lefgren, 2003; Luallen, 2006) which finds that schooling incapacitates property crime but exacerbates violent crime (from increased social interactions).
[3] Likewise, Siminski's (2013) study of Australian Vietnam draftees finds a negative employment effect.

for British and German cohorts coming of age just after the abolition of conscription, and Card and Cardoso (2012) find a small positive effect on earnings for low-educated men in Portugal.[4] Bingley et al. (forthcoming) find large earnings losses for high ability men in Denmark, while Grönqvist and Lindqvist (2016) find that officer training in the military significantly increases the probability of becoming a civilian manager.[5]

Few papers study the effect of conscription on crime in a quasi-experimental setting.[6] Those studying Vietnam Veterans in the U.S. find some evidence that conscription increases violent crimes (Rohlfs, 2010; Lindo and Stoecker, 2012; Wang and Flores-Lagunes, 2017), while Siminski et al (2016) do not find a comparable effect in Australia.[7]

Work has also been done on the effects of peacetime conscription on crime. In Argentina, where males are randomly assigned eligibility based on the last three digits of their national identity number, Galiani et al. (2011) find that conscription increases crime, especially property and white collar crimes, and decreases labour market outcomes; these effects are even larger for wartime draftees. For a subset of the 1964 Danish birth cohort, Albaek et al. (2017) find that service reduces property crime among men with previous convictions for up to four years (starting from the first year of service), though they cannot cleanly estimate an incapacitation effect separately from a post-service effect. Using the same lottery but data on the full Danish population of more recent cohorts born in the 1970s and 1980s, Vincent Lyk-



---

[4] Maurin and Xenogiani (2007) use the French abolition of conscription to study the returns to schooling.

[5] Hanes et al. (2010) find a positive effect of service on earnings. Albrecht et al. (1999) study of the negative returns to time spend out of work also report regression coefficients that (in some specifications) show a positive return to Swedish military service. Neither paper specifically addresses the issue of selection into military service.

[6] Beckerman and Fontana's (1989) survey of the early literature finds that Vietnam veterans do not have higher arrest rates than non-veterans. Studies (Yager et al, 1984; Resnick et al, 1989; and Yesavage, 1983) that find a positive effect on violent crime are often restricted to those in combat or with mental health problems.

[7] Lindo and Stoecker (2014) find large increase in violent crime for whites, but reductions in property crime. For non-whites they report insignificant effects. However, they argue that this is most likely due to the small first stage for non-whites and imprecise estimates; they cannot rule out larger effects for non-whites than whites. Revisiting this question, Wang and Flores-Lagunes (2017) argue that if you correct for the direct effect of the lottery on crime (through avoidance behaviors), then the effects of military service on crime in the complier population are most likely zero, except for non-violent offending among non-whites. Interestingly, they also find an increase in both violent and non-violent crime among Vietnam volunteers born in 1951 and 1952, but only among those from low SES backgrounds. This new result, in fact, echoes are own findings of heterogeneous effects of military service.

Jensen (2018) finds (on average) no crime inducing effects of peacetime service. She does, however, report crime increasing effects for juvenile offenders among her two oldest cohorts and argues that this may be due to the poor labour market conditions that these recruits faced when leaving service.

The current paper addresses a number of limitations of the existing research. First, we look at the effect of conscription on more modern cohorts who come of age in the 1990s.[8] Second, our sample from Sweden's Multigenerational Register (representing about 70% of the population) allows us to study crimes committed when the men are young adults (i.e. before age 30).[9] Methodologically, we apply a new research design to identify the causal effects of service on crime in a country that does not use a lottery. Finally, and in contrast to all existing literature, we directly estimate the incapacitation effects of service.

Our identification strategy hinges on the unique details of the Swedish draft system. Sweden's mandatory military conscription dates back to 1901 and was abolished in 2010, after a gradual decline that began upon the end of the Cold War. For most of this period, all male citizens underwent an intensive drafting procedure at age 18, including tests of physical and mental ability. These test results were reviewed by a randomly assigned officiator, who determined the "intended" service category of each conscript (i.e. medic, infantry soldier, tank driver, etc.). Each healthy young man left the test centre with a service category and date in hand, expecting to serve.

Importantly, due to the downsizing of the armed forces during the 1990s, troop orders were changed by the military ex post. That is, the military did not need and could not absorb all



[8] Anderson and Rees (2015) also study more modern data – namely those deployed in the Iraq war from Fort Carson, Colorado from 2001 to 2009, concluding that never-deployed units have a greater impact on public safety than units recently returned from combat. Bingley et al. (forthcoming) study the labour market outcomes of Danish men born 1976-1983. In an attempt to explain their labour market findings, they do actually run a regression with crime as an outcome and report a zero effect of service on non-vehicular crime for all men aged 26-35. As discussed in the main text, Vincent Lyk-Jensen (2018) also uses these same Danish cohorts in her study.

[9] Both of the Danish studies (Albaek et al. (2017) and Vincent Lyk-Jensen (2018)) can do this as well.

of the troops initially ordered from the draft authority. Thus, many of the above mentioned service categories were oversubscribed. To decide which recruits to release from service from within each service category, each officiator spoke for or against "their own" recruits in quarterly meetings across and within draft offices. We use the exogenous variation in the chance of serving that arises from the as good as random assignment to an officiator who is more (less) decisive and influential in these meetings than others, thereby by placing a relatively high (low) share of their own recruits into actual service.

Specifically, we instrument for service with the leave out annual mean service rate of the assigned officiator, and argue that the instrument is both valid and relevant after conditioning on county by test year and test month by test office fixed effects. With regards to validity, we provide both anecdotal and empirical evidence of random assignment to officiators as well as evidence supporting the validity of our exclusion restriction. The first stage results are such that assignment to an officiator whose other testees are 10% more likely to serve increases one's chance of service by 6.6 percentage points, or 8.8% relative to the mean service rate of 75%. Lastly, to ensure identification of the local average treatment effect (LATE) of service in the presence of heterogeneous effects, we supply evidence consistent with the monotonicity assumption. Positive and significant first stage estimates are seen for a wide variety of heterogeneous subsamples when using both baseline and reverse sample instruments; the latter classifies officiators based only on individuals outside of the specific sub-sample.

The baseline results are striking: military service significantly increases both the likelihood of crime and the number of crimes between ages 23 and 30 (a cleanly defined post-service period). Specifically, serving in the military increases the chance of post-service conviction by 6.5 percentage points or 32%. These results are driven mainly by those from disadvantaged backgrounds with respect to criminal history or father's education. We do see some effects on men from advantaged backgrounds, but these are mostly traffic offenses.

Effects on the more disadvantaged groups are driven by more serious offenses: weapons, violent, theft, and drugs and alcohol. We also find evidence of a contemporaneous incapacitation effect of service on those with a prior criminal history, though the post-service results suggest it is not enough to remove this high risk population from a path of future crime.

Importantly, our results do not support the hypothesis that military service can "straighten out" troubled young men. Instead, military service in our context compounds pre-existing behavioural problems. This contrasts Albaek et al. (2017), who studied an older Danish cohort, but is consistent with results reported in more recent work for Denmark (Vincent Lyk-Jensen, 2018) and the United States (Wang and Flores-Lagunes, 2017). Our results are also consistent with the literature evaluating "military boot camps", which shows that these types of programs do not reduce recidivism among juvenile offenders or young adults.[10] As such, our own results may generalize beyond the specific setting that we study.

How does military service affect crime? We show that one feasible mechanism is the negative effect that military service appears to have on the labour market outcomes of young men from disadvantaged backgrounds; in contrast, there is evidence of a positive effect on those from advantaged backgrounds. However, given the well-known simultaneity issues in studying the impact of the labour market on crime (and vice versa), we cannot rule out that it is the impact of the military on crime, especially for this high risk criminal history subsample, that mediates the effect of service on the labour market. Lastly, we demonstrate that a second feasible mechanism is peer effects, since individuals with pre-service criminal histories and from low socioeconomic status households were concentrated together in units when conscripted, leading to potentially intense social interactions. Using an identification strategy borrowed from the economics of education literature, in which we rely on idiosyncratic variation in pre-service crime across successive cohorts within the same regiment and rank, we

[10] See e.g. Layton et al. (2001), Meade and Steiner (2010), and MacKenzie and Farrington (2015) for comprehensive reviews and meta-analyses.

find a strong causal relationship between peer pre-service criminal history and an individual's post service crime, but only for those individuals from disadvantaged backgrounds. As such, negative peer interactions appear to be one of the main explanations for the unintended negative consequences of military service.

The remainder of the paper proceeds as follows. Section 1 provides the institutional details of Sweden's draft system that are necessary to understand our identification strategy. We present the data in Section 2. Section 3 presents the officiator-based instrumental variable strategy, including tests of relevance, conditional independence, excludability, and monotonicity. Section 4 presents the main estimates of the causal effects of military service on post-service and contemporaneous crime. Section 5 discusses mechanisms that may explain the large effect of service on crime, highlighting the potential roles of the labour market and negative peer effects. Section 6 summarizes our results and discusses potential policy implications.

## 1. Mandatory Conscription in Sweden

### *1.1. Background*

Mandatory military service in Sweden dates back to 1901. Shortly after turning 18, all Swedish male citizens underwent an intensive drafting procedure, including tests of cognitive ability and physical and mental health. Test results were used to determine whether a recruit was healthy enough to serve. All healthy male citizens were required by law to serve and this law was strictly enforced. Test results were also used to assign each recruit to a specific service category, denoting a job task (medic, tank driver, etc.), rank and military unit. Most individuals enlisted at age 19 or 20, for 7 to 15 months, depending on unit and rank. Individuals were trained in three stages: soldiering skills, skills specific to each line of service (army, navy, air force, coastal artillery), and joint training exercises to prepare for wartime deployment.

The number of men placed into military service started decreasing upon the end of the Cold War and accelerated after the fall of the Berlin Wall in 1989. Figure 1 shows (i) the share of men born in Sweden (by birth cohort) who were called and tested by the Swedish Draft Authority, (ii) the share deemed fit to serve and placed in service categories, and (iii) the share who actually served. Roughly 95% of all men born before 1979 were called and tested; the remainder were typically individuals with severe mental or physical problems that exempted them from service, or non-citizens. Over time, as the number of men actually required to serve fell, so did the number who were called and tested.

On July 1, 2010, Sweden adopted an all voluntary military service. Due to an inability to attract a sufficient number of quality volunteers, the Swedish Parliament voted on March 2, 2017, to reinstate mandatory military conscription for all Swedish citizens (male and female) born 1999 or later. It is intended that a much lower share of the population will serve than in the past. The first of these new conscripts will enter basic training in the summer of 2018.

### *1.2. The Testing Procedure, the Test Office, and the Role of the Test Officiator*

This paper focuses on men drafted between 1990 and 1996. At this time, each young man was called to his regional test office shortly after turning 18. The specific date was based only on month and year of birth, municipality of residence at age 17, and, in some cases, the expected date of high school graduation.[11] In 1990, there were six regional test offices, each serving a specific geographic catchment area; one office closed in 1995. Each test office filled military troop orders from all branches of the service, both locally and across the country.

The testing procedure typically took two days. On day one, groups of young men (typically from the same local area) were transported by bus or train to their regional test office. The day began with a meeting to inform conscripts about both their rights and obligations as

[11] Carlsson et al. (2015) discuss the assignment of test dates and test offices in great detail.

well as the testing procedure. The conscripts then took part in (i) a set of written tests measuring verbal, spatial, logical and technical ability, (ii) a telegraph test,[12] and (iii) medical and physical tests, examining hearing, vision, strength, height, weight, blood pressure, physical condition, etc. They were examined by a medical doctor, and (typically on day two) met with a psychologist for an interview. Each test result was entered into the computer system; additional written information was placed in a folder carried by each draftee from station to station.

Lastly, they met with a randomly assigned test officiator (*mönstringsförättare*).[13] The first order of business was to ascertain whether the medical staff and psychologist had found the testee fit to serve. A conscript could be exempted from service due to mental and/or physical health problems. Such exemptions were based on a pre-determined set of health criterion and were not discretionary. A conscript's health scores were determined by the office's doctors *before* meeting the officiator; the officiators were not involved in this process.

All recruits who were not exempted from service due to health reasons (the vast majority) were then placed by the test officiator into a service category (with a well-defined job description, rank and military unit) and given a starting date for their service.[14] That is, all non-exempted conscripts left the test office with a specific assignment and start date in hand, expecting to serve.[15] Placement into service categories was based on an individual's full range of test scores, interviews, specific skills (e.g. driver's license, language skills) and, to some extent, the conscript's preferences for service type, year, and location. Each service category was associated with a pre-specified range of test scores that constrained the officiators when

[12] Testees were as asked to listen to a series of Morse code (dashes and dots) and then to repeat these back to the tester using a telegraph key.
[13] All test officiators had been (or still were) officers in the military. Most were men. Towards the end of this period, there were also a number of women, but the data only have an anonymous officiator identification number.
[14] Though there is some variation over time, there were approximately 1200 service categories during most of the 1990s and 2000s (SOU 2000:21 Bilaga 3).
[15] Few draftees requested weapon-free service – just 0.1% of all draftees in 1994 (Pliktverket 1994). A slightly larger share (0.2%) stated that they were conscientious objectors and refused to do any form of military or civil service (Pliktverket 1994). These cases, however, were dealt with by the central office *after* the test day. Conscientious objectors had to apply for an exemption in writing during the six month period after their test date.

assigning service categories; conscripts should not be grossly over- or under-qualified for the tasks that they were expected to perform. For example, a specific officer position might require a conscript to receive at least a 7 (on a 1-9 stanine score) on all major test categories.

Officiators used computers to help make the first match between a candidate's scores and a small set of suitable service categories. The computer presented the officiator with a set of potential service categories that was the intersection of the troop order that needed to be filled in that test office and the conscript's test scores. A typical interview lasted about 30 minutes, and included a discussion of 4 or 5 potential service categories. Lower scoring draftees tended to have somewhat shorter interviews, and discuss fewer service categories. The officiator decided the exact service category assignment after interviewing the conscript.[16]

Importantly, due to the downsizing of the armed forces during the 1990s, troop orders were changed by the military *ex post*.[17] That is, the military did not need and could not absorb all of the troops initially ordered from the draft authority. Thus, many of the above mentioned service categories were oversubscribed.

So, how did the recruitment office choose who would and would not serve? And how could the officiator affect the probability of service? All interviewed officiators stressed that

[16] All officiators interviewed stressed the importance of placing the right man into the right position. Officiators were only allowed to deviate from the minimum requirements by one point on one test; and then only if the recruit had an exceptionally high score on at least one other test deemed particularly relevant to the position. While a clear set of rules prevented the placement of low score recruits into high positions, officiators also tried to not place high scoring men into low positions. Though we were given several concrete examples of the latter, the recruit in these cases had a clear motivation for wanting to perform a specific task and the officiator agreed this would benefit the recruit and the military. Deviations from the normal matching process (in either direction), however, were rare at this time, since there was no shortage of men with the correct qualifications and scores to fill each position.

[17] These ex post changes in troop orders were oftentimes made on very short notice and were quite unpredictable. The Defense Proposition of 1987 (DP87) was approved in June 1987. This proposition outlined the intended development of the Swedish military through to the end of 1992. There was *no* mention of budget cuts in DP87. However, in December of 1989 a new Defense Proposition (FU88) was adopted, which outlined a different military organization for the years 1990 – 1992. Some units were intended to be disbanded, others meant to be re-organized in order to have a slimmer administration, and there were also plans on for creating new units (typically taking over critical functions of the disbanded units). But even this plan was only partially implemented before a new plan was submitted to the Swedish Parliament for a vote (FB92). However, the government needed the support of the New Democratic Party, which forced them to change their plans (again); saving some regional units, while closing other units instead. Because of the financial crisis in the fall of 1992, the government was then forced to make further unexpected cuts in the military and needed to negotiate with the Social Democrats about where (geographically) and when cuts in military spending would be made.

the most suitable persons *within* each service category were chosen to serve.[18] This within category ranking of the "most suitable" candidates was based on multiple inputs, including test scores, willingness to serve, a conscript's preferences, and *the test officiators personal, subject judgment* (written down in the conscript's case file as brief notes). Test scores alone were not sufficient to create a unique ranking of candidates since (i) conscripts within each service category had (by construction) quite similar test scores and (ii) each candidate had a range of test scores on multiple tests of mental and physical ability. Thus, subjective judgement had to be used to decide which recruits to release from service from within each service category.

Draft officiators met at least four times a year both within and between the various draft offices to make these decisions. During these meetings, an officiator would discuss the relative merits of the recruits that he had interviewed. Aside from test scores, officiators also relied upon notes that they may have added to a conscript's file concerning particular qualities of the recruit. The use and content of such notes most likely varied across officiators, as did their levels of engagement and participation in these discussions (particularly between offices). The end result of these meetings was that some officiators ended up with higher placement rates into actual service than others.[19] Random assignment to a relatively high versus low service rate officiator provides us with a source of exogenous variation in military service.[20]

[18] This is consistent with the training and instructions that they were given (SOU 2000:21 Bilaga 3 and SOU 2004:5). According to an interview with one of the officiators who worked in the recruitment office's training center, the training and instructions were quite clear and consistent throughout this period.

[19] Officiators had nothing to gain in terms of financial rewards or promotions from placing more of "their own" recruits in service. The ones we interviewed were all genuinely motivated by the central idea of recruiting the right man for the right job. But they were operating in an environment in which service was considered a good thing to do and they were taking some flak from those who were disappointed about not being allowed to serve (and their parents). During our interviews, officiators expressed a desire to promote the wish of many young men to serve, which would be easier to do for one of their own recruits as opposed to a recruit that they had not met.

[20] From 1990 to 1996, downsizing was achieved through cutbacks and downsizing of existing military units as opposed to completely disbanding regiments or closing military bases. Disbanding a regiment (for example) typically followed a process of first downsizing and then elimination several years later. For instance, in 1994, the air defense regiment of Gotland (LV2) was downsized to a corps, and then disbanded completely in 2000. One exception (of which there were few) to this pattern was the disbanding of the infantry regiment in Bohuslän (I 17) in 1992. However, even in this case, not all I 17 conscripts were released from service. Many were simply sent to other regiments. Starting in 1997, full-scale disbanding became more prevalent. Thus, some 1996 testees would have been released from service due to the shutting down of their assigned unit, while others would have been released on officiator discretion. Since base and/or unit closures equivalently affected all officiators working in

We argue that draftees were randomly assigned to officiators based on the actual test day routines and anecdotal evidence from interviews with officiators working in different offices during this period. The story is simple. Draftees were led through a series of test stations, which ran in parallel and took more or less time to complete. Each conscript carried a folder with his personal information from one station to another. The conscript arrived in a waiting room outside the officiators' offices (there were always multiple officiators) and placed his folder on the top of a pile in a box. The next available officiator met with the conscript whose folder was at the bottom of the pile. Since test date and office were determined by month and year of birth, and municipality of residence at age 17, the draftee-officiator match is as good as random once we condition on county by test year and test office by test month fixed effects.

The test officiators themselves, as well as many Swedish conscripts that we have talked to, insist upon this random match. In interviews, the officiators stressed that test officers did not specialize in filling certain types of jobs or pick who to interview. All recruits had to be interviewed, and these were done on a first-come first-serve basis – the first available test officiator was matched with the next draftee in line.[21] Furthermore, officiators did not have individual quotas nor their own list of positions to fill; all officiators worked together to fill the office-wide order. Section 3 provides empirical evidence of this random matching.

## 2. Data

### *2.1. Data Description*

Our analysis studies males born in Sweden who take the enlistment tests between 1990 and 1996. We have a 70% sample of these men from Statistics Sweden's Multigenerational Register (*flergenerationsregistret*), which allows us to connect these men to their parents. These data

the same office at the same time, such closures in no way affect the validity of our identification strategy. Rather, identification (outlined in Section 3) relies on officiator deviations from the office-wide mean service rate.

[21] When asked in interviews about how draftees were assigned to officiators, the only answer we ever received was that it was as good as random (in Swedish, *slumpmässigt*).

have been matched to data from the Swedish Military Archives (*Krigsarkivet*), the Swedish Military Recruitment Office (*Rekryteringsmyndigheten*), the Convictions Register (*lagföringsregitsret*), and various register data from Statistics Sweden using each individual's unique personal identification number.

Our analysis, which capitalizes on the identifier of the test officiator, focuses on the 1990 to 1996 test cohorts for two reasons. First, almost everyone tested before 1990 served. As such, there was little room for officiator discretion. Second, the officiator identifier is missing in the draft authority's data for the 1997 to 2001 test cohorts.[22]

We use the national tax register to identify treatment status. Every conscript who served for at least two months received a small taxable income from the government. This payment is marked in the register as a payment from the military for compulsory service. The tax data are annual. So, we can see that a payment was received during the year, but not when it was received. This enables us to identify the year that a conscript begins and ends his military service, but we do not know the exact dates of service.

The variables that we use from the draft board data include test date and test office, the conscript's height, weight, BMI, general ability test scores (stanine scores, 1-9), physical capacity (stanine scores, 1-9), health categories, psychological capacity (stanine scores, 1-9), and the service category an individual was assigned to (if not exempted from service due to health reasons). General ability, health categories, physical- and psychological capacity scores are summary measures based on a series of underlying tests.[23]

[22] We have been told that these data are missing due to a re-organization of the draft authority, the adoption of a new computer system, and a change in routines for archiving data. The officiator variable re-appears in 2002. But by this time, less than 30% of Sweden born males served in the military (see Figure 1). Though still illegal to refuse to serve, it had become more or less optional for young men.

[23] The test score for general ability is a summary measure of a set of written tests measuring, verbal, spatial, logical and technical ability. Physical capacity and health categories were determined through tests examining hearing, vision, strength, height, weight, blood pressure, physical condition, etc. Psychological capacity was determined by a psychologist after interviewing each conscript to evaluate his ability to cope with stress and contribute to group cohesion, willingness to assume responsibility, independence, emotional stability, outgoingness, persistence, and ability to take initiatives. The main aim of this interview was to aide in selecting potential officers. But it was also used to screen for mental health issues that would exempt a conscript from service.

The official crime register (*lagföringssregistret*) provides a full record of criminal convictions, including the type of crime and date of offense, from 1973 to 2012. As is typical with administrative data, we do not directly observe criminal behaviour; rather, we use convictions to proxy for criminality. We study overall crime, *Any Crime*, and six specific categories: *Weapons*, *Violent*, *Traffic*, *Theft*, *Other*, and *Drugs & Alcohol*. We define extensive margin variables that equal one if the individual has at least one conviction in the appropriate category. At the intensive margin, we look at the number of convictions as well as dichotomous variables indicating two or more and five or more convictions. We use the latter to investigate and alleviate concerns that the intensive margin results are driven by a few individuals with an extremely large number of convictions. Using the offense date, we create age-specific crime categories and classify crimes as pre-service (ages 15-17) and post-service (ages 23-30).

When considering potential mechanisms in Section 5, we investigate and describe in detail several non-crime outcome variables – education, income and unemployment. We also use a number of background and control variables from register data held by Statistic's Sweden, including *Birth Year*, and *County* or *Municipality* of residence at age 17. We record if a person was enrolled in a 2- or 3-year high school program, since this was used in some cases to help assign test dates. We also create measures of mother's and father's education and income to ascertain the socioeconomic background of our draftees.[24]

### *2.2. Sample Creation and Descriptive Statistics*

Our baseline data set consists of 231,583 non-immigrant males who tested from 1990 to 1996. We omit about 24,000 individuals missing officiator identifiers or who are assigned officiators with less than 100 cases that year, and slightly more than 33,000 individuals assigned to health groups that are not allowed to serve in a given year or are missing health group information (88

[24] Education is measured in seven levels. Income is measured as the log of average income using all available income data from 1968-2012. The income concept used here is pre-tax total factor income.

individuals). As the officiator is uninvolved in the assignment of individual health categories or the decision of which categories will not serve, these individuals are omitted since it is impossible for the officiator to influence whether or not they serve. We return later to this ineligible to serve for health reasons sample in a falsification exercise. After dropping less than 1,000 individuals who are not assigned to a service category (for unknown reasons) and less than 5,000 individuals who are 23 or older in the year they finish service (or for whom the year is unknown), we obtain our final sample of 168,805 non-immigrant males who tested between 1990 and 1996 and were born between 1968 and 1978.[25, 26] This final age-based restriction is primarily to ensure that we are identifying a post-service effect of crime; we thus define our outcome variables by age, emphasizing crime between ages 23 and 30.[27]

Table 1 provides summary statistics– overall and by service. During this period, 75% of the sample that was eligible to serve actually served. The middle panels characterize the potential conscript's offense specific criminal history and socioeconomic status prior to testing, as well as his performance on the test. The service sample is positively selected in all dimensions: same or less criminal history (for all crime categories), more educated families, more likely to attend 3-year high schools, and higher ability, physical, and psychological



[25] Not being assigned to a service category for unknown reasons is not an officiator specific phenomenon. Most officiators have less than 1% of their testees unassigned in this way, and just six officiators have assignment for unknown reasons at a rate between 1% and 2% (representing about 300 testees). These officiators were primarily at three offices. While we do not know the reason for this office specific phenomenon, there is no significant relationship between officiator service rates (i.e. the instrument) and being unassigned for unknown reasons once controlling for office fixed effects.

[26] Specifically, for these cohorts, we know the last year in which they are observed receiving income from the military according to tax records. We drop individuals who are 23 or older in this last year. 97% of the sample that remains is born between 1972 and 1978, with 3% born between 1968 and 1971.

[27] Just 3% of the sample finishes military service in the year they are 23 or older. In contrast to this, 4% finish when they are 19 or younger, 50% when age 20, 33% when age 21, and 10% when age 22. Extending the age threshold to (for example) 24 would only add 819 more individuals. But, then we would have to measure crime after age 24 – i.e. further along the age crime profile (which is declining at that point) and further and further from the end of service for the bulk of individuals – who finished service many years earlier. Though we believe the analysis is cleaner (i.e. we can estimate a clean post-service effect on crime) if we restrict to those who finish before age 23, this may raise concerns regarding sample selection. We have checked that the baseline results are robust to including all individuals, regardless of age of service finish; if anything the effects become larger, but this is hard to interpret since this could in fact include crimes committed before service.

capacity test scores.[28] Having a criminal history does not disqualify one from service; 13% of the service sample has at least one conviction prior to age 18.

Finally, the bottom of Table 1 considers the main crime outcomes. Overall, 10% of the sample is convicted of at least one crime age 23 and 30. The average number of convictions (including zeroes) is 0.31, 4% of the sample has two or more convictions, and 1% has five or more convictions. At both the extensive and intensive margins, the largest crime category is traffic offenses. The other crime categories have much lower conviction rates: 1% for weapons, 2% for violent, 1% for theft, 2% for drugs, and 2% for other offenses. For almost every crime outcome, the post-service crime rate is lower for the service sample than the non-service sample. The above described positive selection into service, however, makes it clear that this cannot be interpreted as anything more than a correlation.

## 3. Empirical Methodology

### *3.1. Officiator Assignment as Instrument for Military Service*

The primary purpose of this paper is to identify the causal effect of mandatory military conscription on post-service crime. To that end, consider a regression that relates an outcome of interest, $y_i$, for individual $i$ to whether he was conscripted into the military, $Conscript_i$.

$$y_i = \propto + \beta Conscript_i + X_i + \varepsilon_i \quad (1)$$

Even with a large set of observable controls, $X$, conscription is likely to be correlated with the error term due to the selection process. Because the tests themselves, as well as unobservable determinants of the results (like background, ability, performance under pressure, etc.), affect the likelihood of both service and committing a crime, Ordinary Least Squares (OLS) estimation of equation (1) will yield biased estimates of the effect of conscription on crime.

[28] This positive selection into service is observed regardless of who assigns service – i.e. high or low service rate officiators. Available from the authors upon request.

To remove this bias, we capitalize on the random assignment of potential conscripts to officiators who assign more or less individuals to service in an instrumental variable design similar in spirit to work using randomly assigned judges or investigators as instruments.[29] We therefore focus on the 1990 to 1996 test cohorts, for whom we can identify the officiators who reviewed the draft board test results and made service decisions. Specifically, we instrument for *Conscript* with the leave out annual mean service rate of the officiator to which the individual is assigned (*Z*); that is, for individual *i* assigned to an officiator *j* in year *t*, the instrument is the share of all other testees assigned to officiator *j* in year *t* who serve, excluding individual *i*.[30] In robustness checks, we use a dichotomous version of the continuous leave out annual mean service rate – a dummy variable indicating whether the individual is assigned to a "*high service rate officiator*" (one whose annual service rate is greater than the national share who serve in that test year). Importantly, we use the leave out *annual* mean rather than simply the leave out mean for the officiator because of the downward trend in the number of service positions and since not all officiators are observed in the same years.

Specifically, our data contain 67 officiators in the six primary test offices in Sweden. In any given year, we observe between 25 and 29 officiators (1993 is an outlier with 37 officiators). On average, there are about 10 officiators seen in each test office per year, since some officiators rotate across test offices throughout the year. In fact, just 42% of officiators are stationed in a single office each year; 19% in two, 17% in three, and the remaining in four or more. Each county is assigned a primary test office (at which 99% of the annual draft board tests are conducted) and testing occurs regularly (nearly every month) throughout the year at each office; the test month is generally very close to the birth month of the individual. Because

[29] Our design is essentially the same as that of previous work using randomly assigned judges as an exogenous source of variation in prison sentences (Kling, 2006; Aizer and Doyle, 2015; Mueller-Smith, 2015; Bhuller et al., 2016; Bhuller at al. 2018; Dobbie et al. 2018) and disability insurance take-up (Dahl et al. 2014) or randomly assigned welfare investigators as exogenous variation in foster care placement. (Doyle, 2007; Doyle 2008).
[30] We create the leave out annual mean service rate based on our full sample of all non-immigrant males who tested each year, and not the final analysis sample.

of this geographic and temporal sorting into test offices and dates, officiator characteristics could vary *across* counties and/or test offices as well as test months and/or years in a way that is correlated with testee characteristics. However, once we condition on when and where the individual was assigned to take the test, the assigned officiator is as good as random. Our baseline instrumental variable specification therefore includes county (*c*), test year (*y*) and county by test year (*c*y*) fixed effects as well as test month (*m*, a continuous measure that begins in January 1990), test office (*o*), and test month by office (*m*o*) fixed effects.[31] Equations (2) and (3) below present our baseline specification. Standard errors are clustered on the officiator, and a sensitivity analysis to alternative clustering choices is presented in Appendix Table A7.

$$Conscript_{ijt} = \gamma Z_{j,t,-i} + X_i\delta + \alpha_c + \alpha_y + \alpha_{c*y} + \alpha_m + \alpha_o + \alpha_{m*o} + \varepsilon_{ijt} \quad (2)$$

$$Crime_{ijt}^{23-30} = \beta Conscript_{ijt} + X_i\mu + \theta_c + \theta_y + \theta_{c*y} + \theta_m + \theta_o + \theta_{m*o} + \rho_{ijt} \quad (3)$$

Intuitively, our instrument utilizes the exogenous variation in the chance of service given the officiator one is assigned from the pool of potential officiators in a specific test office and month. Recall that each test office always has at least two officiators working on a given test date. To better understand the nature and variation of our instrument, Figure 2 and Figure 3 take a closer look at the leave out mean service rate. Figure 2 presents histograms of the leave out annual mean service rate in both the raw data and residualized after conditioning on county by test year and test office by test month fixed effects. The average leave out annual mean service rate is 0.64, with a standard deviation of 0.13; the standard deviation of the residualized leave out mean is 0.04. Though the distribution is compressed in the residualized figure, substantial variation remains, even within a given test office and month.

For each year that an officiator is observed, Figure 3 plots the deviation of the officiator's annual service rate from the national service rate in that year. Each vertical line

[31] While parish of residence is officially what is used in assigning test offices, this unit is too small to conduct this analysis. However, we provide evidence that this baseline is sufficient to achieve conditional random assignment.

represents a unique officiator, and each point on these lines represents an officiator-year observation. Deviations that are positive correspond to officiators being classified as "high service" in our dichotomous instrument for a given year. This figure demonstrates that officiators are fairly persistent in their behaviour, relative to all other officiators, over time. For 56 of 67 officiators, their classification as high (above national service rate) or low service officiators is constant across all years. And for those that change from high (low) to low (high) service officiators, one can see that in most cases the mean service rate is actually quite close together, i.e. all points are clustered very close together on the vertical line around zero. Thus, the persistent nature of the officiator's service rate over time indicates that the variation across officiators is driven by something inherent about the officiator – his type – rather than a shock to an officiator/office in a given year.[32]

### *3.2. Assessing the Instrument*

#### *3.2.1. Instrument Relevance*

Table 1 provides preliminary evidence of the relevance of our instrument. Despite coming from comparable cohorts, those who serve face an average leave out annual mean service rate of 0.66 compared to 0.59 for those who do not serve and are 21 percentage points more likely to have been assigned a high service rate officiator (i.e. the dichotomous instrument).

Table 2 further demonstrates the relevance of the instrument: does an increase in the annual service rate of the assigned officiator increase the likelihood of serving in the military? Panels A and B present the results for the continuous and dichotomous leave out annual mean instruments, respectively. Our baseline specification, which includes county by test year and

[32] As described in footnote 20 in Section 1, the dissolution of full regiments was very rare during our sample period. But even those that did occur do not affect our identification strategy, since all officiators worked together to fulfill an office-wide order to supply troops to specific regiments. Closing down a regiment and releasing all of those conscripts from service would simply lower the mean office-wide placement rate, whereas we are identifying off of officiator deviations from this mean, since we control for test office and test office by month fixed effects.

test office by test month fixed effects, is presented in column (1). Being assigned an officiator whose other testees in that test year are 10% more likely to serve increases the chance of service by 6.6 percentage points, or 8.8% relative to the mean service rate of 75%. Looking at the dichotomous instrument helps put the magnitude of these relationships in context: assignment to a high service rate officiator increases the likelihood of service by 7.3 percentage points. The F-statistics associated with our baseline specifications are 66 and 35 for the continuous and dichotomous instruments, respectively, and well above the weak instrument threshold.

#### *3.2.2. Instrument Validity: Conditional Independence*

The first criteria for our instrument to be valid is that, conditional on county by test year and test month by office fixed effects, the leave out annual service rate of the officiator is uncorrelated with testee characteristics that can affect post-service crime. The discussion in Section 1 of the random assignment of conscripts to officiators on a first-come first-serve basis provides anecdotal support of conditional independence. We now provide empirical support.

Table 3 tests for balancing in observable characteristics for those assigned to high and low service rate officiators (i.e. above and below the median leave out annual mean service rates). The corresponding average leave out annual mean service rate for these samples is 0.74 and 0.55. Specifically, we present the raw sample means of the test-day and pre-test day controls in columns (1) and (2), as well as the percent difference between these two samples and associated p-values in columns (3) and (4). P-values associated with the difference conditional on county by test year and test office by month fixed effects are shown in columns (5) and (6). In terms of raw observables, individuals above and below the median significantly differ from each other for nine of twenty variables. Just one of these variables – father income – significantly differs at the 5% level when conditioning on county by test year fixed effects, though the raw difference is just 0.3%. None are significantly different with our baseline specification of county by test year and test month by office fixed effects.[33] In addition, an F-test of the joint significance of all of these controls gives p-values of 0.1588 and 0.3245, respectively, in regressions of the dichotomous and continuous instruments on all controls (simultaneously) and baseline fixed effects (see columns (3) and (6) of Appendix Table A2).[34]

Additional evidence of conditional independence is seen in columns (2) and (3) of Table 2, which add pre-test day characteristics (including criminal history and parental background) and test day characteristics (including ability, physical capacity and psychological scores) respectively to the baseline first stage regressions. Controlling for these variables has little impact on the first stage relationship, even though many of these variables are clearly important inputs into whether an individual is conscripted. The robustness of the first stage and associated F-statistic is not surprising given that officiators are randomly assigned, as these test and pre-test day characteristics should be uncorrelated with the leave out mean service rate instrument.

### *3.2.3. Instrument Validity: Exclusion Restriction*

To interpret the instrumental variable estimates as the causal effect of military service on post-service crime, an exclusion restriction is needed – namely that the officiator's propensity to assign testees to service *only* affects post-service crime through whether or not the testee serves in the military and not any other channel. There are two potential alternative channels. First, does the actual meeting with the officiator have a direct effect on the testee, regardless of



[33] Moreover, given that the variation in the main instrument is not just above and below the median but through the whole distribution of the leave out mean, Appendix Table A1 redoes this analysis using tertiles of the leave out mean. This highlights that it is the top tertile driving the significant raw differences observed in Table 3, but that these differences also disappear when conditioning on our baseline set of fixed effects.
[34] Even when coefficients are significant in these regressions, they are not economically meaningful nor consistent in one direction or another. For instance, in column (3) of Appendix Table A2, a one-point higher score on the cognitive and non-cognitive ability tests are associated, respectively, with an on average 0.09 percentage point *lower* and 0.13 percentage point *higher* chance of a high service officiator.

whether they serve? Second, do high service officiators assign different types of service? That is, is it the type of service assigned to the testee that affects post-service outcomes?

We provide both anecdotal and empirical evidence that the first channel – the meeting with the officiator – does not have a direct impact. Anecdotally, these meetings are quite short (less than 30 minutes); indeed, most Swedes with whom we have discussed their experience at the draft hardly recollect meeting the officiator at all. In this context, where the officiator is largely seen as a human resource officer as opposed to a criminal court judge who might want to leave a lasting impression on the defendant, it is not hard to believe that the meeting with the officiator does not have a direct impact. We present empirical evidence of this in a falsification exercise using the sample of individuals ineligible for service because of health reasons; these individuals were assigned health category Y – no individuals in this category served. For this sample, we examine the reduced form specification of whether the service rate of the assigned officiator affects crime outcomes. It could only do so if (i) the short meeting with the officiator actually had a direct effect on the testee or (ii) random assignment fails – i.e. testees are assigned officiators according to characteristics related to future crime. Thus, this falsification exercise can also be seen as an alternative test of random assignment. However, as seen in Panel B of Appendix Table A3, the service rate of the assigned officiator is not significantly associated with future crime for these individuals who are ineligible for service.

Finally, Table 4 regresses the type of service on the continuous and dichotomous instruments to investigate whether the service rate of the assigned officiator affects more than whether an individual simply serves or not. The draft board data are not ideal for measuring military experience, as their emphasis is on testing. We are limited to looking at the following measures of military "experience": army/air force/navy, rank (private, corporal, or sergeant/2$^{nd}$ lieutenant), and whether the testee is assigned a combatant position. Note that rank can also be a proxy for length of service, as higher ranks serve more months. Compared to F-statistics of

66 and 35 (for the continuous and dichotomous instruments) on whether an individual serves at all, the F-statistics characterizing the type of service are between zero and five. In fact, for the dichotomous instrument, having a high service officiator is not significantly related at the 5% level to any measure of the type of service. We do, however, see a statistically significant relationship between our continuous instrument and assignment to a positon in the navy and air force.[35] Given the weakness of these "alternative" first stage relationship, we do not believe that they are driving our instrumental variable results. We show this empirically in Appendix Table A4, which demonstrates the robustness of our main first stage, reduced form and IV estimates to an expanded model that controls for the officiator's army, navy, and air force propensity.[36]

### *3.2.4. Instrument Validity: Monotonicity*

To ensure that that the IV identifies the LATE effect of service in the presence of heterogeneous effects, the assumption of monotonicity is required. In the current context, this implies that testees who are assigned to service by a low-service rate officiator would also have served if they met a high-service officiator, and vice versa for individuals not assigned to service.

As highlighted by Bhuller et al (2016), there are two testable implications of monotonicity. The first is that assignment to a high service rate officiator (or higher leave out mean) should have a non-negative effect on the chance of service for any sub-sample in the data. Using the same instrument, i.e. defined based on all cases, Appendix Table A5 presents the first-stage results using both the continuous and dichotomous instruments for subsamples

[35] We believe this association to be purely mechanical. The chance of actually serving in the military was higher for those assigned to naval service categories (85% served) than those assigned to army or air force categories (71% and 79% served, respectively). This implies that the caseload handled by an officiator (e.g. randomly filling more navy than army positions) may result in a randomly higher service rate among testees to whom a particular officiator has been assigned. This, in turn, generates a mechanical relationship between the leave-out annual mean and assignment to navy that is unrelated to the officiator's own propensity to place conscripts in service.

[36] This robustness tests is similar in spirit to that of Bhuller et al (2016), who are concerned about multiple judge treatments in sentencing.

(i) with and without a criminal history before age 18, (ii) with low and high father schooling, and (iii) with low (<=5) and high (>5) general (cognitive) ability, physical ability, and psychological (non-cognitive) ability. We also break the sample into quartiles according to their predicted propensity to serve, which is obtained from regressions of service on all test day and pre-test day controls (including those listed in (i) – (iii) above).[37] Consistent with monotonicity, the first stage coefficients are significantly positive for all subsamples, with F-statistics ranging from 32 to 136 and 17 to 47 for the continuous and dichotomous instruments, respectively.

The second testable implication is that officiators should be more likely to assign a particular type of individual to service (e.g. those with a pre-service criminal history) if they are more likely to assign to service all other types of individuals (e.g. those without a criminal history). Following the approach of Bhuller et al (2016), we test this implication by creating a 'reverse sample instrument' for each sub-sample. That is, we calculate the officiator's annual service rate for all individuals outside of the current subsample; e.g., when looking at the criminal history subsample, we create an instrument based on all individuals without a criminal history (and vice versa). Appendix Table A6 shows the first stage subsample regressions for the baseline continuous instrument and corresponding reverse sample instrument.[38] The reverse sample first stages are still positive and significant, with appropriately high F-statistics, for all subsamples. As required by monotonicity, these results suggest that officiators with a high propensity to assign one type of testee to serve are also more likely assign other testees to serve.

## 4. Instrumental Variable Estimates of the Effect of Service on Crime

### *4.1. Baseline Results and Robustness Checks*

[37] Baseline fixed effects are included in these regressions but not used to determine the predicted propensity to serve index.

[38] We do not conduct the reverse sample exercise for the predicted propensity to serve sub-samples since the predicted propensity regression is for the analysis sample, but the instrument is created using the entire sample.

Table 5 presents our baseline estimates of the impact of military service on post-service crime from ages 23 to 30; Panel A considers the extensive margin (at least one conviction) while Panels B – D consider the intensive margin (number of convictions, two or more convictions, five or more convictions). All specifications include the baseline set of fixed effects, while pre-test and test-day controls are added in the even numbered columns. OLS estimates in columns (1) and (2) find a negative correlation between service and overall crime, regardless of how crime is measured. When including just the baseline fixed effects in column (1), service is associated with a 1.7 percentage point (17%) reduction in the likelihood of at least one conviction, 0.13 (42%) fewer convictions on average, and 1.3 and 0.6 percentage points (31% and 46%) lower chance of being convicted of two or more and five or more crimes, respectively. Though controlling for observable pre-test and test day characteristics substantially reduces the magnitudes of these relationships, they remain negative and highly significant.

Columns (3) and (4) of Table 5 present the reduced form regressions of the post-service crime measure on the leave out annual mean service rate of the assigned officiator. Assignment to an officiator with a 10% higher annual service rate significantly increases the chance of at least one conviction between ages 23 and 30 by 4% (.1*.04/.1) and the number of convictions by 14% (.1*.44/.31). Controlling for the full set of observable controls in column (4) has little impact on these estimates. Though effects of similar magnitudes are seen for convictions of two or more and five or more crimes, these estimates are not significant at the 5% level.

Columns (5) and (6) of Table 5 present the 2SLS estimates of the relationship between military service and post-service crime, using the continuous leave out annual mean as an instrument for military service. In other words, we scale the reduced form estimates in columns (3) and (4) by the first stage estimates in Table 2 (approximately 0.66 without controls and 0.64 with controls) to identify the causal effect of service on crime. These results indicate that military service in fact has a large positive causal impact on post-service crime from age 23 to

30. Serving in the military increases the chance of post-service conviction for our complier population by 6.5 percentage points and, on average, leads to an increase of 0.66 convictions. Relative to the mean outcome of untreated compliers, these effects are quite large – an increase of about 33% at the extensive margin and more than 100% at the intensive margin.[39] Using the dichotomous intensive margin variables, we see comparatively large, though less significant, point estimates. Columns (7) and (8) of Table 5 demonstrate the robustness of these results to the alternative dichotomous instrument. Though the associated standard errors and confidence intervals are quite large, the IV estimates clearly indicate a significant positive effect of service on crime. As this is the local average treatment effect (LATE) or the effect of service on young men to whom officiator assignment matters, it does not seem infeasible that service differentially affects these young men.[40]

Our baseline specification clusters on officiator. Appendix Table A7 demonstrates the robustness of our results to clustering the standard errors in other dimensions: county (column 2), both officiator and test year (column 3), officiator by test year (column 4), and officiator by test office by test month (column 5).

These results clearly demonstrate a positive impact of military service on *overall* crime from ages 23 to 30. But, one may ask whether such an effect is (i) seen across crime categories or driven by a particular category and (ii) persistent across all ages or driven perhaps by the years immediately post-service. Table 6 addresses the former by re-estimating the baseline specification for crime specific dependent variables. The dependent variable in Panel A is

[39] In Appendix B, we calculate mean outcomes for untreated compliers and characterize the complier population by their observable characteristics.

[40] In her Table 7, Lyk-Jensen (forthcoming) reports intensive margin effects between 0% and 74% for Danish men born in 1976 and 1977 with at least one pre-service conviction. She measures crime at ages 25-30, while we measure crime at ages 23-30. Wang and Flores-Lagunes (2017) report various estimates of the lower bound effect of voluntary military service in the US on incarceration rates of low SES men born in 1951 and 1952. Their estimates range between 14% and 160%. Thus, crime inducing effects have been found in Denmark, the US, and (now) Sweden. While our effects are consistently larger than those reported in Lyk-Jensen (forthcoming), they are of the same magnitude as those in Wang and Flores-Lagunes (2017). An important caveat is that the confidence intervals around our point estimates are quite large. Thus, effect sizes should be interpreted with some caution.

whether one has at least one conviction in the crime category listed at the top of the column: weapons, violent, traffic, theft, drugs and alcohol, and other offenses. The dependent variables in Panels B and C measure the intensive margin: the number of convictions and whether there are two or more convictions. Row (a) of each panel presents the baseline specification; row (b) adds the full set of controls, including a set of crime-specific history variables.

At the extensive margin, service significantly (5% level) increases the chance of a weapons and traffic conviction by 1.4 and 4.3 percentage points, respectively, while it increases (at 10% significance) the chance of a violent conviction by 1.9 percentage points. Relative to the dependent variable means, these point estimates (as well as those for the other offense categories) are quite large.[41] At the intensive margin, service significantly increases the number of convictions for all crime categories except the other category: weapons by 0.041, violent by 0.055, traffic by 0.25, theft by 0.12, and drugs and alcohol by 0.15. Service also significantly increases the chance of two or more convictions for weapons, traffic and other offenses. Controlling for the full set of controls has minimal impact on the magnitude or precision of the effects. Given that some variables, especially criminal history, are particularly strong predictors of future crime, the insensitivity of the estimates to their inclusion further supports the assumption of random officiator assignment.

Figure 4 considers the temporal dynamics of the effect of service on crime at the extensive (Panel A) and intensive (Panel B) margins, respectively. Specifically, it presents the point estimates and 95% confidence intervals of re-estimating the baseline specification using crime measured in two-year age intervals: ages 23-24, 25-26,…, 33-34 as the dependent variables. There are two clear takeaways. First, the effect of service on crime is immediate. Though there is some loss of precision when disaggregating crime into two-year intervals, large effects are seen at both margins for ages 23 and 24; these are only significant at the intensive

[41] The confidence intervals around our point estimates are also quite large. Thus, effect sizes should be interpreted with some caution.

margin. Second, the effect of service on crime is persistent; the effects are seen until age 34 at the extensive margin and age 30 at the intensive margin. Though persistent, the effects do appear to peak between ages 27 and 30, depending on the margin.

### *4.2. Heterogeneity Analyses*

This section examines whether there are heterogeneous effects of service in terms of the individual testee's pre-test background. Table 7 begins by considering two characteristics that are particularly well-known to be strong predictors of criminality: (i) having a criminal history prior to age 18 (Panel A) and (ii) coming from a low socioeconomic status family, for which we use father's education as a proxy (Panel B).[42] Panel C considers each quartile of the previously described propensity to serve index (that is based on all pre-test and test day characteristics, including criminal history and father schooling). There is, in fact, a monotonic relationship between the propensity to serve and crime from age 23 to 30: the lowest quartile has the highest conviction rate (14.9%) while the highest quartile has the lowest conviction rate (7.8%). Results are presented for crime overall at the extensive and intensive margins (columns (1) – (6)) and extensive margin crime categories (columns (7)-(12)).

A first takeaway from these results is that the effect of service on crime is not limited to those from disadvantaged backgrounds. For instance, some large and significant (or marginally significant) effects of service on post-service crime are seen for those: (i) without a criminal history (any conviction), (ii) with high educated fathers (number of convictions), and (iii) from quartile four (any convictions and two or more convictions). A second takeaway, however, is that the effects tend to be larger for those from disadvantaged backgrounds and more consistent across the extensive and intensive margins in terms of magnitude and significance. One should, however, be cautious in making this comparison given that the large standard errors imply that

[42] We use father's education as a measure of family SES in part because it such a strong predictor of a son's criminal behavior. See, e.g., Hjalmarsson and Lindquist (2012) and Meghir et al. (2012).

most of the estimates are not significantly different across subsamples. Large and significant effects are seen for those: (i) with a criminal history (number of convictions), (ii) low educated fathers (number of convictions and two or more convictions), and (iii) lowest quartile (any conviction, and number of convictions). Moreover, the crime category results paint a more nuanced picture: much of the effect for the advantaged backgrounds is driven by relatively minor offenses (traffic and other) while that for the disadvantaged backgrounds is driven by the more serious offenses of weapons, violent, theft, and drugs and alcohol.[43]

### *4.3. Incapacitation*

This section uses the same IV approach to test whether service has an incapacitation effect on crime, i.e. whether individuals commit less crime *during* service. However, our ability to do this is constrained by not knowing the exact dates of service for our sample. Rather, we know the age at testing and, for those who were conscripted, the year that they began and ended service. These dates are quite informative about the timing of conscription relative to the test date. All but about 500 individuals in our baseline sample took the test when they were 18, 19 or 20 (the vast majority when 18). For those that served of this sample, 33% started service in the year after testing (that is, if they tested at 18, they started service when 19) and 47% started in the second year after testing. 57% ended service in the second year after testing. More generally, the median number of years after testing that individuals both started and ended service is two. Thus, even though we cannot identify the exact dates of service, we can say with a high degree of certainty that most individuals serve (or would have served if conscripted) in the second year after testing; given a minimum service length of seven months, even those who began in the first year after testing would still spend some of the next year conscripted.

[43] Heterogeneity analyses for additional subsample splits by cognitive-, physical-, and non-cognitive ability scores are shown in Appendix Table A8.

Thus, to test for incapacitation, we estimate our baseline specification when defining the dependent variables to be crime in the first year after the test year and the second year after the test year (the year individuals turn 20, 21, and 22 for age 18, 19, and 20 year old testers). It is during this second year that we expect the strongest incapacitation effect, if any. Of course, this design is not perfect, as we cannot rule out that some of crimes occurred before or after service, given that service is not for the whole year.

The results are presented in Table 8. No significant evidence of incapacitation is seen when looking at all 18-20 year old testers in Panel A, though negative coefficients are found in the second year after the test (i.e. the most likely year of incapacitation) at both the extensive and intensive margins in columns (2) and (5), respectively. For those with a criminal history, however, Panel B shows large and significant (10%) incapacitation effects in this year. For these individuals, the chance of committing a crime during the second post-test year is 13 percentage points lower while the average number of crimes is -0.39 lower. Such an incapacitation effect is not seen, however, for those with no criminal history. While these incapacitation effects are not measured very precisely, they do contrast sharply with the large positive effects found during the post-service period. Moreover, they are consistent with incapacitation effects found using an alternative research design for a later sample of test cohorts for which exact dates of service are available.[44]

An alternative explanation of the suggested incapacitation effect is a mechanical effect of non-reporting. If crimes committed during military service are dealt with internally through alternative disciplinary channels, then they would not be recorded as a criminal conviction in



[44] We have also tested for incapacitation in later cohorts using an alternative design. For the 1997 to 2001 test cohorts, we know the exact date of service (though not the officiator). For this sample, we match each individual who serves in the military to one specific control individual who does not serve. We use the dates of service for the treated individual to construct the counterfactual time of incapacitation for his matched control. We then apply a difference-in-difference design to this matched sample to estimate the incapacitation effects. We find large and significant incapacitation effects for drug and alcohol offenses at both the extensive and intensive margins. For traffic crimes, we find a large and significant incapacitation effect at the intensive margin, especially for those with prior convictions. The analysis is available in an earlier working paper (Hjalmarsson and Lindquist, 2016).

our data. While this phenomenon may be prevalent in some countries, it is clearly not what is going on in our Swedish data. Military courts were abolished in Sweden in 1949. Criminal infractions, regardless of whether they are committed on or off base, are handled by the local police and criminal justice system. The Swedish military police do work to prevent crime (typically guarding munitions, weapons, facilities, and personnel) and may also aid the local civilian prosecutor when investigating crimes that take place on base or that are aimed at the military. But all investigations are led by a civilian prosecutor and tried in the civilian court system and are, therefore, included in our data. Note also that the majority of draftees do not spend all of their time on base; some live at home (off base) and most are free to leave the base during weekends and evenings. Furthermore, during interviews with former military personnel, we were given numerous examples of crimes (both large and small) that were reported directly to the local police and that led to convictions in the civilian court.

## 5. Potential Mechanisms: Labour Market and Peer Effects

The aim of this section is to shed some light on the mechanisms underlying our baseline results: military service significantly increases post-service crime, especially for those from disadvantaged backgrounds. We discuss four potential channels: (i) changes in criminal activity concurrent with service put individuals on a new post-service crime trajectory, (ii) the positive effect of service on crime is moderated through a negative impact of service on the labour market, (iii) service generates negative peer effects, and (iv) desensitization to weapons and violence.

### *5.1. Changes in Post-Service Crime Trajectories Due to Incapacitation*

Our analysis provided evidence that the contemporaneous effect of service is, if anything, incapacitative: potential offenders, especially those with a high risk of crime, commit fewer crimes while in service. Given the incapacitative concurrent effect of service and the fact that one of the strongest predictors of future crime is past crime, it is hard to imagine an immediate and persistent increase in post-service crime resulting from a change in the age-crime trajectory. One possibility is a temporal displacement of the crimes that would have been committed while doing service to immediately after service. But, this would seem an unlikely explanation since the effect on crime is persistent and not only at ages 23 and 24 (see Figure 4).

### *5.2. Labour Market: The Impact of Service on Non-Crime Outcomes*

The second proposed channel is one in which service impacts crime through its effect on legitimate labour market activity. This is a particularly relevant concern in the current context given that our cohorts were leaving service and entering an environment with rather high unemployment among young adults. One conjecture is that low skilled workers who do not partake in military service are able to establish themselves on the labour market more quickly.

To explore this channel, we create a set of non-crime outcome variables using data from income and education registers maintained by Statistics Sweden. We begin by creating a dichotomous variable for *Education*, which equals one if an individual has obtained at least some college by 2012 and zero otherwise. *Income* is the log of average (pre-tax total factor) income between ages 30 and 34; one can think of this as a measure of permanent income, which aims to assess whether service has a long-lasting effect on income.[45] For individuals who have no taxable income reported during this period, we estimate specifications in which (i) they are excluded (*Income_30_34A*) and (ii) they are assigned a log income of zero (*Income_30_34B*).



[45] Measuring income for Swedish men at these ages has been shown to be a reasonably good proxy of their permanent income (Böhlmark and Lindquist 2006). While it would be preferable to average income over a longer period and at later ages, e.g. 30 – 40, our cohorts are simply too young. Using income measured before age 30 would severely bias our measure of permanent income, since high skilled workers have not yet reached their earnings potential. It may even appear as if their earnings potential is lower than that of low skilled workers.

*Unemployment Benefits* equals the number of years during which an individual has received at least one payment from the unemployment insurance system between ages 23 and 34.

Panel A of Table 9 presents the baseline instrumental variable results for the whole sample for the labour market outcomes. When considering the sample as a whole, military service does not significantly affect the chance of having more than 12 years of schooling nor the receipt of unemployment benefits. Service appears to increase income by about 14%, when excluding those with zero reported income (column (7)).

Panels B-D present the labour market outcomes for criminal history, father schooling, and propensity to serve index subsamples. With regards to education, we find a large and significant negative effect of service on the likelihood of higher education for the criminal history subsample but no effect for those with no criminal history. The only other subsample for which the likelihood of higher education is significantly negatively affected is those in the fourth quartile in terms of the propensity to serve index. With regards to unemployment and income, we see that service decreases (increases) income for those with (without) a criminal history; the magnitude of these effects depends on how the individuals with no reported income are treated. Individuals with a criminal history who serve also receive unemployment benefits during fewer years (on average) than those who do not serve. Though the unemployment effect is not significant, taken together with the income results, it suggests that military service decreases the chance that individuals with a criminal history are in the legitimate labour market. The income of those without a criminal history, in contrast, are positively affected.

Similarly, we find that service increases income for those with high education (more than 9 years) fathers while it decreases or has no effect on income for those with low education fathers. A similar pattern of heterogeneous effects is again seen for the unemployment outcomes: military service significantly increases (decreases) the number of years in which the low (high) education father sample received at least one unemployment benefit payment.

Findings beneficial labour market effects of service for those from advantaged backgrounds are consistent with Grönqvist and Lindqvist (2016), who show that officer training can raise the probability of becoming a manager later in life and improve wages.[46] When looking at the predicted propensity to serve quartiles, this diverging impact of service on income is seen again, especially for those at the bottom and top of the distribution.

For the above subsamples, regardless of how income is treated (with or without zeros for those missing income), a similar story of divergent effects of service on advantaged and disadvantaged subsamples tends to appear. The income measure used, however, does effect the magnitude of the effect, especially for those with a disadvantaged background.

To further understand the impact of service on the legitimate labour market, Table 10 re-estimates the effect of service on log income (columns (1) – (4)) and the chance of being unemployed (columns (5) – (8)) at each age separately from 23 to 34; zero income is used for those missing income in a given year. That is, we want to get at the dynamics of the relationship, rather than just the effect of service on permanent income. For those from advantaged backgrounds (no criminal history and high education fathers), we again see generally positive effects of service on income; these effects are especially strong and significant at ages 33 and 34, which are known to be good proxies of permanent income for Swedish men in these cohorts (Böhlmark and Lindquist 2006).

Though many of the point estimates are comparably large in the earlier ages, they are less precisely estimated; it is also during these earlier ages that individuals (especially from the advantaged sample) may be enrolled in school, depressing income. For those with a criminal history, a large and persistent negative effect of service on income is seen almost immediately. With regards to the dynamics of the unemployment relationship, we see that service decreases the chance of unemployment, especially between ages 24 and 26, for the high education sample,

[46] They study cohorts tested between 1970 and 1988. At this time, all men who were fit were required to serve.

which could again reflect school enrolment. In contrast, for conscripts with low-education fathers, there is a positive (albeit marginally significant or insignificant) effect of service on unemployment. For those with a criminal history, there is (if anything) a reduction in the chance of unemployment in the years immediately after service, but these effects are not precisely estimated. Again, one should keep in mind that not receiving unemployment benefits could either signal employment or being out of the labour market; for this high risk sample, the latter would seem more likely.

Taken together, these results suggest that peacetime conscription increases participation in the illegitimate labour market and does not significantly improve legitimate labour market outcomes for the most disadvantaged individuals. Importantly, these findings speak against the belief that providing discipline and structure to individuals already at risk for a life of crime by placing them in military service will put them on a better path and that the human capital skills gained during conscription improve the labour market outcomes for those coming from a disadvantaged starting point.

The divergent impact of military service on the labour market outcomes of individuals from disadvantaged and advantaged backgrounds is generally consistent with the pattern of results seen for the crime outcomes. From that perspective, it is certainly feasible that the effect of military service on crime is (partially) mediated through the effect of service on labour market participation. However, given the general well-known simultaneity issues in studying the impact of the labour market on crime (and criminal participation on the labour market), we cannot rule out that it is the impact of the military on crime, especially for this high risk criminal history subsample, that mediates the effect of service on the labour market. Despite this possibility, we argue that the effect of service on labour market outcomes is one feasible underlying mechanism mediating (at least part of) the relationship between military service and crime.

### *5.3. Peer effects*

Alternatively, or in addition, the harmful effects of service on the criminal activity of the 'disadvantaged' sample may be due to negative peer effects. Such a story would be consistent with Grönqvist and Lindqvist's (2016) argument that peer effects may explain the positive effect of officer training on educational attainment: young men assigned to the two officer ranks (sergeant and 2nd lieutenant) find themselves amongst a strongly positively selected group.

To study this potential mechanism, we take our IV sample and group those who actually serve into "units". These are not the actual units (or platoons) in which they serve, as we do not have access to data with platoon identifiers; instead, this is the pool from which platoon mates are drawn.[47] Our units are created by grouping men by test-year, *t*, regiment, *R*, and rank, *r*. There are 114 regiments in our data and four ranks – private, corporal, sergeant and 2nd lieutenant. We drop units with less than 10 members to allow for more accurate measures of average peer characteristics within a unit – namely pre-service crime. The median unit size is 142. The mean is 162 and the maximum is 589.

For each conscript, we calculate the leave-out average pre-service crime rate of all other men in his unit, excluding himself, $\bar{y}_{(-i)Rrt-s}$. Figure 5 shows the distribution of the leave-out average pre-service number of crimes across units, reported separately by rank. Pre-service crime is clearly concentrated among units of peers from the lowest ranks. The entire distribution for privates, and to a lesser extent corporals, is markedly shifted to the right. Men from low SES backgrounds tend also to be concentrated in these units; 29% of privates have low education fathers, compared to 19% and 12 % for corporals and sergeants/2nd lieutenants, respectively. Thus, one unintentional side-effect of the recruitment and placement process is

[47] Platoons were typically comprised of 20 privates and corporals, one sergeant, and one 2nd lieutenant.

that high crime, low SES men are concentrated together, with intensive exposure over a long period of time. But could this exposure lead to peer effects in crime?

We estimate these potentially harmful peer effects using an identification strategy borrowed from the economics of education literature (see e.g. Hoxby 2000, Ammermueller and Pischke 2009, Gould et al. 2009 and Lavy and Schlosser 2011). This strategy has also been recently used by Østergaard et al. (2017) to study peer effects in juvenile delinquency. We estimate the following peer effects regression:

$$y_{iRrt} = \alpha_R + \beta_r + \gamma_t + \delta_{Rt} + \pi \bar{y}_{(-i)Rrt-s} + \boldsymbol{X}'_{it-s}\boldsymbol{\theta} + \varepsilon_{iRrt}. \quad (4)$$

The outcome variable, $y_{iRrt}$, is criminal activity between ages 23 and 30 of agent *i* who served in regiment *R* with rank *r* and test-year *t*. Equation (4) includes regiment fixed effects, $\alpha_R$, rank fixed effects, $\beta_r$, test-year fixed effects, $\gamma_t$, and test-year by regiment linear time trends, $\delta_{Rt}$. The main variable of interest is $\bar{y}_{(-i)Rrt-s}$, i.e. average pre-service crime among the young men *j* who serve together in soldier *i*'s unit (excluding agent *i* himself). The corresponding coefficient, $\pi$, measures the peer effect. $\boldsymbol{X}'_{it-s}$ includes dummies for birth month, birth year and municipality, as well as the full set of individual level test and pre-test-day controls.

The identification argument is that after conditioning on regiment, rank, test-year, and regiment-specific linear time trends, there exists random variation in pre-service crime across successive cohorts within the same regiment and rank. This strategy addresses the fact that soldiers are not randomly assigned to ranks and/or regiment, but rather they are sorted into their service positions based on observable characteristics, which are correlated across all *j* in agent *i*'s unit (as shown above). In equation (4), we only use the within-regiment variation in unit members' pre-service crime that arises across successive cohorts of each rank. Furthermore, we control for $\boldsymbol{X}'_{it-s}$, which includes lagged crime, $y_{it-s}$, as a control for individual level fixed effects. We cluster standard errors by regiment.[48]

Before presenting results, we first assess the plausibility of our identifying assumption by testing whether the variation across successive cohorts within rank and within regiment is, in fact, as good as random. At the very least, this variation should be uncorrelated with a set of observables that are known to be strong predictors of crime. Following Bayer et al. (2009) and Østergaard et al. (2017), we use $\boldsymbol{X}'_{it-s}$ together with regiment and rank fixed effects to predict future crime outcomes $\hat{y}_{iRrt}$. In Appendix Table A9, we regress $\hat{y}_{iRrt}$ onto our peer group measure, $\bar{y}_{(-i)Rrt-s}$. We find that that these two measures are, in fact, highly correlated with each other. However, after conditioning on regiment, rank, test-year, and regiment-specific linear trends, this correlation goes to zero. This implies that our identifying variation is uncorrelated with our rich set of observables, $X'_{it-s}$, as summarized by the index $\hat{y}_{iRrt}$.

In Table 11, we estimate potential peer effects by regressing own post-service crime (at ages 23-30) on the leave-out mean pre-service crime rate in one's unit and include the above mentioned fixed effects and $\boldsymbol{X}'_{it-s}$. The first specification looks at the baseline relationship between peer criminal history and an individual's post-service crime. The second and third specifications interact peer criminal history with whether the individual himself has a criminal history or a low educated father, respectively. The extensive margin crime outcome is considered in Columns (1) – (3) and the number of convictions in Columns (4) – (6).[49]

For men from advantaged backgrounds, exposure to peers with a criminal history does not increase post-service crime. But, the results in Table 11 are indicative of strong peer

[48] Lavy and Schlosser (2011) study gender peer effects at school. Their Equation (4) includes school, grade, and year fixed effects, as well as school-specific linear time trends. Our regiments are equivalent to their schools and our ranks are equivalent to their grades. Their identification strategy relies on idiosyncratic variations in gender composition across adjacent cohorts within the same schools, while ours relies on idiosyncratic variation in the pre-service crime records of adjacent cohorts of privates (for example) within the same regiment. They cluster standard errors on schools, while we cluster on regiments.

[49] Unit (peer) pre-service crime is defined in each exercise consistently with the dependent variable measure.

influences for disadvantaged individuals; increased exposure to peers with a pre-service criminal history is associated with higher post-service crime for conscripts from lower SES households. For example, having a criminal record prior to service increases the likelihood of committing a crime post-service by almost nine percentage points; evaluated at the mean, exposure to peers with a criminal history further increases the chance of post service crime by an additional three percentage points.[50] Thus, these peer effects appear to reinforce the criminal path that individuals are already on.[51, 52]

Taken together, these findings make negative peer effects one plausible mechanism behind the detrimental effects of service on crime for 'disadvantaged' men. Quantifying this effect, however, is quite difficult, since (i) we lack a well-defined measure of the counterfactual peer groups that these young men would have faced if they had not been placed in service, and (ii) military service not only changes the composition of peers but also the intensity of peer interactions. In this way, peer effects of service could be quite strong even if the average peer characteristics are not that different from those in the counterfactual.

### *5.4. Desensitization to Violence*

A final potential explanation is that a desensitization to violence and weapons can exacerbate post-service crime; however, we cannot study this channel directly in our framework. Though such an explanation would seem more feasible during wartime enlistment, we cannot conclusively rule it out as playing a role in the current context.

[50] Multiplying the coefficient on the interaction term in column (2) of Table 11 by the mean of unit pre-service crime for those with a prior conviction gives us 0.200*0.131 = 0.026, i.e. approximately 3 percentage points.

[51] This is consistent with the findings of crime-specific reinforcing peer effects in juvenile correctional facilities by Bayer et al. (2009). Such non-linear peer effects imply that how conscripts are allocated to a unit can affect post-service crime. One way to limit the potential negative (unintended) effects of military service may be to not group all "bad apples" together. However, as demonstrated by Carrell et al. (2013), caution should be exercised when trying to optimally design peer groups based on reduced form peer effects. They in fact found perverse effects of the 'optimal' assignments to squadrons in the United States Air force Academy.

[52] Results are unchanged if we also include the additional fixed effects that we have in our IV specification; namely county by year fixed effects and test office by test month by test year fixed effects. Other robustness checks indicate that there is no single rank driving all of the results.

## 6. Conclusion

With the end of the Cold War, numerous countries in Europe abolished mandatory military conscription. With no imminent military threat, and with the security of NATO or EU membership, it became hard for politicians to both justify the financial costs of such a large-scale national policy and convince voters of the need for it on civic grounds alone (Bieri, 2015). In recent years, the debate has about-faced, with many countries considering a re-instatement of mandatory conscription in some form. Perhaps not surprisingly, with the annexation of Crimea by Russia, both Lithuania and the Ukraine have already reinstated the draft. However, this conversation is also happening in countries in Western Europe – namely France, Italy, and the UK (Bieri, 2015). Sweden, the subject of this study, has in fact already reinstated a limited draft in March 2017. While one should clearly debate these issues with respect to the direct costs and likely need for and competence of such a military, one must also consider the potential indirect costs associated with mandatory conscription.

Using an instrumental variable approach that takes advantage of exogenous variation in the likelihood of service due to randomly assigned draft board officiators, we show that the potential indirect costs of mandatory conscription may indeed be high. We find that conscription significantly increases post-service crimes from ages 23 to 30 across a number of crime categories, and especially for 'high risk' populations with respect to future crime. Though we find evidence of a contemporaneous incapacitation effect of service on those with a prior criminal history, this temporary drop in crime does not appear to remove this high risk population from a path of future crime.

Importantly, our results do not support the hypothesis that military service can "straighten out" troubled young men. Instead, service in our context compounds pre-existing behavioural problems. This stands in contrast to Albaek et al. (2017) who studied an older cohort that served in Denmark, but is consistent with results reported in more recent work for Denmark (Vincent Lyk-Jensen, 2018) and the United States (Wang and Flores-Lagunes, 2017). Our results are also consistent with those reported in the literature investigating the effects of 'military boot camps' aimed at treating juvenile delinquents (MacKenzie and Farrington, 2015). As such, our own results may generalize beyond the specific setting that we study.

We highlight two mechanisms that we believe are responsible for the crime inducing effects of mandatory military service. First, we show that military service appears to have a negative impact on the labour market outcomes of young men from disadvantaged backgrounds, but a positive effect on those from advantaged backgrounds. Thus, post-service labour market outcomes may mediate part of the causal effect of service on crime. Second, we find a strong causal relationship between peer pre-service criminal history and an individual's post service crime, but only for those from disadvantaged backgrounds. We argue that these negative peer interactions are plausibly the main channel through which military service increases crime.

Two policy recommendations follow directly from our analysis of mechanisms. First, in the new draft regime, the Swedish military will test (approximately) four young men and women for every position that needs to be filled. This will give them a large enough pool of potential conscripts to avoid sorting all low SES individuals into one job and all high SES individuals into another job. The military could try to avoid hiring "bad apples" altogether or, at the very least, avoid putting them all into the same barrel. Second, the Swedish military could (and we argue should) work together with the conscripts' own union organization to provide post-service job placement and/or study counselling services. This could help dampen the negative effects that service may have on the labour market outcomes of disadvantaged youths.

Other important social programs and institutions share some features of Sweden's former conscription system. For example, mandatory schooling, labour market training programs, juvenile detentions centres, and "boot camps" for troubled youths all have the potential to incapacitate young disadvantaged men during their crime prone ages. But, as we see in this study, incapacitation effects may be short lived and then reversed shortly after the program is over. Importantly, the content and the context of each program matters. For example, extending compulsory schooling in Sweden appears to have both raised the human capital of disadvantaged boys and provided them with a better pool of peers (Hjalmarsson et al., 2015), while (at the other extreme) juvenile jails in the United States appear to interfere with schooling and generate strong negative peer effects (Aizer and Doyle, 2015; Hjalmarsson, 2008; Bayer et al., 2009). Officer training in the Swedish military appears to build human capital and provide (primarily advantaged youths) with a set of positive peers (Grönqvist and Lindqvist, 2016). At the same time, military service worsens the outcomes of the majority of recruits from disadvantaged background who do not serve as officers. As such, Sweden's mandatory military service appears to have magnified pre-existing inequalities – an important (albeit unintended) consequence of mandatory service.

**Author Affiliations**

University of Gothenburg and CEPR

Swedish Institute for Social Research, Stockholm University

## References


Aizer, A. and Doyle, J. (2015). ‘Juvenile incarceration, human capital and future crime: evidence from randomly-assigned judges’, *Quarterly Journal of Economics*, vol. 130(2), pp. 759-803.

Albaek, K., Leth-Petersen, S., le Maire, D. and Tranaes, T. (2017). ‘Does peacetime military service affect crime?’, *Scandinavian Journal of Economics*, vol. 119(3), pp. 512-40

Albrecht, J. W., Edin, P.-A., Sundström, M. and Vroman, S. B. (1999). ‘Career interruptions and subsequent earnings: a re-examination using Swedish data’, *Journal of Human Resources*, vol. 34(2), pp. 294-311.

Ammermueller, A. and Pischke, J.-S. (2009). ‘Peer effects in European primary schools: evidence from the progress in international reading literacy study’, *Journal of Labour* Economics, vol. 27(3), pp. 315-48.

Anderson, D. M. and Rees, D. (2015). ‘Deployments, combat exposure, and crime’, *Journal of Law and Economics*, vol. 58, pp. 235-67.

Angrist, J. (1990). ‘Lifetime earnings and the Vietnam era draft lottery: evidence from social security administrative records’, *American Economic Review*, vol. 80(3), pp. 313-36.

Angrist, J. and Chen, S. (2011). ‘Schooling and the Vietnam era GI bill: evidence from the draft lottery’, *American Economic Journal: Applied Economics*, vol. 3(2), pp. 96-118.

Angrist, J., Chen, S. and Song, J. (2011). ‘Long-term consequences of Vietnam era conscription: new estimates using social security data’, *American Economic Review: Papers and Proceedings*, vol. 101(3), pp. 334–38.

Bayer, P., Hjalmarsson, R. and Pozen, D. (2009). ‘Building Criminal Capital Behind Bars: Peer Effects in Juvenile Corrections’, *Quarterly Journal of Economics*. Vol. 124(1), pp. 105-47.

Becker, G. (1968). ‘Crime and punishment: an economic approach’, *The Journal of Political Economy*, vol. 76(2), pp. 169-217.

Beckerman, A. and Fontana, L. (1989). ‘Vietnam veterans and the criminal justice system: a selected review’, *Criminal Justice and Behaviour*, vol. 16 (4), pp. 412-28.

Bedard, K. and Deschênes, O. (2006). ‘The long-term impact of military service on health outcomes: evidence from World War II and Korean war veterans’, *The American Economic Review*, vol. 96(1), pp. 176-94.

Bieri, M. (2015). ‘Military conscription in Europe: new relevance’, *CSS Analyses in Security Policy*, Center for Security Studies, ETH Zurich.

Bhuller, M., Dahl, G. B., Løken, K. V. and Mogstad, M. (2016). ‘Incarceration, recidivism and employment’, NBER Working Paper 22648.

Bhuller, M., Dahl, G. B., Løken, K. V. and Mogstad, M. (2018). 'Intergenerational effects of incarceration', *AEA Papers and Proceedings*, vol. 108, pp. 234-40.

Bingley, P., Lundborg, P. and Vincent Lyk-Jensen, S. (forthcoming). 'Opportunity cost and the incidence of a draft lottery', *Journal of Labor Economics*.

Böhlmark, A. and Lindquist, M. J. (2006). 'Life-cycle variations in the association between current and lifetime income: replication and extension for Sweden', *Journal of Labour Economics*, vol. 24(4), pp. 879-96.

Card, D. and Cardoso, A. R. (2012). 'Can compulsory military service raise civilian wages? evidence from the peacetime draft in Portugal', *American Economic Journal: Applied Economics*, vol. 4(4), pp. 57-93.

Carlsson, M., Dahl, G. B., Öckert, B. and Rooth, D.-O. (2015). 'The effect of Schooling on cognitive skills', *Review of Economics and Statistics*, vol. 97(3), pp. 533-47.

Dobkin, C. and Shabani, R. (2009). 'The health effects of military service: evidence from the Vietnam draft', *Economic Inquiry*, vol. 47(1), pp. 69-80.

Carrell, S., Sacerdote B. and West, J. (2013). 'From natural variation to optimal policy? The importance of endogenous peer group formation', *Econometrica*, vol. 81(3), pp. 855-82.

Dahl, G. B., Kostøl, A. R. and Mogstad, M. (2014). 'Family welfare cultures', *Quarterly Journal of Economics*, vol. 129(4), pp. 1711-52.

Dobbie, W., Goldin, J. and Yang, C. (2018). 'The effects of pre-trial detention on conviction, future crime, and employment: evidence from randomly assigned judges', *American Economic Review*, vol. 108(2), pp. 201-40.

Doyle Jr., J. J. (2007). 'Child protection and child outcomes: measuring the effects of foster care', *American Economic Review*, vol. 97(5), pp. 1583-1610.

Doyle Jr., J. J. (2008). 'Child protection and adult crime: using investigator assignment to estimate causal effects of foster care', *Journal of Political Economy*, vol. 116(4), pp. 746-70.

Galiani, S., Rossi, M. A. and Schargrodsky, E. (2011). 'The effects of peacetime and wartime conscription on criminal activity', *American Economic Journal: Applied Economics*, vol. 3(2), pp. 119-36.

Grenet, J., Hart, R. and Roberts, E. (2011). 'Above and beyond the call: long-term real Earnings effects of British male military conscription in the post-war years', *Labour Economics*, vol. 18(2), pp. 194-204.

Gould, E. D., Lavy, V. and Paserman, M. D. (2009). 'Does immigration effect the long-term educational outcomes of natives? Quasi-experimental evidence', *Economic Journal*, vol. 119(540), pp. 380-96.

Grossman, D. (1995). *On Killing. The Psychological Cost of Learning to Kill in War and Society*, Boston: Little, Brown and Company.

Grönqvist, E. and Lindqvist, E. (2016). 'The making of a manager: evidence from military officer training', *Journal of Labour Economics*, vol. 34(4), pp. 869-98.

Hanes, N., Norlin, E. and Sjöström, M. (2010). 'The civil returns of military training: a study of young men in Sweden', *Defense and Peace Economics*, vol. 21(5), pp. 547-65.

Hjalmarsson, R. and Lindquist, M. J. (2012). 'Like godfather, like son: exploring the intergenerational nature of crime', *Journal of Human Resources*, vol. 47(2), pp. 550-82.

Hjalmarsson, R. and Lindquist, M. J. (2016). 'The causal effect of military conscription on crime and the labor market', CEPR Discussion Paper 11110.

Hoxby, C. (2000). 'Peer effects in the classroom: learning from gender and race variation', NBER Working Paper 7867.

Jacob, B. and Lefgren, L. (2003). 'Are idle hands the devil's workshop? Incapacitation, concentration, and juvenile crime', *American Economic Review*, vol. 93(5), pp. 1560-77.

Kling, J. (2006). 'Incarceration length, employment, and earnings', *American Economic Review*, vol. 96(3), pp. 863-76.

Lavy, V. and Schlosser, A. (2011). 'Mechanisms and impacts of gender peer effects at school', *American Economic Journal: Applied Economics*, vol. 3(2), pp. 1-33.

Layton MacKenzie, D., Wilson, D. B. and Kider, S. B. (2001). 'Effects of correctional boot camps on offending', *The Annals of the American Academy of Political and Social Science*, vol. 578, pp. 126-43.

Lindo, J. M. and Stoecker, C. (2014). 'Drawn into violence: evidence on 'what makes a criminal' from the Vietnam draft lotteries', *Economic Inquiry*, vol. 52(1), pp. 239-58.

Luallen, J. (2006). 'School's out...forever: a study of juvenile crime, at-risk youths and teacher strikes', *Journal of Urban Economics*, vol. 59(1), pp. 75-103.

MacKenzie, D. and Farrington, D. (2015). 'Preventing future offending of delinquents and offenders: what have we learned from experiments and meta-analyses?', *Journal of Experimental Criminology*, vol. 11(4), pp. 565-95.

Maurin, E. and Xenogiani, T. (2007). 'Demand for education and labor market outcomes. lessons from the abolition of compulsory conscription in France', *Journal of Human Resources*, vol. 42(4), pp. 795-819.

Meade, B. and Steiner, B. (2010). 'The total effects of boot camps that house juveniles: a systematic review of the evidence', *Journal of Criminal Justice*, vol. 38(5), pp. 841-53.

Meghir, C., Palme, M. and Schnabel, M. (2012). 'The effect of education policy on crime: an intergenerational perspective', Working Paper, Stockholm University.

Mueller-Smith, M. (2015). ‘The criminal and labor market impacts of incarceration,” Working Paper, University of Michigan.

Resnick, H. S., Foy, D. W., Donahoe, C. P. and Miller, E. N. (1989). ‘Antisocial behaviour and post-traumatic stress disorder in Vietnam veterans’, *Journal of Clinical Psychology*. Vol. 45(6), pp. 860-66.

Rohlfs, C. (2010). ‘Does combat exposure make you a more violent or criminal person? Evidence from the Vietnam draft’, *Journal of Human Resources*, vol. 45(2), pp. 271-300.

Siminski, P. (2013). ‘Employment effects of army service and veterans compensation: evidence from the Australian Vietnam-era conscription lotteries’, *Review of Economics and Statistics*, vol. 95(1), pp. 87-97.

Siminski, P., Ville, S. and Paull, A. (2016). ‘Does the military train men to be violent criminals? new evidence from Australia’s conscription lotteries’, *Journal of Population Economics*, vol. 29(1), pp. 197-218.

Vincent Lyk-Jensen, S. (2018). ‘Does peacetime military service affect crime? New evidence from Denmark's conscription lotteries’, *Labour Economics*, vol. 52, pp. 245-62.

Wang, X. and Flores-Lagunes, A. (2017). ‘Conscription and military service: do they result in future violent and non-violent incarceration and recidivism?’, Working Paper, Syracuse University.

Yesavage, J. (1983). ‘Differential effects of Vietnam combat experiences vs criminality on dangerous behaviour by Vietnam veterans with schizophrenia’, *The Journal of Nervous and Mental Disease*, vol. 171(6), pp. 382-84.

Østergaard Larsen, B. and Kristensen, N. (2017). ‘Building human capital or criminal capital? School peer effects on future offending,” IZA Discussion Paper 11124.

*Figure 1. Share of Sweden born Males Who Were Tested, Share Assigned to a Service Category, and Share Who Served in the Military by Birth Cohort.*

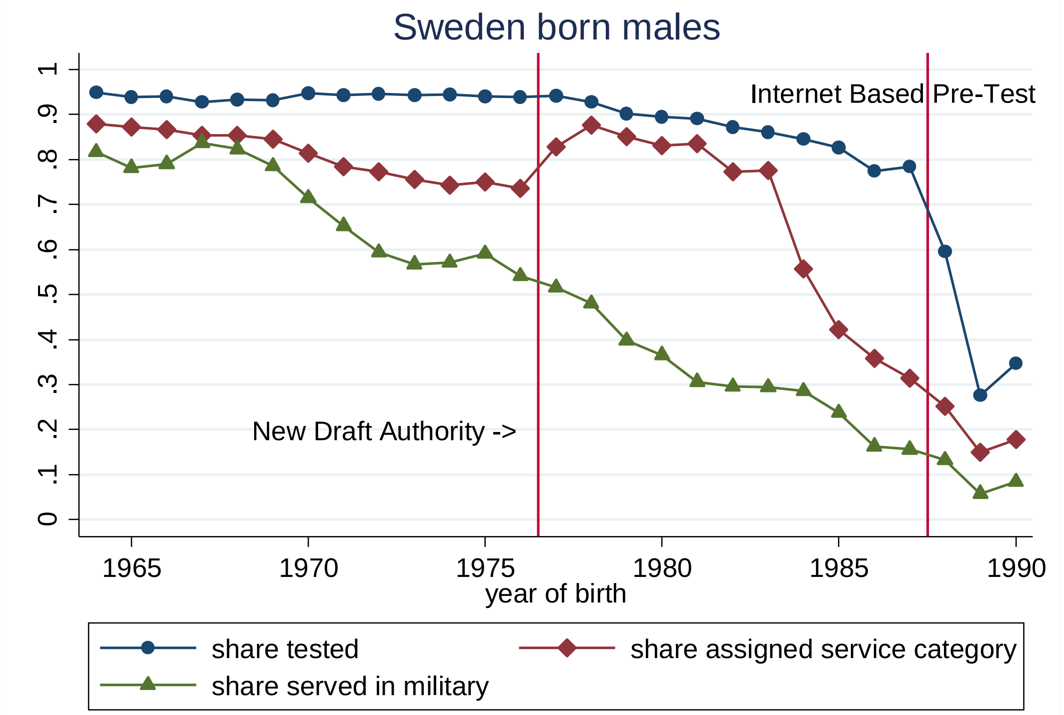
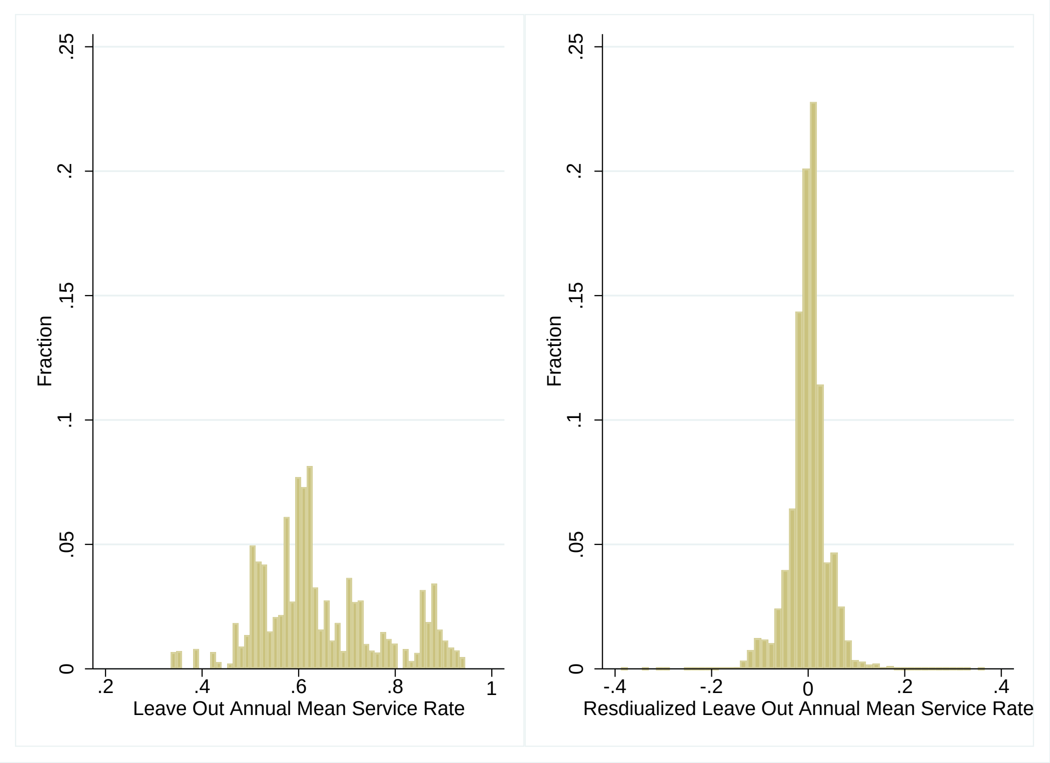
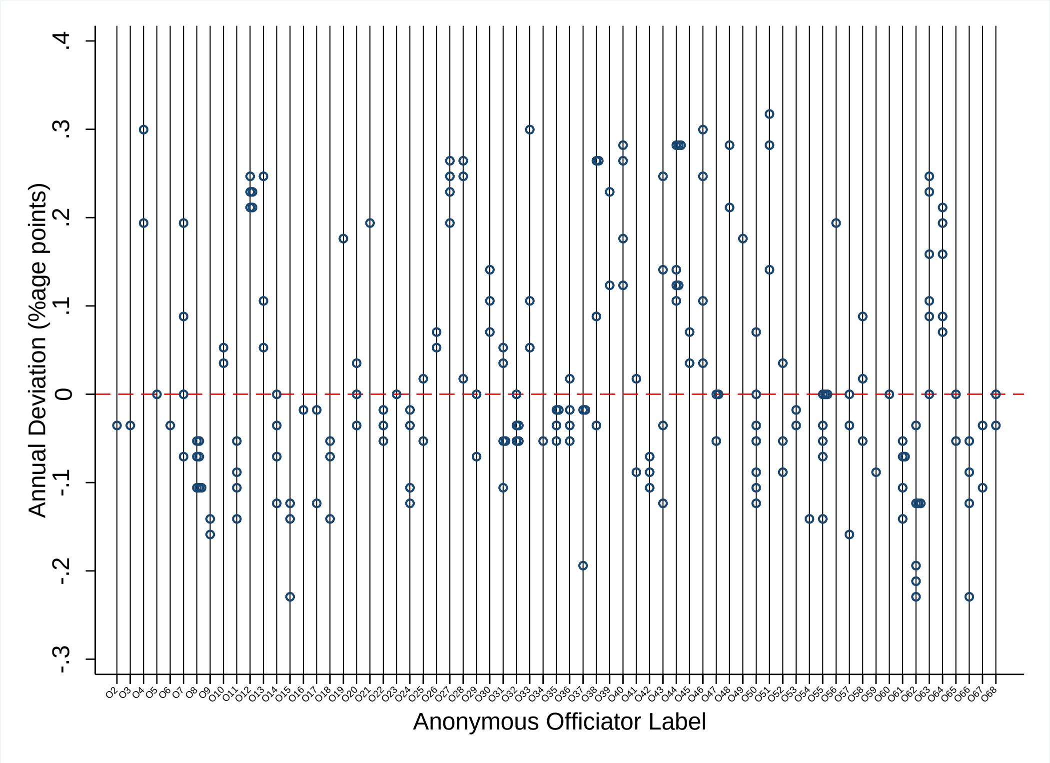


*Figure 2. Variation in Officiator Annual Service Rate*




Note – The residualized leave out annual mean is from a regression of the leave out annual mean on county by test year and test office by test month fixed effects.

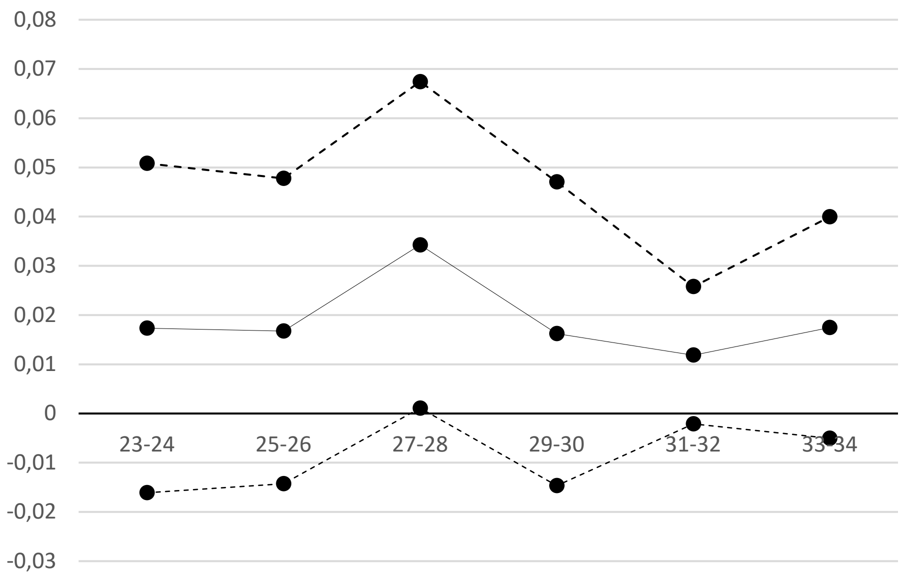
*Figure 3. Persistency of Officiator Leave Out Mean and Dichotomous Classification*



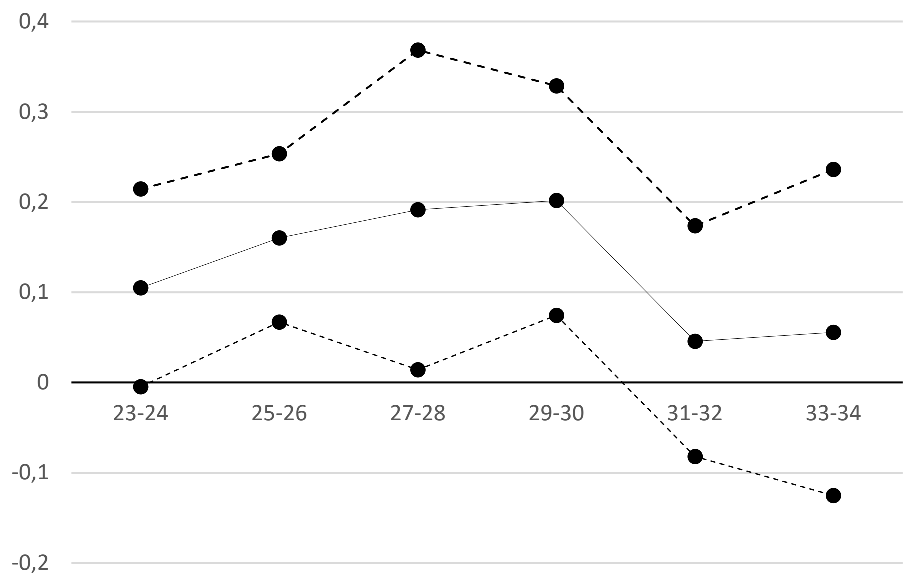

Note - This figure plots the deviation of the annual share who serve for each officiator from the national share in that year. Each vertical line represents an officiator, while each dot on the line represents the share who serve for a single year. The horizontal red dashed line at zero indicates that the officiator share serve is equal to the national share served in a given year. Note that the national annual share that serve decreases from 66% in 1990 to 52% in 1996. The number of dots on the line therefore indicate the number of years the officiator is observed in the data (with 100 or more cases).

*Figure 4. Path Dynamics of the Effect of Service on Crime*

Panel A. Extensive Margin Dynamics: Estimates and 95% CI for Any Crime

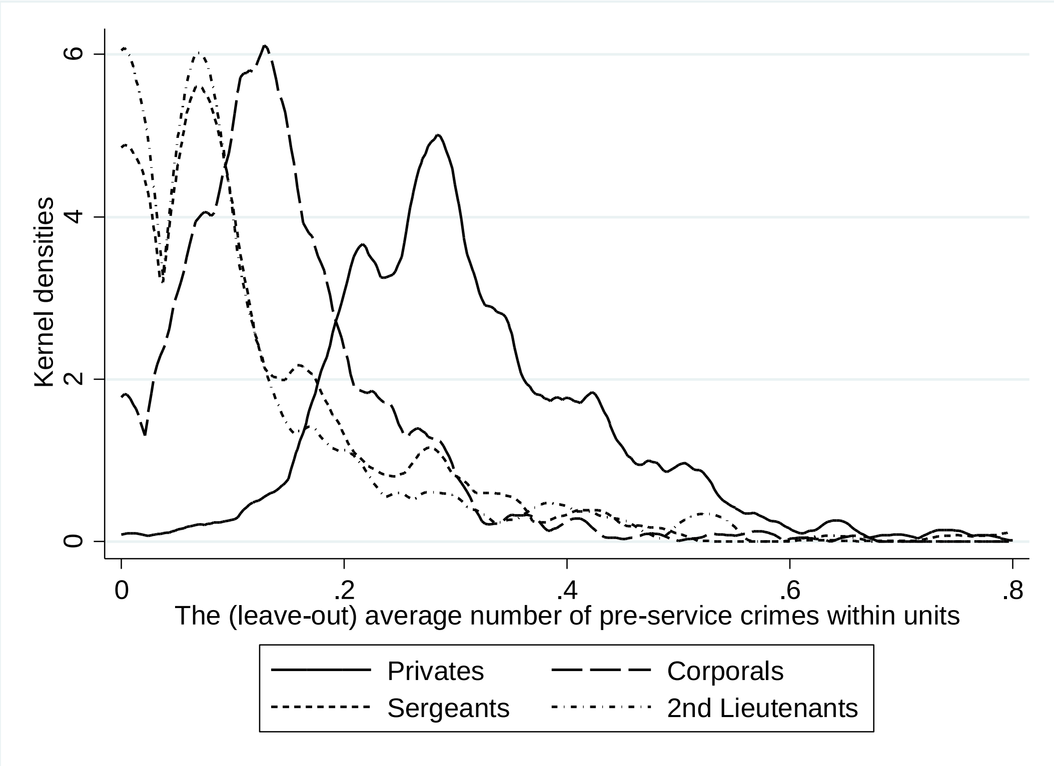



Panel B. Intensive Margin Dynamics: Estimates and 95% CI for # of Crimes




Note – The solid line represents the point estimates of effect of service on the chance of conviction (Panel A) and number of convictions (Panel B) for each two-year age interval. That is, each point estimate is the result of the baseline instrumental variable specification where the dependent variable is defined as crime (extensive or intensive) at the ages listed on the x-axis. The dashed lines correspond to the 95% confidence interval.

*Figure 5. Kernel Densities Over Distribution of the "Leave-Out" Mean Number of Pre-Service Crimes in Each Unit by Rank*




Note – This figure is based on men in our IV sample who actually serve in the military.

*Table 1. Summary Statistics: Overall and By Service*

| | All Individuals | | Service = 1 | Service = 0 |
|---|---|---|---|---|
| | *N = 168805* | | *N=126539* | *N = 42266* |
| Variable | Mean | SD | Mean | Mean |
| military service (tax records) | 0.75 | 0.43 | 1.00 | 0.00 |
| Leave out Annual Mean | 0.64 | 0.13 | 0.66 | 0.59 |
| high service rate officiator (Leave Out Mean > Annual Mean) | 0.50 | 0.50 | 0.56 | 0.35 |
| test_year | 1992.76 | 1.98 | 1992.72 | 1992.88 |
| birth_year | 1974.57 | 2.03 | 1974.55 | 1974.62 |
| | | | | |
| *Pre-test day controls* | | | | |
| Any weapons < 18? | 0.01 | 0.08 | 0.01 | 0.01 |
| Any violent < 18? | 0.02 | 0.14 | 0.02 | 0.02 |
| Any traffic < 18? | 0.06 | 0.23 | 0.05 | 0.06 |
| Any theft < 18? | 0.06 | 0.24 | 0.06 | 0.07 |
| Any other < 18? | 0.04 | 0.19 | 0.04 | 0.04 |
| Any drugs< 18 | 0.00 | 0.06 | 0.00 | 0.00 |
| schooling father | 11.14 | 2.65 | 11.16 | 11.08 |
| schooling mother | 11.55 | 2.35 | 11.57 | 11.49 |
| income father | 12.32 | 0.40 | 12.32 | 12.20 |
| income mother | 11.87 | 0.39 | 11.87 | 11.72 |
| 2 year school? | 0.18 | 0.39 | 0.18 | 0.19 |
| 3 year school? | 0.76 | 0.43 | 0.77 | 0.72 |
| | | | | |
| *Test day controls* | | | | |
| Height | 179.75 | 6.44 | 179.77 | 179.66 |
| weight | 71.59 | 10.53 | 71.65 | 71.40 |
| BMI | 22.14 | 2.89 | 22.15 | 22.10 |
| Ability Score | 5.17 | 1.84 | 5.22 | 5.05 |
| Physical Capacity Score | 6.26 | 1.45 | 6.35 | 5.98 |
| Psychological Test Score | 5.35 | 1.53 | 5.49 | 4.92 |
| | | | | |
| *Selected Outcome variables* | | | | |
| Any crimes 23-30? | 0.10 | 0.31 | 0.10 | 0.11 |
| # crimes 23-30 | 0.31 | 2.33 | 0.28 | 0.40 |
| 2 or more crimes 23-30? | 0.04 | 0.20 | 0.04 | 0.05 |
| 5 or more crimes 23-30? | 0.01 | 0.11 | 0.01 | 0.02 |
| Any weapons 23-30? | 0.01 | 0.07 | 0.01 | 0.01 |
| Any violent 23-30? | 0.02 | 0.15 | 0.02 | 0.03 |
| Any traffic 23-30? | 0.06 | 0.25 | 0.06 | 0.07 |
| Any theft 23-30? | 0.01 | 0.11 | 0.01 | 0.02 |
| Any other 23-30? | 0.02 | 0.15 | 0.02 | 0.03 |
| And drugs 23-30? | 0.02 | 0.13 | 0.01 | 0.02 |
| schooling | 12.95 | 2.10 | 12.98 | 12.85 |
| More than 12 years school? | 0.42 | 0.49 | 0.42 | 0.41 |
| (log) income_30_34 (with zeros) | 12.15 | 1.91 | 12.20 | 11.99 |
| Any unemployment benefits 23-34? | 0.46 | 0.50 | 0.46 | 0.45 |

Note – Missing observations are replace with sample means for control variables.

*Table 2. First Stage Estimates of the Impact of Officiator Service Propensity on Military Service*

| | (1) | (2) | (3) |
|---|---|---|---|
| | Dependent variable = military service | | |
| | Full Sample | | |
| *Panel A: Continous Instrument: Leave Out Annual Mean* | | | |
| leave_out_annual_mean | 0.66438*** | 0.66111*** | 0.64393*** |
| | [0.08201] | [0.08197] | [0.07652] |
| *F-Statistic* | 66 | 65 | 71 |
| R-squared | 0.08 | 0.08 | 0.11 |
| *Panel B: Dichotomous Instrument: Leave Out Annual Mean > Annual Mean service rate* | | | |
| High Service Rate Officiator | 0.07273*** | 0.07231*** | 0.07046*** |
| | [0.01229] | [0.01229] | [0.01171] |
| *F-Statistic* | *35* | *35* | *36* |
| R-squared | 0.08 | 0.08 | 0.1 |
| Observations | 168805 | 168805 | 168805 |
| Mean Dependent Variable | 0.75 | 0.75 | 0.75 |
| county FE | yes | yes | yes |
| test year FE | yes | yes | yes |
| county x test year FE | yes | yes | yes |
| test office FE | yes | yes | yes |
| test month FE | yes | yes | yes |
| test office x test month FE | yes | yes | yes |
| pre-test day characteristics | no | yes | yes |
| test day variables | no | no | yes |

Note – Robust standard errors in brackets, clustered by officiator. * significant at 10%; ** significant at 5%; *** significant at 1%

*Table 3. Balancing Test of Conditional Random Assignment: Leave Out Annual Mean Service Rate Above and Below Median*

| | Sample Mean | | % Difference | p-values of comparisons : no controls | county x test year FE | county x year FE office x month FE |
|---|---|---|---|---|---|---|
| | Bottom half | Top half | | | | |
| Variable | (1) | (2) | (3) | (4) | (5) | (6) |
| leave out annual mean | 0.55 | 0.74 | 34.7% | **0.021** | **0.009** | **0.000** |
| test_year | 1992.76 | 1992.75 | 0.0% | 0.973 | 0.325 | 0.458 |
| birth_year | 1974.56 | 1974.57 | 0.0% | 0.972 | 0.644 | 0.051 |
| Any weapons < 18? | 0.01 | 0.01 | 41.4% | 0.059 | 0.792 | 0.967 |
| Any violent < 18? | 0.02 | 0.02 | 11.5% | 0.213 | 0.987 | 0.913 |
| Any traffic < 18? | 0.06 | 0.05 | -12.6% | **0.000** | 0.445 | 0.821 |
| Any theft < 18? | 0.05 | 0.07 | 21.8% | **0.000** | 0.388 | 0.560 |
| Any other < 18? | 0.04 | 0.04 | 10.0% | **0.006** | 0.560 | 0.370 |
| Any drugs< 18 | 0.00 | 0.00 | -4.5% | 0.652 | 0.448 | 0.385 |
| schooling father | 10.94 | 11.35 | 3.8% | **0.000** | 0.547 | 0.583 |
| schooling mother | 11.39 | 11.72 | 2.9% | **0.000** | 0.340 | 0.200 |
| income father | 12.30 | 12.33 | 0.3% | **0.034** | **0.028** | 0.076 |
| income mother | 11.83 | 11.91 | 0.7% | **0.000** | 0.059 | 0.180 |
| 2 year school? | 0.19 | 0.18 | -3.5% | 0.789 | 0.515 | 0.811 |
| 3 year school? | 0.75 | 0.76 | 0.7% | 0.824 | 0.828 | 0.839 |
| Height | 179.77 | 179.72 | 0.0% | 0.471 | 0.977 | 0.682 |
| weight | 71.70 | 71.46 | -0.3% | 0.144 | 0.105 | 0.615 |
| BMI | 22.17 | 22.11 | -0.3% | 0.207 | 0.070 | 0.506 |
| Ability Score | 5.13 | 5.22 | 1.6% | **0.012** | 0.372 | 0.597 |
| Physical Capacity Score | 6.21 | 6.30 | 1.4% | 0.165 | 0.691 | 0.362 |
| Psychological Test Score | 5.29 | 5.41 | 2.3% | **0.035** | 0.210 | 0.254 |

Note – This table compares the sample means for those with leave out annual means above and below the median. (Note that some variables, particularly parent schooling and income, are missing a little less than 20% of observations. Any regressions with these controls will include dummies indicating missing values.) Column (3) presents the percentage difference. Column (4) presents p-values of tests of whether the top and bottom sample means differ; these are obtained from regressions of the 'variable' on dummies for the top half of the data, clustering on officiator. Column (5) and (6) present the p-values from comparable regressions, which include county by test year fixed effects, i.e. county dummies, test year dummies, and their interaction, and month x test office fixed effects (including month fixed effects and office fixed effects as well.) Bold indicates statistical significance at 5%.

*Table 4. Do High Service Officiators Affect Type of Service?*

| | Dependent Variable: | | | | | | | |
|---|---|---|---|---|---|---|---|---|
| | military service | army | navy | airforce | private | corporal | sergeant or 2nd lieutenant | combatant |
| *Panel A: Continous Instrument: Leave Out Annual Mean* | | | | | | | | |
| leave_out_annual_mean | 0.66438*** | -0.10639 | 0.15855** | -0.07121** | -0.11662 | 0.08316 | 0.03404 | -0.15064* |
| | [0.08201] | [0.09972] | [0.06651] | [0.03200] | [0.09148] | [0.07135] | [0.04731] | [0.08888] |
| F-statistic | 66 | 1.14 | 5.68 | 4.95 | 1.63 | 1.36 | 0.52 | 2.87 |
| | | | | | | | | |
| *Panel B: Dichotomous Instrument: Leave Out Annual Mean > Annual Mean service rate* | | | | | | | | |
| High Service Rate Officiator | 0.07273*** | -0.0008 | 0.00839 | -0.00825 | -0.00727 | 0.00814 | -0.00078 | -0.01827* |
| | [0.01229] | [0.00928] | [0.00600] | [0.00684] | [0.01421] | [0.01007] | [0.00641] | [0.00992] |
| F-Statistic | 35 | 0.01 | 1.96 | 1.45 | 0.26 | 0.65 | 0.01 | 3.39 |
| | | | | | | | | |
| Mean Dependent Variable | 0.75 | 0.76 | 0.11 | 0.12 | 0.71 | 0.2 | 0.09 | 0.47 |
| county FE | yes | yes | yes | yes | yes | yes | yes | yes |
| test year FE | yes | yes | yes | yes | yes | yes | yes | yes |
| county x test year FE | yes | yes | yes | yes | yes | yes | yes | yes |
| test office FE | yes | yes | yes | yes | yes | yes | yes | yes |
| month FE | yes | yes | yes | yes | yes | yes | yes | yes |
| office x month FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Observations | 168805 | 165928 | 165928 | 165928 | 168805 | 168805 | 168805 | 152158 |

Note – Robust standard errors in brackets, clustered by officiator. * significant at 10%; ** significant at 5%; *** significant at 1%

*Table 5. OLS, Reduced Form, and Instrumental Variable Baseline Results*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | | | 2SLS | | | |
| | OLS | | Reduced Form (continous instrument) | | Continous Instrument = leave out annual mean | | Dichotomous Instrument = High Service Rate Officiator | |
| *Panel A: Dependent Variable = Any Crimes from Age 23-30 (Mean = 0.10)* | | | | | *{UC_Mean = 0.20}* | | *{UC_Mean = 0.16}* | |
| military_service | -0.01708*** | -0.00500** | 0.04308** | 0.04222** | 0.06484** | 0.06557*** | 0.10188* | 0.10349** |
| | [0.00280] | [0.00203] | [0.01949] | [0.01618] | [0.02867] | [0.02485] | [0.05235] | [0.04685] |
| *Panel B: Dependent Variable = # Crimes from Age 23-30 (Mean = 0.31)* | | | | | *{UC_Mean = 0.42}* | | *{UC_Mean = 0.51}* | |
| military_service | -0.13512*** | -0.04895*** | 0.43705*** | 0.43107*** | 0.65783*** | 0.66944*** | 0.59591** | 0.61383*** |
| | [0.01948] | [0.01626] | [0.10993] | [0.09152] | [0.20061] | [0.18587] | [0.25931] | [0.21835] |
| *Panel C: Dependent Variable = # Crimes from Age 23-30 >=2 (Mean = 0.042)* | | | | | *{UC_Mean = 0.14}* | | *{UC_Mean = 0.12}* | |
| military_service | -0.01316*** | -0.00450*** | 0.01795 | 0.01819 | 0.02701 | 0.02824 | 0.04605 | 0.04834* |
| | [0.00150] | [0.00104] | [0.01409] | [0.01163] | [0.02132] | [0.01840] | [0.02941] | [0.02546] |
| *Panel D: Dependent Variable = # Crimes from Age 23-30 >=5 (Mean = 0.013)* | | | | | *{UC_Mean = 0.02}* | | *{UC_Mean = 0.01}* | |
| military_service | -0.00615*** | -0.00193** | 0.01230* | 0.01217* | 0.01852* | 0.01889* | 0.01848 | 0.01932 |
| | [0.00083] | [0.00072] | [0.00685] | [0.00634] | [0.01062] | [0.01027] | [0.01477] | [0.01359] |
| First Stage F-Statistic | | | | | 66 | 71 | 35 | 36 |
| County x Test Year FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Office x Month FE | yes | yes | yes | yes | yes | yes | yes | yes |
| Pre-test and Test day Controls | no | yes | no | yes | no | yes | no | yes |

Note – Columns (1) and (2) present the results of regressing crime from age 23 to 30 (at the extensive margin in Panel A and various intensive margin measures in Panels B - D) on military service and the indicated set of controls. County x test year fixed effects includes both county dummies, test year dummies, and county by test year dummies. Likewise, office x month fixed effects include office fixed effects and month fixed effects. (Note that this is the continuous month, from 1 to 84, and not month of the year.) For the ease of presentation, just the coefficient on military service is reported. Columns (3) - (4) present the reduced form using our baseline instrument: the continuous leave out annual mean service rate of the assigned officiator. Columns (5) - (8) instrument for military service with either the continuous leave out mean service rate or a dummy indicating assignment to an officiator with a higher than average annual service rate. Robust standard errors, clustered by county in columns (1) and (2) and officiator in columns (3) - (8). *** significant 1%, ** significant 5%, * significant 10%. N= 168805 . *UC_Mean* is the mean outcome for the untreated compliers. See Appendix B.

*Table 6. Instrumental Variable Estimates of the Effect of Service on Post-Conscription Crime, by Crime Type*

| | Dependent Variable: | | | | | | |
|---|---|---|---|---|---|---|---|
| | (1) Any Crime | (2) Weapons | (3) Violent | (4) Traffic | (5) Theft | (6) Other | (7) Drugs/Alc |
| **Extensive Margin: Any Crime 23-30** | | | | | | | |
| Baseline | 0.06484** | 0.01397** | 0.01919* | 0.04270** | 0.00791 | 0.01439 | 0.01327 |
| | [0.02867] | [0.00665] | [0.01095] | [0.01884] | [0.00998] | [0.01355] | [0.01137] |
| + pre-test day and test day controls | 0.06557*** | 0.01434** | 0.02012* | 0.04100** | 0.00851 | 0.01575 | 0.01329 |
| | [0.02485] | [0.00640] | [0.01059] | [0.01673] | [0.00954] | [0.01290] | [0.00934] |
| *Mean Dependent Variable* | *0.1* | *0.0056* | *0.023* | *0.064* | *0.013* | *0.025* | *0.016* |
| **Intensive Margin: # crimes 23-30** | | | | | | | |
| Baseline | 0.65783*** | 0.04065*** | 0.05525** | 0.24621*** | 0.12265** | 0.03983 | 0.15323*** |
| | [0.20061] | [0.01453] | [0.02771] | [0.09432] | [0.06003] | [0.04152] | [0.04411] |
| + pre-test day and test day controls | 0.66944*** | 0.04092*** | 0.05819** | 0.24418*** | 0.12751** | 0.04121 | 0.15741*** |
| | [0.18587] | [0.01466] | [0.02779] | [0.09040] | [0.06176] | [0.04241] | [0.03650] |
| *Mean Dependent Variable* | *0.31* | *0.01* | *0.046* | *0.13* | *0.038* | *0.046* | *0.047* |
| **Intensive Margin: 2 or more crimes 23-30?** | | | | | | | |
| Baseline | 0.02701 | 0.00699** | 0.00389 | 0.01775* | 0.00746 | 0.02045* | 0.01085 |
| | [0.02132] | [0.00308] | [0.00765] | [0.00979] | [0.00660] | [0.01050] | [0.00829] |
| + pre-test day and test day controls | 0.02824 | 0.00707** | 0.00447 | 0.01742** | 0.00767 | 0.02092** | 0.01143 |
| | [0.01840] | [0.00308] | [0.00759] | [0.00875] | [0.00606] | [0.00905] | [0.00860] |
| *Mean Dependent Variable* | *0.042* | *0.0018* | *0.009* | *0.02* | *0.0051* | *0.0076* | *0.0079* |

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. N = 168,805. The baseline regressions control for test year x county and test office x month fixed effects. These controls are also included in all other regressions. All specifications instrument for military service with the leave out annual mean service rate of the assigned officiator. Just the coefficient on military service is presented. The first-stage F statistic equals 66 in the baseline (no controls) and 71 with the full set of controls.

*Table 7. Heterogeneity of the Effect of Service on Post-Service Crime*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) | (12 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Dependent Variable: | | | | | | | | | | | |
| | Any Convictions 23-30 | | # Convictions 23-30 | | >=2 Convictions 23-30 | | Weapons | Violent | Traffic | Theft | Other | Drugs/Alc |
| *Subsample* | Coeff/SE | Dep. Mean | Coeff/SE | Dep. Mean | Coeff/SE | Dep. Mean | Coeff/SE | Coeff/SE | Coeff/SE | Coeff/SE | Coeff/SE | Coeff/SE |
| *By Criminal History Before Age 18* | | | | | | | | | | | | |
| Prior Criminal History | 0.11628 | 0.245 | 4.67389** | 1.182 | 0.10198 | 0.133 | 0.10166* | 0.05077 | 0.06695 | 0.13141* | 0.08508 | 0.11835 |
| | [0.13146] | | [2.02544] | | [0.11375] | | [0.06113] | [0.05652] | [0.08337] | [0.07265] | [0.07609] | [0.07320] |
| No History | 0.04541* | 0.082 | -0.04604 | 0.178 | 0.00564 | 0.028 | -0.00021 | 0.00955 | 0.03104* | -0.01294 | 0.00037 | -0.00557 |
| | [0.02349] | | [0.11058] | | [0.01631] | | [0.00481] | [0.01087] | [0.01618] | [0.00890] | [0.01066] | [0.00909] |
| *By Father Schooling (SES proxy)* | | | | | | | | | | | | |
| Father <=9 years school | 0.04562 | 0.122 | 1.11546** | 0.418 | 0.07060** | 0.053 | 0.02488** | 0.06103** | 0.03043 | 0.01743 | 0.0167 | 0.05366** |
| | [0.04275] | | [0.46021] | | [0.02960] | | [0.01234] | [0.02603] | [0.04160] | [0.01686] | [0.02229] | [0.02328] |
| Father >9 years school | 0.06395 | 0.096 | 0.57655** | 0.269 | 0.01965 | 0.037 | 0.00433 | 0.00878 | 0.03474 | 0.0075 | 0.01291 | -0.00283 |
| | [0.04137] | | [0.27010] | | [0.03043] | | [0.00867] | [0.01554] | [0.02816] | [0.01222] | [0.01580] | [0.01717] |
| *Predicted Propensity to Serve:* | | | | | | | | | | | | |
| Quartile 1 (lowest) | 0.08580** | 0.149 | 1.78877*** | 0.641 | 0.04152 | 0.075 | 0.04426** | 0.02716 | 0.05915 | 0.03915* | 0.00989 | 0.05333** |
| | [0.04299] | | [0.66863] | | [0.03617] | | [0.02166] | [0.02515] | [0.03697] | [0.02192] | [0.02444] | [0.02571] |
| Quartile 2 | 0.04185 | 0.102 | 0.22628 | 0.266 | -0.01067 | 0.04 | 0.00186 | -0.00686 | 0.03154 | 0.01094 | 0.02961 | 0.02038 |
| | [0.06586] | | [0.29654] | | [0.03538] | | [0.01261] | [0.03045] | [0.04659] | [0.02128] | [0.02493] | [0.02051] |
| Quartile 3 | 0.05935 | 0.086 | 0.2719 | 0.191 | 0.05214 | 0.03 | 0.00406 | 0.04936*** | 0.01578 | -0.01638 | 0.01689 | -0.01119 |
| | [0.05653] | | [0.18472] | | [0.04074] | | [0.01182] | [0.01850] | [0.03848] | [0.01424] | [0.02135] | [0.01671] |
| Quartile 4 (highest) | 0.14736* | 0.078 | 0.30708 | 0.15 | 0.07142** | 0.024 | 0.01058 | 0.03395 | 0.09686* | 0.00916 | 0.04188* | -0.00289 |
| | [0.07641] | | [0.41984] | | [0.03320] | | [0.00926] | [0.02373] | [0.05854] | [0.02059] | [0.02302] | [0.02005] |

Note – Robust standard errors in brackets, clustered by officiator. * significant at 10%; ** significant at 5%; *** significant at 1% Subsamples based on predicted propensity to serve are based on regressions of service on test day and pre-test day controls. Baseline fixed effects are included but not used to calculate predicted propensity. Sample sizes and f-statistics for each subsample are as follows: criminal history (N = 22590, F = 32), no criminal history (N = 146215, F = 67), father <= 9 years school (N = 43904, F = 51), father > 9 years school (N = 95072, F = 45), Quartile 1 (N = 42201, F = 72), Quartile 2 (N=42201, F=63), Quartile 3 (N=42202, F = 44), Quartile 4 (N=42201, F=52).

*Table 8. Incapacitation Analysis*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
|---|---|---|---|---|---|---|---|---|
| | | | Potential Incapacitation Periods | | | | Post Service (age 23-30) | |
| | any crime 1st year after test | any crime 2nd year after test | any crime 1st and 2nd year after test | # crime 1st year after test | # crime 2nd year after test | # crime 1st and 2nd year after test | Any crime | # crimes |
| *Panel A: All 18, 19, 20 year testers (N=168395)* | | | | | | | | |
| Military Service | 0.02228* | -0.01227 | 0.00854 | 0.02463 | -0.0318 | -0.00716 | 0.06471** | 0.68338*** |
| | [0.01192] | [0.01568] | [0.01987] | [0.03088] | [0.04025] | [0.05541] | [0.02868] | [0.20415] |
| *Panel B: By Criminal History* | | | | | | | | |
| *18-20 year old testers with criminal history (N=22507)* | | | | | | | | |
| Military Service | 0.05418 | -0.13155* | -0.05441 | -0.06944 | -0.38619* | -0.45562 | 0.11671 | 4.78566** |
| | [0.08009] | [0.07297] | [0.10050] | [0.20794] | [0.21711] | [0.28673] | [0.12865] | [2.02161] |
| *18-20 year old testers with no criminal history (N=145889)* | | | | | | | | |
| Military Service | 0.01321 | 0.00053 | 0.01096 | 0.02365 | -0.00143 | 0.02222 | 0.04474* | -0.04919 |
| | [0.01224] | [0.01142] | [0.01737] | [0.01806] | [0.02074] | [0.02962] | [0.02369] | [0.11224] |
| *Panel C: By Father's Education* | | | | | | | | |
| *18-20 year old testers with low father education (N=43775)* | | | | | | | | |
| Military Service | 0.04139* | 0.03018 | 0.05226* | -0.01948 | 0.07158 | 0.0521 | 0.04954 | 1.14397** |
| | [0.02298] | [0.02265] | [0.03004] | [0.06896] | [0.09051] | [0.11278] | [0.04400] | [0.47023] |
| *18-20 year old testers with high father education (N=94880)* | | | | | | | | |
| Military Service | -0.01176 | -0.00723 | -0.01365 | 0.00788 | -0.07645* | -0.06856 | 0.06345 | 0.58622** |
| | [0.02191] | [0.02000] | [0.03268] | [0.04377] | [0.04379] | [0.07009] | [0.04063] | [0.27067] |

Note – Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10%. Military Service is instrumented for with leave out annual mean service rate of the assigned officiator. Each regression controls for test year x county and test office x test month fixed effects. The analysis is restricted to those who test at age 18, 19 or 20, which drops about 500 individuals who test at 17, 21, or 22.

*Table 9. IV Estimates for Non-Crime Outcomes: Education, Unemployment, and Income*

| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
|---|---|---|---|---|---|---|---|---|---|---|
| | | | Dependent Variable: | | | | | | | |
| | | | Schooling > 12 years | | # Years Unemp. 23-34 | | Income_30_34A | | Income_30_34B | |
| *Subsample* | N | *F-stat* | Coeff/SE | Dep. Mean | Coeff/SE | Dep. Mean | Coeff/SE | Dep. Mean | Coeff/SE | Dep. Mean |
| *Panel A. Whole Sample* | | | | | | | | | | |
| Baseline | 168805 | 66 | -0.03051 | 0.42 | -0.01369 | 1.55 | 0.14183** | 12.41 | 0.06451 | 12.15 |
| | | | [0.04293] | | [0.15866] | | [0.05566] | | [0.20855] | |
| *Panel B. By Criminal History Before Age 18* | | | | | | | | | | |
| Prior Criminal History | 22590 | 32 | -0.17597** | 0.22 | -0.53856 | 2.04 | -0.11173 | 12.28 | -1.08556** | 11.92 |
| | | | [0.08657] | | [0.60195] | | [0.19678] | | [0.47349] | |
| No History | 146215 | 67 | 0.00903 | 0.45 | 0.04017 | 1.48 | 0.19629*** | 12.43 | 0.23494 | 12.18 |
| | | | [0.04173] | | [0.13943] | | [0.05437] | | [0.21182] | |
| *Panel C. By Father Schooling (SES proxy)* | | | | | | | | | | |
| Father <=9 yearsschool | 43904 | 51 | 0.01578 | 0.44 | 0.52201 | 1.79 | -0.12805 | 12.37 | 0.00927 | 12.12 |
| | | | [0.04890] | | [0.31859] | | [0.10204] | | [0.28252] | |
| Father >9 years school | 95072 | 45 | -0.08642 | 0.50 | -0.47710** | 1.49 | 0.29735*** | 12.41 | 0.23834 | 12.10 |
| | | | [0.06642] | | [0.20778] | | [0.06147] | | [0.26331] | |
| *Panel D. Predicted Propensity to Serve:* | | | | | | | | | | |
| Quartile 1 (lowest) | 42201 | 72 | -0.06908 | 0.28 | 0.07891 | 1.99 | 0.09852 | 12.25 | -0.73607** | 11.93 |
| | | | [0.06668] | | [0.32778] | | [0.08337] | | [0.35777] | |
| Quartile 2 | 42201 | 63 | 0.01116 | 0.37 | 0.17885 | 1.62 | 0.14736 | 12.38 | 0.34977 | 12.15 |
| | | | [0.08971] | | [0.36761] | | [0.14424] | | [0.25139] | |
| Quartile 3 | 42202 | 44 | -0.08035 | 0.47 | -0.00675 | 1.39 | -0.03298 | 12.46 | 0.14564 | 12.22 |
| | | | [0.08345] | | [0.35045] | | [0.15294] | | [0.23992] | |
| Quartile 4 (highest) | 42201 | 52 | -0.21276* | 0.57 | -0.14013 | 1.20 | 0.08645 | 12.54 | 0.74515 | 12.30 |
| | | | [0.12551] | | [0.48026] | | [0.11631] | | [0.55912] | |

Note –Robust standard errors in brackets, clustered by officiator. * significant at 10%; ** significant at 5%; *** significant at 1% Subsamples based on predicted propensity to serve are based on regressions of service on test day and pre-test day controls. Baseline fixed effects are included but not used to calculate predicted propensity. All specifications include the baseline set of fixed effects (county x year and test month x office) and present the effect of service on the outcome found when using the leave out annual mean as the instrument. Income_30_34A and B differ in how individuals with missing observations are treated; for the latter, missing observations are assigned a log income of 0.

*Table 10. The Dynamics of the Effects of Service on Unemployment and Income, by Age and Subsample*

| | Dependent Variable: log income During Age X (0 if missing income) | | | | Dependent Variable: Any Unemployment During Age X | | | |
|---|---|---|---|---|---|---|---|---|
| Age | Criminal History = 1 | Criminal History = 0 | low education fathers | high education fathers | Criminal History = 1 | Criminal History = 0 | low education fathers | high education fathers |
| 23 | -0.12328 | 0.32058** | 0.11725 | 0.68813** | -0.14382 | -0.02532 | -0.00651 | -0.05985 |
| 24 | -1.42320** | 0.07791 | -0.07256 | 0.16491 | -0.13757 | -0.04833 | -0.00687 | -0.12280*** |
| 25 | -1.05357* | 0.06499 | -0.20608 | 0.3206 | -0.09897 | -0.04575 | 0.02118 | -0.10787** |
| 26 | -1.51587** | 0.31789 | 0.34274 | 0.20894 | 0.04879 | -0.0194 | 0.10618* | -0.05670* |
| 27 | -1.44096*** | 0.38531 | -0.07086 | 0.47744 | -0.03717 | 0.07387*** | 0.08019* | 0.03823 |
| 28 | -1.30694** | 0.31205* | 0.41614 | 0.32366 | -0.07757 | 0.03831* | 0.09817** | -0.02929 |
| 29 | -1.73329*** | 0.27511 | 0.35198 | 0.07445 | -0.18058*** | 0.02393 | 0.07521* | -0.06463 |
| 30 | -1.34625** | 0.00592 | -0.23518 | -0.08503 | -0.04145 | 0.01477 | 0.05196 | -0.0268 |
| 31 | -2.20521** | 0.02156 | -0.50873 | -0.00278 | -0.00553 | 0.02586 | 0.03047 | 0.01031 |
| 32 | -1.98112** | 0.25866 | -0.25599 | 0.33054 | -0.02357 | 0.01907 | 0.09052** | -0.01525 |
| 33 | -1.97689** | 0.41474** | -0.65955** | 0.68062** | 0.03149 | -0.00674 | -0.01635 | -0.03005 |
| 34 | -1.42771 | 0.46366** | -0.27064 | 0.57039** | 0.08504 | -0.01247 | -0.00507 | -0.01842 |
| N | 22590 | 146215 | 43904 | 95072 | 22590 | 146215 | 43904 | 95072 |

Note – Presents estimates of the effect of service on income (columns (1)- (4)) and unemployment (columns (5) - (8)) at each age, instrumenting for service with the leave out annual mean service rate of the assigned officiator, by subsample. Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. To keep sizes consistent across ages, the income variable used assigns log income of zero to those with missing income.

*Table 11. Peer Effects in Crime*

| Dependent variable: | Any crime between ages 23 - 30 | | | Number of crimes between ages 23-30 | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Unit pre-service crime | 0.015 | -0.007 | 0.012 | 0.036 | -0.054 | -0.012 |
| | [0.0287] | [0.0292] | [0.0297] | [0.0463] | [0.0394] | [0.0406] |
| Own pre-service crime = 1 | 0.115*** | 0.089*** | 0.115*** | 0.454*** | 0.238*** | 0.454*** |
| | [0.0040] | [0.0122] | [0.0040] | [0.0273] | [0.0513] | [0.0274] |
| Father education <= 9 years | 0.001 | 0.001 | -0.001 | 0.011 | 0.010 | -0.042** |
| | [0.0023] | [0.0023] | [0.0063] | [0.0145] | [0.0146] | [0.0208] |
| Unit pre-service crime * Own pre-service crime = 1 | | 0.200** | | | 0.766*** | |
| | | [0.0950] | | | [0.2089] | |
| Unit pre-service crime * Father education <= 9 years | | | 0.018 | | | 0.190** |
| | | | [0.0495] | | | [0.0854] |
| Mean dependent variable | 0.093 | 0.093 | 0.093 | 0.213 | 0.213 | 0.213 |
| Mean unit crime | 0.123 | 0.123 | 0.123 | 0.262 | 0.262 | 0.262 |
| If pre-service crime = 1 / = 0 | | 0.131/0.122 | | | 0.284/0.259 | |
| If father education <= 9 years / > 9 years | | | 0.129/0.121 | | | 0.280/0.256 |
| Observations | 102,085 | 102,085 | 102,085 | 102,085 | 102,085 | 102,085 |

Robust standard errors in brackets are clustered on regiments; *** p<0.01, ** p<0.05, * p<0.1. This utilizes sample of individuals who tested from 1990-1996 (i.e. the IV sample) who served. All specifications include the full set of test day and pre-test day controls, birth month, birth year, municipality, regiment-, rank-, and test year fixed effects, and regiment-specific linear trends. Columns (3) and (7) exclude controls for father's years of schooling, which is one of our pre-test day controls, instead using the dummy for fathers having nine or less years of education. Note that unit pre-service crime is the leave out mean crime rate for each individual's unit, as defined by test year, rank, and regiment cells. Unit pre-service crime is defined using the appropriate measure of crime, i.e., >= 1 crime in columns (1)-(3) and the number of crimes in columns (4)-(6).