The Causal Effect of Military Conscription on Crime and the Labor Market*

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Abstract

We identify the causal effect of mandatory peacetime military conscription on crime and labor market outcomes of Swedish males born in the 1970s and 1980s, taking advantage of the exogenous variation in the chance of service resulting from randomly assigned draft board officiators. Service significantly increases post-service crime, especially for relatively disadvantaged males with pre-service criminal history or low socioeconomic status. Similarly heterogeneous results are seen for labor market outcomes. We also provide the first clean evidence that conscription contemporaneously incapacitates crime. Finally, we highlight the role that peer effects may play in explaining the unintended negative impacts of service.

Keywords: Conscription, Crime, Criminal Behavior, Draft, Military Conscription, Military Draft, Incapacitation, Labor Market, Unemployment.

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1. Introduction

Young men in more than 60 countries around the world still face the prospect of mandatory military conscription today.[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 labor market. It is thus not surprising that conscription remains a hotly debated topic; in fact, a number of European countries have recently abolished it (France, 1996; Italy, 2005; Sweden, 2010; and Germany, 2011), while others have had failed referendums (Austria and Switzerland in 2013). Yet, despite a growing body of academic literature, there is little consensus about the impact of this potentially life transforming event.

The current 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. To shed some light on the potential mechanisms through which conscription affects crime, we complement this analysis by applying the same research design to legitimate labor market outcomes, including education, income, and unemployment benefits.

Swedish 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 whether the draftee would be enlisted. It is this exogenous variation in the chance of serving (due to the assignment of a relatively higher service rate officiator) that we use to identify the causal effect of conscription on crime in an instrumental variable framework. In addition, for a subset of cohorts for whom we know the exact dates of service, we utilize a difference-in-difference matching design to identify the incapacitation effects of service.

[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.

There are a number of channels through which military conscription may affect both contemporaneous and future criminal behavior. Conscription may decrease contemporaneous crime by incapacitating young men, i.e. keeping them otherwise engaged and isolated from society. 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 the obedience and discipline training that one receives may decrease post-service 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 one's criminal tendencies (Grossman, 1995). Military conscription may also positively or negatively impact crime through its impact on education and the labor 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 labor market, and reduces future labor market opportunities. Finally, exposure to a new peer group may have either positive or negative effects on criminal behavior, depending on the relative characteristics of the new and old peer groups.

The existing research yields mixed results, with respect to both labor 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, such that by age 50, draftees are on par with

[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).

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 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. (2014) find large earnings losses for high ability men in Denmark. Using a regression discontinuity design to address the biases arising from the endogenous selection process in Sweden, 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 and Lindo and Stoecker, 2012), while Siminski et al (2016) do not find a comparable effect in Australia. Just two published papers consider peacetime conscription. 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 labor market outcomes; these effects are even larger for wartime draftees. Finally, for a subset of the 1964 Danish birth cohort, Albaek et al. (forthcoming) 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.[7]

[3] Likewise, Siminski's (2013) study of Australian Vietnam draftees finds a negative employment effect.
[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] Current work by Vincent Lyk-Jensen (2016) uses the Danish lottery on more recent and complete cohorts of Danish men to study the impact of peacetime service on crime and incarceration. Overall, she finds no crime inducing effects of service, but she does find an increase in prison among juvenile offenders. This finding sharply contrasts Albaek et al. (forthcoming) and questions the idea that military service can straighten out troubled youths. Vincent Lyk-Jensen (2016) also concludes that her findings support our own results concerning juvenile offenders and crime (presented here and in an earlier CEPR Discussion Paper No. 11110 (Hjalmarsson and Lindquist, 2016)).

What can explain these diverse findings? First, the effect of conscription may change over the lifecycle. Since crime peaks as a young adult, focusing on crime after age 40, as in Siminski et al (2013) and Galiani et al (2011), may skew the results. Second, the conscription 'experience' varies greatly across studies. While peacetime versus wartime conscription is the most obvious example, other differences may emerge as countries approach the end of their mandatory conscription regimes. Third, measured differences may arise due to differing identification strategies. Because of the 'selection' into military service, one cannot simply compare outcomes for those who do and do not serve. While the most convincing studies rely on random assignment to service (i.e. lotteries), alternative strategies need to be used in countries without a lottery. Several studies compare cohorts before and after the abolition of conscription, which can yield different results than the lottery design since: (i) the conscription experience likely differs when it is about to be abolished, (ii) it may include general equilibrium effects, and (iii) the average and marginal individuals 'treated' may not be comparable across studies. If conscription has heterogeneous effects, then it is not surprising if studies with different identification strategies find different effects.

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 percent of the population) matched onto various individual registers allows us to study crimes committed when the men are young adults (i.e. before age 30) and a comprehensive set of legitimate labor market outcomes. Methodologically, we apply a new research design to

[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. (2014) study the labor market outcomes of Danish men born 1976-1983. In an attempt to explain their labor market findings, they do actually run a regression with crime as an outcome. They report a zero effect of military service on non-vehicular crime for all men aged 26-35.

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 empirical analysis is conducted in two stages, which take advantage of (i) the ability to identify the officiators who reviewed the 1990 to 1996 draft board test results and placed the draftees into service and (ii) information about the exact dates of service for the 1997 to 2001 test cohorts. The first stage identifies the causal effect of service on post-service crime by using the leave out annual mean service rate of the assigned officiator to instrument for service, and deal with the 'selection' of conscripts into service based on their draft board test scores. We argue that the leave out annual mean instrument (i.e. the annual share of testees (excluding oneself) assigned to that officiator who serve) is both valid and relevant when conditioning on county by test year fixed effects. With regards to the former, we provide both anecdotal and empirical evidence of random assignment to officiators, including a falsification test for a sample who meets an officiator but is excluded from service for health reasons. With regards to the first stage, we find that assignment to an officiator whose other testees are 10% more likely to serve increases one's chance of service by almost 10%; similar first stage relationships are seen for heterogeneous subsamples (by criminal history and paternal education). In addition, while the service rate of the assigned officiator affects the chance of service, it does not affect the type of service, including the branch (e.g. army), rank, or assignment to a combat position.

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 5.7 percentage points or 50 percent. These results appear to be driven by those from disadvantaged backgrounds with respect to criminal history or father's education. This heterogeneous impact is also seen with respect to the labor market, including higher eduction, unemployment and, especially, income. Military service significantly increases post-service income for those at the upper end of the distribution, while individuals from disadvantaged backgrounds tend to be worse off in terms of income (though this is insignificant).

The second stage of the analysis matches each individual in the 1997 to 2001 test cohorts who serves in the military to one specific control individual who does not serve. Using the exact dates of service for the treated individual to construct the counterfactual time of incapacitation for the control group, we apply a difference-in-difference design to estimates the incapacitation effects of service. We find evidence of incapacitation, especially for those at low risk for crime.

Finally, we demonstrate that individuals with pre-service criminal histories and from low socioeconomic status households are likely to be concentrated together when conscripted, leading to potentially intense social interactions. We find a strong relationship between peer pre-service criminal history and an individual's post service crime, for just those individuals from disadvantaged backgrounds. As such, negative peer interactions appear to be one feasible explanation for the unintended negative consequences of military service. The fact that we find an immediate and persistent effect of service on crime, but little immediate effect on income and mixed effects on unemployment for the disadvantaged subsamples makes a labor market mechanism unlikely to be the whole story.

The remainder of the paper proceeds as follows. Section 2 provides institutional details about Swedish military conscription and an overview of the data. Section 3 presents the officiator-based instrumental variable strategy used to identify the post-service effect of conscription, while Section 4 presents the corresponding results. Section 5 presents the difference-in-difference matching framework to isolate incapacitation. Section 6 discusses potential mechanisms that may explain the large effects of service on crime, highlighting the possibility of negative peer effects. Section 7 concludes.

2. Mandatory Conscription in Sweden

2.1. A Brief History

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, the results of which determined whether one would be conscripted and the assigned unit and rank. 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.

Though peacetime conscription was officially suspended on July 1, 2010, the number of men placed into military service actually started decreasing upon the end of the Cold War and accelerating after the fall of the Berlin Wall in 1989. This decrease was formalized in the Defense Proposition of 1992. Figure 1 shows the share of men born in Sweden (by birth cohort) who were called and tested by *Värnpliktsverket* (The Swedish Conscription Authority), as well as the share deemed fit to serve and placed in service categories and 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. At that time, noncompliance was punishable by jail, which was rigorously enforced.

In 1995, *Värnpliktsverket* and *Vapenfristyrelsen* (The Civil Conscription Committee) were merged into a single conscription authority called *Pliktverket*. Between 1995 and 2007, the number of young males called and tested by *Pliktverket* fell by 10 percentage points, due to a more thorough pre-test screening process for pre-existing mental and physical health problems.[9] *Pliktverket* began using an online tool in 2007 to further pre-screen potential conscripts, so that only a small share of the most suitable, willing and able were called for testing and placed in service. On July 1, 2010, Sweden adopted an all voluntary military service,

[9] On July 1, 1995 a new law stated that those clearly unable to participate in military or civil service should not be called to the testing days (*lagen om totalförsvarsplikt* 1994:1809).

though male citizens must still register with the official recruitment office (*Rekryteringsmyndigheten*) at 18. Despite being one of the most significant policy changes in recent Swedish history, its consequences have not yet been thoroughly evaluated.

2.2. The Testing Process, the Test Office, and the Role of the Test Officiator

This paper focuses on men drafted between 1990 and 2001. 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.[10] 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 an information meeting, at which conscripts were provided procedural information about the testing process and informed of their rights and obligations. The conscripts then took part in (i) a set of written tests measuring, verbal, spatial, logical and technical ability, (ii) a telegraph test, and (iii) medical and physical tests, examining their 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. The results from each test were 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 test officiator (*mönstringsförättare*) who examined their test scores and determined whether a conscript would be exempted from service due to mental and/or physical health problems. Such exemptions were based on a pre-determined set of health

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

criterion and were not discretionary. A conscript's health scores were determined by the office's doctors *before* meeting the officiator; the officiators were uninvolved in this process. Non-exempted testees (the vast majority) were then assigned a specific service category and preliminary start of service date by the officiator; all non-exempted conscripts left with a specific assignment and service commitment that they were expected to fulfill.[11]

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 had a well-defined job description and correspondingly well-defined tasks and ranks.[12] Each service category was also associated with a set of minimum tests scores needed for assignment to that category. For example, a specific officer position might require a conscript to receive at least a 7 (on a 1-9 stanine score) on all of the major test categories. Officiators were constrained by these test score requirements when assigning service categories.[13]

Test officiators used computers to help make the first match between a candidate's scores and a set of suitable service categories. The computer would present the officiator with a set of potential service categories that was created as the intersection of the troop order that needed to be filled in that test office and the conscript's test scores. Higher scoring draftees were presented more options. A typical interview lasted about 30 minutes, during which time the officiator and conscript typically discussed 4 to 5 potential service categories. Lower scoring

[11] 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.

[12] 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).

[13] Officiators were only allowed to deviate from these 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. These exceptions were rare at this time, since there was no shortage of qualified people to fill all positions.

draftees tended to have somewhat shorter interviews, and discuss fewer service categories. The officiator decided the exact service category assignment after interviewing the conscript.[14]

During the downsizing of the Swedish armed forces, nearly all service categories were oversubscribed. More people were required by law to serve than the military could place into service. 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 the most suitable persons *within* each service category were chosen to serve (SOU 2000:21 Bilaga 3 and SOU 2004:5). Thus, qualified persons placed into higher ranking and more skilled categories were not always called to serve, while someone with lower qualifications and tests scores might serve since they were assigned to a service category with different needs. Ranking of the "most suitable" candidates was not strictly based on tests scores; willingness to serve, a conscripts preferences for when and where to serve, and *the test officiators personal, subject judgment* (written down in the conscripts case file as brief notes) all played into this decision.

Importantly, since the military was continually downsizing during this period, it was the officiators' responsibility to decide which individuals within each service category would be called to serve. On the margin, this left room for the officiators to use discretionary judgement when determining the most suitable candidates. Officiators discussed these decisions both within and between test offices regularly (typically four times a year). Thus, the officiator affected the probability of service of the marginal draftee by (i) assigning a service category in relatively high demand and (ii) advocating his or her favorite candidates for the position.[15]

[14] All officiators interviewed stressed the importance of placing the right man into the right position. While a clear set of rules prevented the placement of low score recruits into high positions, officiators also tried to not place high score 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, however, were rare at this time, since there was no shortage of men with the correct qualifications and scores to fill each position.

[15] All test officers had been (or still were) officers in the military. Most were men. Towards the end of this time period, there were also a number of women but we do not know the specific identities of the test officiators.

Our empirical strategy also relies on the random assignment of draftees to officiators. We argue this to be the case 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 and that which was added at each station (information is also entered into the computer). The conscript arrived in a waiting room outside the officiators' offices (there are 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 the test office is determined by geographical location, this match is as good as random conditional on test year and county fixed effects.

The test officiators themselves, as well as many Swedish conscripts that we have talked to, insist upon this random match.[16] 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. 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 3.3 provides empirical evidence of this random matching.

2.3. Data Description

We study men born in Sweden between 1968 and 1983 who take the enlistment tests from 1990 to 2001. 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. This data have been matched to data from The Swedish Military Archives (*Krigsarkivet*), The

[16] When interviewing draft officiators, we asked them how each draftee was assigned to his test officiator. The only answer we ever received was that it was as good as random (in Swedish, *slumpmässigt*), since all offices used a simple first-come first-serve que system to assign draftees to officiators.

Swedish Military Recruitment Office (*Rekryteringsmyndigheten*), The Official Convictions Register (*belastningsregitsret*), and various register data from Statistics Sweden using each individual's unique personal identification number.

We have draft board data from The Swedish Military Archives and The Swedish Military Recruitment Office, though the former was only used to characterize historical trends in testing and service (see Figure 1). Our main analysis, which capitalizes on the identifier of the test officiator, focuses on the 1990 to 1996 test cohorts since almost everyone tested before 1990 served (i.e., there is little room for officiator discretion) and the officiator id is missing from the data for the 1997 to 2001 test cohorts.[17]

The draft board data cannot be used to assign treatment status, since the data are incomplete when it comes to identifying who actually served in the military. Thus, to identify treatment status we turn to the national tax registers. Every conscript who served for at least two months received a small taxable income from the government, which is specially marked in the tax register on an annual basis. Since we see that a payment was received during the year but not when it was received, we can only use this tax data to identify individuals who were enlisted but not exact dates of enlistment. We do, however, have exact service dates for those who tested from 1997 to 2001, which we use to analyze incapacitation (in a non-IV framework).

Finally, a number of additional variables are used from the draft board data, primarily for descriptive purposes, tests of identification, or robustness checks. For the 1990 to 1996 cohorts, we use the test date, test office, 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 whether the person was assigned a service category. Health and physical capacity scores are both summary measures based on a series of underlying tests.

[17] 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.

Our data were matched with the official crime register (*belastningsregistret*) for Sweden by the National Council for Crime Prevention (BRÅ), providing a full record of criminal convictions from 1973 to 2012. As is typical with administrative crime data, we cannot directly observe criminal behavior, and rather, use convictions as a proxy for criminality. For each conviction, we observe the type of crime and date the offense was committed. We study overall crime, *Any Crime*, and six specific crime 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 set of variables to investigate and alleviate concerns that the intensive margin results are driven by a few individuals with an extremely large number of convictions. Since we know when each crime was committed, we create crime categories based on age and classified as pre-service (ages 15-17), during service, and post-service (ages 23-30). The non-crime outcome variables are described when presenting the results.

We also use a number of background and control variables from register data held by Statistic's Sweden. We make use of *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, and create measures of mother's and father's education and income to ascertain the socioeconomic background of our draftees.[18]

3. Identifying a Post-Service Effect

The first part of the empirical analysis utilizes the 1990 to 1996 test cohorts, for whom we can identify the officiators who reviewed the draft board test results and made service decisions. We identify the causal effect of service on post-service crime using an instrumental variable

[18] 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.

design that capitalizes on the random assignment of potential conscripts to officiators who assign more or less individuals to service.[19]

3.1. Officiator Assignment as Instrument for Military Service

The primary aim of this section is to identify the causal effect of conscription on post-service crime and labor market outcomes. 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, $Conscript_i$.

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

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 service as well as crime and labor market performance, Ordinary Least Squares (OLS) estimation of equation (1) will yield biased estimates of the effect of conscription.

Thus, we propose to instrument for *Conscript* with the leave out annual mean service rate of the officiator to which the individual is assigned; 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.[20] 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). Thus, our instrument utilizes the exogenous variation in the chance of service given the officiator one is assigned in a given year from the pool of potential officiators in that year. Importantly, we use the leave out *annual*

[19] The design is similar to using randomly assigned judges (Kling, 2006; Aizer and Doyle, 2015; Mueller-Smith, 2015) or investigators (Doyle, 2008) as exogenous sources of variation for sentences and foster care, respectively.
[20] We create the leave out annual mean service rate based on our baseline sample of all non-immigrant males who tested each year, and not the final analysis sample.

mean rather than simply the leave out mean for the officiator because of the downward trend in the number of service positions and not all officiators are observed in the same years.

To isolate the post-conscription effect of service from incapacitation (and be certain that outcomes are observed after service is completed), we (i) define our primary outcome variables by age, emphasizing crime between ages 23 and 30 and labor market outcomes between ages 23 and 34, and (ii) restrict the sample to individuals who completed service before age 23.

3.2. Sample Creation and Descriptive Statistics

Our baseline data set consists of 231,583 non-immigrant males born from 1964 to 1990, who tested from 1990 to 1996, i.e. when the officiator variable is available. 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 'never' serve in a given year or missing health group information (88 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 service (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 168,806 non-immigrant males tested between 1990 and 1996.[21]

Our data contain 67 officiators in the six primary test offices in Sweden. In any given year, the number of officiators observed is between 25 and 29 (except 1993 when we observe 37 officiators). The average number of officiators in each test office and year is about 10, since some officiators rotate across test offices in a given year. In fact, just 42 percent of officiators

[21] 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.

are stationed in a single office each year; 19 percent in two, 17 percent in three, and the remaining in four or more. 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 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.

Table 1 provides summary statistics– overall and by service. During this period, 75 percent of the sample that was eligible to serve actually served. Table 1 also provides preliminary evidence of the relevance of our instrument: despite coming from comparable birth and test 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).

The middle panels of Table 1 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 capacity test scores.[22] Having a criminal history does not disqualify one from service; 13 percent 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 percent of the sample is convicted of at least one crime between ages 23 and 30. The average number of convictions (including zeroes) is 0.31, four percent of the sample has two or more convictions, and one percent has five or more convictions. At both the extensive and intensive margins (not shown), the largest crime category is traffic offenses (six percent of the sample). The other crime categories have much lower conviction rates: one percent for weapons, two

[22] 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.

percent for violent, one percent for theft, two percent for drugs, and two percent for other offenses. For almost every crime outcome, the 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.

3.3. Instrument Relevance and Validity

A good instrument is one that is relevant and valid, and for which the assumption of monotonicity holds. We argue that the proposed instrument meets all three criteria.

With regards to validity, recall from Section 2 that testees are randomly assigned an officiator after completing their battery of tests. This implies that both observable and unobservable testee characteristics should be uncorrelated with officiator characteristics, including the rate at which their assigned testees serve. However, since officiator characteristics may vary *across* test offices or years in a way that is correlated with testee characteristics, we argue that conditional on when and where the individual was assigned to take the test, the assigned officiator is as good as random. We use county fixed effects to control for the fact that test office assignment is based on where one lives.[23] We believe that county fixed effects to be more appropriate than test office fixed effects themselves since (i) individuals from the same region (and birth month groups) were often bussed to the test offices together and (ii) it was common for the same officiators to be in multiple test offices. However, we demonstrate that our results are completely robust to controlling for test office.

As a first test of random assignment, Table 2 presents the raw sample means of the test-day and pre-test day controls for individuals with low, middle, and top tertile leave out annual mean service rates; the average leave out annual mean service rate in each of these tertiles is 0.53, 0.63, and 0.78 respectively. The percent difference between the middle and top tertiles in

[23] 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 county is sufficient to achieve conditional random assignment.

comparison to the bottom tertile is presented in columns (4) and (5). P-values of these raw differences are presented in columns (6) and (7), while p-values obtained from regressions with county, test year, and county by test year fixed effects are in columns (8) and (9). In terms of observable characteristics, individuals in the middle tertile are generally not significantly different than individuals in the bottom tertile, even in the raw data. However, for eight of the 20 observable characteristics, individuals in the top tertile significantly differ at the 5% level from those in the bottom (though the direction of these raw differences is not always consistent in sign; individuals in the top tertile, for instance, are less likely to be convicted of a traffic offense but more likely to be convicted of an other offense). When conditioning on county by test year fixed effects, however, just two of these twenty controls are significantly different at the 5% level for the top versus bottom tertiles. In addition, an F-test of the joint significance of all of these controls gives p-values of 0.1129 and 0.1889, respectively, in regressions (not shown) of the continuous instrument and dichotomous instruments on the controls and county by test year fixed effects; p-values are 0.0000 when excluding all fixed effects.

We thus argue that officiator assignment is random when conditioning on county by test year fixed effects, and define our baseline instrumental variable specification accordingly. Of course, we cannot rule out non-random assignment based on unobservable characteristics, but, together with the anecdotal evidence of random assignment from officiator interviews, we believe this makes a strong case for the validity of the instrument. We provide further evidence of random assignment throughout the paper – namely that the baseline first stage and instrumental variable estimates are not sensitive to a large set of controls. We also use the sample of individuals ineligible for service because of health reasons to conduct a falsification exercise; 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 random assignment fails – i.e. testees are assigned officiators according to characteristics related to future crime.

Table 3 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, including county, test year, and county by test year fixed effects is presented in column (3). Being assigned an officiator whose other testees in that test year are 10% more likely to serve increases the chance of service by 7.2 percentage points, or 10% 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 7.9 percentage points. The F-statistics associated with our preferred specification are 79 and 32 for the continuous and dichotomous instruments, respectively, well above the weak instrument threshold. Column (4) demonstrates the stability of this first stage relationship to controls for (i) test office and office by test year fixed effects, (iii) pre-test day characteristics (including all crime specific history variables), and (iv) test day characteristics (including ability, physical capacity and psychological scores). This has little impact on the first stage effect and associated F-statistics.

Columns (5)-(8) of Table 3 present results in support of the monotonicity assumption. In the current context, this implies that officiators do not dramatically change their behavior depending on the type of testee they meet; that is, all testees must be at least weakly more likely to serve when facing a high service rate officiator.[24] Specifically, we re-estimate the first stage relationship by subsamples: criminal versus no criminal history and low versus high education fathers. Regardless of subsample, assignment to officiators with a 10% higher service rate (excluding oneself) significantly increases the chance of service by at least 9% (relative to the subsample mean service rate): 10.3% and 9.6% for those with and without a criminal history,

[24] See Mueller-Smith (2015) for an in depth discussion of the monotonicity assumption in the context of the judge fixed effects identification strategy.

respectively, and 10.5% and 9.1% for those with low and high education fathers. The estimates for the high and low background subsamples do not significantly differ from each other.

Finally, we investigate whether the service rate of the assigned officiator affects more than whether an individual simply serves or not. Does it also affect the type of service? Our ability to interpret the IV estimates as the causal effect of military service relies on the assumption that it is only service that is affected. The results in Table 4 clearly support this assumption. The service rate of the assigned officiator does not significantly affect the rank of service (private, corporal, or sergeant/2nd lieutenant), whether in a combatant position, or whether in the army or air force. Note that rank can also be a proxy for length of service, as higher ranks serve more months. Compared to F-statistics of 79 and 32 (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 three. This implies that our officiator instruments explain very little of the variation in the placement of testees into alternative service categories.

We do, however, see a statistically significant relationship between our continuous instrument (the leave out annual mean) and assignment to a positon in the navy (see Column (3) in Panel A1 in Table 4). This association, however, is a purely mechanical one. 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 service categories (71% and 79% served, respectively. This implies that the caseload handled by an officiator (e.g. randomly filling more navy positions than army positions) may result in a randomly higher service rate among those 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 not related to the officiator's own propensity to place conscripts in service.

In Panels A2 and B2 in Table 4, we remove this mechanical association by controlling for the share of testees that each officiator *j* assigns to each broad service category *X* (listed at the top of the column) in each year *t*. In Column (2) of Panel A2, for example, we control for the share of testees that officiator *j* assigned to the army in year *t*. Note that for the dichotomous instrument in panel B2, the controls are also dichotomized and equal to one if officiator *j* in year *t* has placed a larger share of his testees in category *X* than the national average placed in category *X* in that year. These controls push all of the associations to zero, including the one for navy in Column (3).[25] While including these controls affects the relationship between the leave out annual mean and the assigned service categories, it is important to note that including all of these shares as controls (in Column (1) of Panels A2 and B2 Table 4) has virtually no effect on the magnitude and significance of our baseline first stage relationship.

In addition, robustness checks indicate that excluding individuals assigned to the navy does not affect the baseline results. This gives further support to our claim that our instrument has an impact on outcomes by inducing military service (in general) and not by inducing service in a specific service category (e.g. navy). Though we present the baseline reduced form results in the next section, the remainder of the paper emphasizes the instrumental variable analysis.

4. High Service Rate Officiator Instrumental Variable Results

4.1. Baseline Results and Robustness Checks

Table 5 looks at the relationship between military service and overall 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). OLS estimates in Columns (1) and (2) find a negative correlation between service and overall crime, regardless of how crime is measured. When including county by test year 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,

[25] Including all shares for all categories makes no difference.

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 test office fixed effects and the full set of pre-test and test day characteristics in column (2) 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. Our baseline specification, with just county by test year fixed effects, is presented in column (3). 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% (.004/.10) and the number of convictions by 12.9% (.040/.31). Controlling for the full set of observable controls in column (4) has little impact on these point estimates, but increases the precision with which they are measured. Though effects of similar magnitudes are seen for convictions of two or more and five or more crimes, these estimates are not significant.

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 3 (approximately 0.72 without controls and 0.69 with controls) to identify the causal effect of service on crime. All instrumental variable specifications cluster the standard errors on the officiator. 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 by 5.7 percentage points and, on average, leads to an increase of 0.55 convictions. Relative to the mean, these effects are quite large – an increase of about 57 percent at the extensive margin and more than 100 percent at the intensive margin. Using the dichotomous intensive margin variables, we see comparatively large, though generally insignificant, 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.

Our baseline specification clusters on officiator. However, one could argue that it is more appropriate to cluster in other dimensions. Appendix Table 1 demonstrates the robustness of our results to clustering the standard errors on county (column 2), both officiator and test year (column 3), and officiator by test year (column 4).

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 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 (10% level) increases the likelihood of at least one weapons conviction by 1.1 percentage point, a violent conviction by 1.9 percentage points, and a traffic conviction by 3.5 percentage points. Relative to the dependent variable means, these point estimates (as well as those for the other offense categories) are quite large: for instance, service increases the chance of a violent conviction by 83% and traffic conviction by 55%. At the intensive margin, service significantly increases the number of convictions for all crime categories except the other category: weapons by 0.033, violent by 0.064, traffic by 0.19, theft by 0.11, and drugs and alcohol by 0.12. Service also significantly increases the chance of two or more convictions for theft, drugs and alcohol offenses, other and weapons. Controlling for the full set of controls in row (b) of Table 6 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 that testees are randomly assigned to officiators.

Figure 3 considers whether these results are driven by the first years immediately after service or are persistent in nature. Specifically, we re-estimate our baseline specification using crime measured in two-year age intervals: ages 23-24, 25-26,…, 33-34. Panels A and B of Figure 3 present the point estimates and 95% confidence interval for the extensive and intensive margins, respectively. 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 the intensive and extensive margins for ages 23 and 24. Second, the effect of service on crime is persistent; the effects seen at ages 23 and 24 are also seen until age 34 at the extensive margin and age 30 at the intensive 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 considers two dimensions of the testee's pre-test background: (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). For the most part, the results are driven by individuals from disadvantaged backgrounds – with at least one pre-service conviction or with low education fathers (nine or less years of schooling). For those with a criminal history, serving in the military increases the overall chance of conviction by 46% (not significant), and has large and positive effects for all crime categories, though only weapons and theft are marginally significant. For the low education father sample, service significantly increases the chance of a weapon, violent, and drugs and alcohol conviction, though the overall effect on the chance of conviction is not significant. In contrast, for those with no criminal history prior to service, none of the coefficients are significant and four are actually negative. For the high educated father sample, there is a marginally significant positive effect of service on overall crime, but again, none of the crime specific categories are significant, and drugs and alcohol is negative.

4.3. Falsification Test: Additional Test of Random Assignment

This section presents a falsification test, which can be interpreted as an alternative test of random assignment. For our main analysis sample, Panel A of Table 8 presents the reduced form relationship between having any convictions between ages 23 and 30 and the leave out annual mean service rate; odd numbered columns include the baseline set of fixed effects while even numbered columns include all controls. Consistent with earlier reduced form results, the service rate of the assigned officiator (measured continuously in columns (1) and (2) or dichotomously in columns (3) and (4)) for the analysis sample is positively associated with the chance of a post-service conviction (at the 5 or 10% level). Panel B presents the same specifications for a sub-sample of individuals who were assigned officiators but did not serve for health reasons; these individuals were assigned health category Y – no individuals in this category served. For these individuals, the service rate of the officiator could only affect crime from ages 23 to 30 if: (i) the short meeting with the officiator actually had a direct effect on the testee (which seems rather unlikely) or (ii) the testees were not randomly assigned to the officiator to start with. In the latter case, if individuals were non-randomly assigned officiators on the basis of characteristics correlated with future crime, then one could see a significant reduced form relationship. However, as seen in Panel B of Table 8, the service rate of the assigned officiator is not significantly associated with future crime for these individuals who are ineligible for service, providing additional support of officiator random assignment.

4.4. Non-Crime Outcomes

Not only does service occur near the peak of the age-crime profile, but it is also at a time when young men are embarking on either higher education or entering the labor market. To this end, we create a set of non-crime outcome variables from Statistics Sweden registers and the tax registers that characterize our testees interactions with the labor market. From the latter, we have information on income and the use of unemployment benefits. 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; individuals who have no income reported during this period are excluded. One can think of this as a measure of permanent income, which aims to assess whether military service has a long-lasting effect on income.[26] *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; we also consider whether the individual received *any* unemployment benefits during this period.

Columns (1) and (6) of Table 9 present the baseline instrumental variable results for the whole sample for the labor 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

[26] 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 income potential is lower than that of low skilled workers.

the receipt of unemployment benefits (at the extensive and intensive margins). However, we do find that service significantly increases income by 12.7%.

The remaining columns of Table 9 decompose these effects into the criminal history versus no history and low versus high education father subsamples. With regards to education, we find a negative relationship between service and the likelihood of higher education for all subsamples, though the effect is especially large and significant for those with a criminal history while it is quite small and insignificant for those with no criminal history. For the father schooling subsamples, however, we only find a (marginally) significant reduction in education for the high father education sample. For income, we find that service significantly increases 'permanent' income for those from better backgrounds (no criminal history or high education fathers) while it decreases (albeit insignificantly) income for those from disadvantaged backgrounds. A similar pattern of heterogeneous effects is seen for the unemployment outcomes for the low and high father education samples. For the low father education sample, military service significantly increases the number of years receiving unemployment benefits while there is a significant reduction in benefit years for the high father education sample. The findings that military service has beneficial labor market effects 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 and argue that officer training improves leadership-specific human capital.[27]

To further understand the impact of service on the legitimate labor 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 from 23 to 34, separately. 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

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

again see that service significantly increases income; by age 27, this effect is established as permanent (seen each year until age 34) and significant. Though many of the point estimates are comparably large in the earlier ages, they are less precisely estimated; it is also worth noting that it is during these earlier ages that individuals (especially from the advantaged sample) may be enrolled in school, depressing income. For the disadvantaged sub-samples, there is little evidence of a consistent effect of service on income at the early ages, though one can clearly conclude that there does not appear to be a large negative effect on income earned in the years immediately after service. The effect becomes more persistently negative as disadvantaged individuals age, but remains noisy and insignificant. With regards to the dynamics of the unemployment relationship, we see that service decreases the chance of unemployment, especially between ages 23 and 26, for the high education sample, which could again reflect school enrollment. In contrast, for conscripts with low-education fathers, there is a positive (albeit marginally significant or insignificant) effect of service on unemployment; the magnitude of this effect is a bit smaller in the early 20s than the mid to late 20s. 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.

Taken together, these results suggest that peacetime conscription increases participation in the illegitimate labor market and does not significantly improve legitimate labor market outcomes for the most disadvantaged individuals. This contrasts the belief/hope that providing discipline to individuals already at risk for a life of crime will put them on a better path and that the human capital skills gained during conscription improve the labor market outcomes for those coming from a disadvantaged starting point.[28]

[28] This does not rule out the possibility that service improves other aspects of these disadvantaged individuals' lives. However, IV estimates using the leave out mean service rate also find no beneficial effect of service on welfare or sick days for these high risk individuals. In contrast, service significantly decreases sick days for those with no criminal history and high education fathers. Of course, one should recall that this sample is already positively selected on health status. It is also possible that other life outcomes that are less directly linked to the labor market, such as family outcomes, fertility, or community participation, are impacted in a positive way.

5. Incapacitation Analysis Using Exact Service Dates and Exact Crime Dates

Without exact dates of service for the 1990 to 1996 test cohorts, we cannot determine whether a crime (prior to age 23) occurred before, during, or after service, and hence cannot study incapacitation for this sample. However, the availability of dates of service for the 1997 to 2001 test cohorts allows us to cleanly estimate the potential incapacitation effect of military service using a difference-in-difference (DiD) strategy applied to a matched sample. Specifically, treated individuals (those who serve) are each matched to one specific control individual (who does not serve). Each control is assigned the exact same service dates as his treated counterpart, enabling us to construct the counterfactual time of incapacitation for the control group.

The matched sample is constructed as follows. For the sample of men who tested between 1997 and 2001, we keep only those who served in the military (48,199) and those who did not serve but were assigned to a service category (72,763). Of those who serve, we keep the 28,551 (59%) for whom we have exact service dates. During this time period, only health and physical aptitude categories A, B, D, E, F, and J actually served. Excluding those assigned some other health category reduces the potential control group from 72,763 to 36,399 individuals. We drop treatment group individuals with obvious errors in their service dates and those who serve for less than two or more than 24 months (the latter became professional military officers). We also drop those who serve at age 23 or older (as in our IV sample), which leaves 28,379 treated individuals who served in the military and 36,399 potential controls.

We then estimate a propensity score for military service using a logit model that includes: mother's and father's education and income, enrollment in a 2-year or 3-year high school program, verbal and general ability test scores, body mass index, physical capacity and health group, as well as test month by test year and test year by test office controls. We match exactly on birth year and municipality and use the estimated propensity score to conduct a 1 to

1 nearest neighbor matching (within each birth year x municipality cell) without replacement.[29] This produces a sample of 13,253 matched pairs (26,506 individuals) of treated and controls.[30]

In Figure 4, we plot the age crime profiles of the treated and control groups for aggregate crime at the extensive and intensive margin. Similar plots are shown for our six crime categories in Appendix Figures 1 and 2. Note that for the moment we are not making use of exact service dates; we are simply looking at age-crime profiles. The typical age during service is between 19 and 22. These ages are depicted by the two vertical lines in each graph.

As in our IV sample, Figure 4 shows that those who serve are clearly positively selected in terms of their pre-service criminal convictions. However, despite this difference in levels, the pre-service trends of the treated and control groups are quite similar, which is important given the pre-service parallel trends assumption in the DiD design. Importantly, the trends between the treated and controls widen at age 19 – the age when many young men begin military service. This widening may indicate a potential incapacitation effect. Note also that it is the drop in crime among those who serve that is mainly responsible for this widening.

The age-crime profiles presented in Figure 4 and Appendix Figures 1 and 2 for the 1997 to 2001 test cohorts also highlight two other important phenomena. First, identifying the incapacitation effect of conscription using a within individual analysis (i.e. comparing pre-service convictions to those during service) will likely be biased given the downward sloped age-crime profile. Likewise, comparing crime during service across treated and control individuals will yield biased estimates due to the selection into service. To deal with these two sources of bias, we apply a difference-in-difference framework (DiD) to our matched sample.

For individual i in group $g \in \{treated,\ control\}$ in period $t \in \{1 = pre\_service,$ $2 = service\}$, we estimate the following DiD regression equation:

[29] We impose a caliper of 0.05, which guarantees that we do not use poorly matched nearest neighbors. This caliper is fine enough to guarantee balance across the observed variables used to estimate the propensity score.
[30] The young men in our matched sample are slightly more positively selected than those in our IV sample.

$$(2) \qquad crime_{igt} = \alpha + \lambda period2_t + \delta treated_g * period2_t + \mu_i + \varepsilon_{igt},$$

where μ_i represents an individual fixed effect. The pre-service period is defined as the months immediately preceding the start of military service, and is equal in length to the length of the service period. For example, if a conscript serves 10 months in the military, then we search his criminal record for any crimes committed during the ten months immediately preceding his service period. Again, we assign this treated individual's control counterpart the exact same service and pre-service period, both equal to 10 months in this example. The incapacitation effect of military service on crime is given by $\hat{\delta}$. Figure 4 and Appendix Figures 1 and 2 provide evidence in favor of the parallel trends assumption that allows us to interpret $\hat{\delta}$ as causal.

Estimates of the incapacitation effect at the extensive margin are shown in Table 11 (Appendix Table 2 shows the intensive margin). The estimated incapacitation effect for overall crime is negative, leading to a 40% reduction in crime while in service. All young men are affected. But, perhaps surprisingly, the largest reductions (as a % of the mean) are seen among young men with no prior criminal convictions. Service appears to keep these men away from trouble. The effect on men with previous criminal convictions is, in fact, quite modest, with the exception of drug and alcohol related crimes.[31]

An alternative explanation of the incapacitation effect seen in Table 11 is that it 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 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

[31] This incapacitation effect also implies that we cannot use this framework to estimate a potential post-service effect, since the parallel trends assumption is broken by the incapacitation effect of military service.

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 the 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.

6. Discussion of Potential Mechanisms Including the Role of Peers

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 labor market, (iii) service generates negative peer effects, and (iv) desensitization to weapons and violence.

Our analysis provided evidence that the contemporaneous effect of service is, if anything, incapacitative: potential offenders, especially those with a low risk of crime, commit fewer crimes while in service. For the high risk group, incapacitation is only seen for drug and alcohol offenses. 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 3).

The second proposed channel is one in which service impacts crime *through* its affect on legitimate labor 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 labor market more quickly. We would argue, based on the path dynamics of crime and labor market outcomes, that this is unlikely to be the whole explanation. The observed crime effects were immediate and persistent. Yet, for the disadvantaged subsample, there was no immediate negative effect of service on income; any negative income effects do not appear until age 26 for the criminal history sample and the 30s for the low father education sample. However, we cannot completely rule out this channel as part of the explanation given that the criminal history sample is less likely to get higher education and some evidence of an (insignificant) increase in unemployment for the low education father sample in the years immediately after service (the opposite was in fact true for the criminal history sub-sample).

Alternatively, the harmful effects of service on the criminal activity of the 'disadvantaged' sample may be due to negative peer effects. Such a story would echo Grönqvist and Lindqvist's (2016) argument that positive effects on educational attainment of officer training may be due to peer effects. Young men who are assigned to the two officer ranks (sergeant and 2nd lieutenant) find themselves among a strongly positively selected group when they are actively choosing whether to pursue higher education. To study this 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.[32] Our units are created by grouping men by test year, regiment and rank. There are 114 regiments in our data and four ranks –

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

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. 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 percent of privates have low education fathers, compared to 19 and 12 percent for corporals and sergeants/2nd lieutenants, respectively. Thus, one unintentional side-effect of the recruitment and placement process is 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? In Table 12, 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. We control for birth month, birth year, municipality, and test year by test office fixed effects, as well as the full set of test day and pre-test day characteristics, including own pre-service crime.[33] 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).[34]

For men from advantaged backgrounds, exposure to peers with a criminal history does not increase post-service crime. In contrast, the results in Table 12 are indicative of strong peer

[33] Adding test year by rank, test year by regiment, and rank by regiment fixed effects does not change the main coefficients of interest, i.e. the interaction terms with unit pre-service crime.
[34] 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. Thus, these peer effects appear to reinforce the criminal path that individuals are already on.[35]

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 the peer interactions. In this way, peer effects of service could be quite strong even if the average peer characteristics are not that different.

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.

7. 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

[35] This is consistent with the findings of crime-specific reinforcing peer effects in juvenile correctional facilities by Bayer, Hjalmarsson, and Pozen (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, Sacerdote, and West (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 Airforce Academy.

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, the UK and Sweden (Bieri, 2015). 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. We provide evidence that grouping high crime, low SES individuals together in an environment with high intensity peer exposure may be one feasible explanation for the negative effects of service for these high risk populations – i.e. reinforcing peer effects. Unfortunately, regardless of the mechanism, these results contradict the idea that military service may straighten out troubled youths and build skills that are marketable in the post-service labor market.

On a brighter note, we find that mandatory conscription can have other positive post-service effects for populations at low-risk for crime. In the instrumental variable analysis, there is little post-service effect on crime and there are actually improved labor market outcomes for individuals from non-criminal backgrounds and better educated families. In addition, we provide the first empirical evidence that military service can incapacitate criminal behavior during the time of service, especially for this low-risk population.

Taken together, the results of our analysis indicate mandatory military conscription does have a significant impact on the life course of young men, and that this impact is quite heterogeneous, such that it may reinforce already existing inequalities in the likelihood of future success. These non-monetary costs (and/or benefits) should be taken into account when deciding whether to reinstate or abolish mandatory conscription or when devising the system through which conscription occurs (e.g. lottery, testing, etc.). Who are the average and marginal conscripts? How will conscription affect these individuals?

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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.

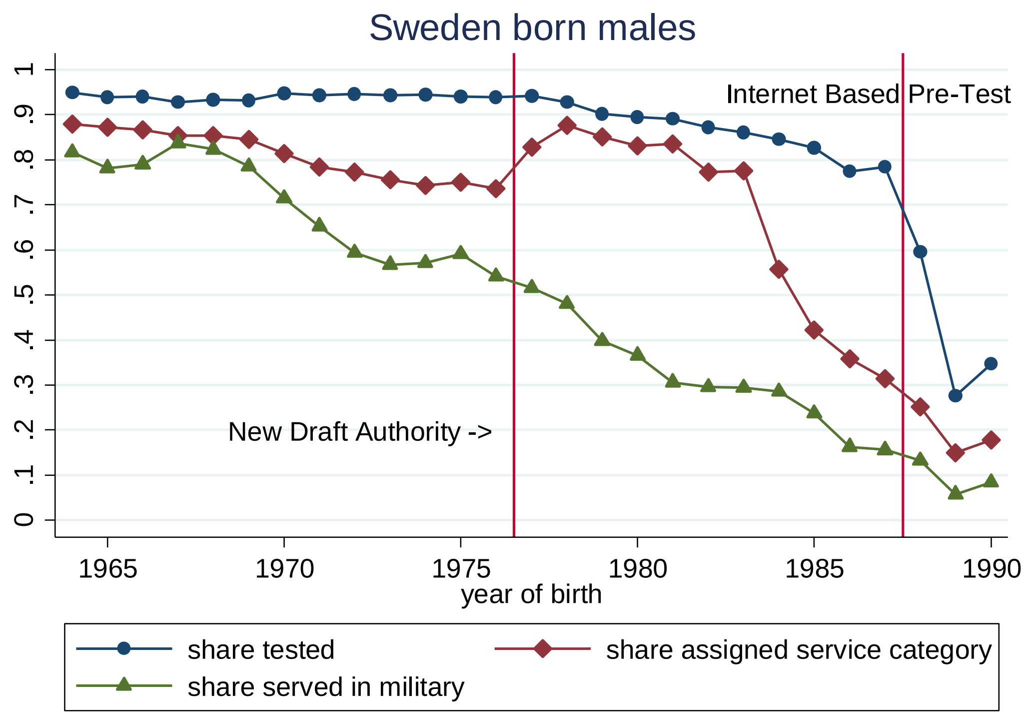
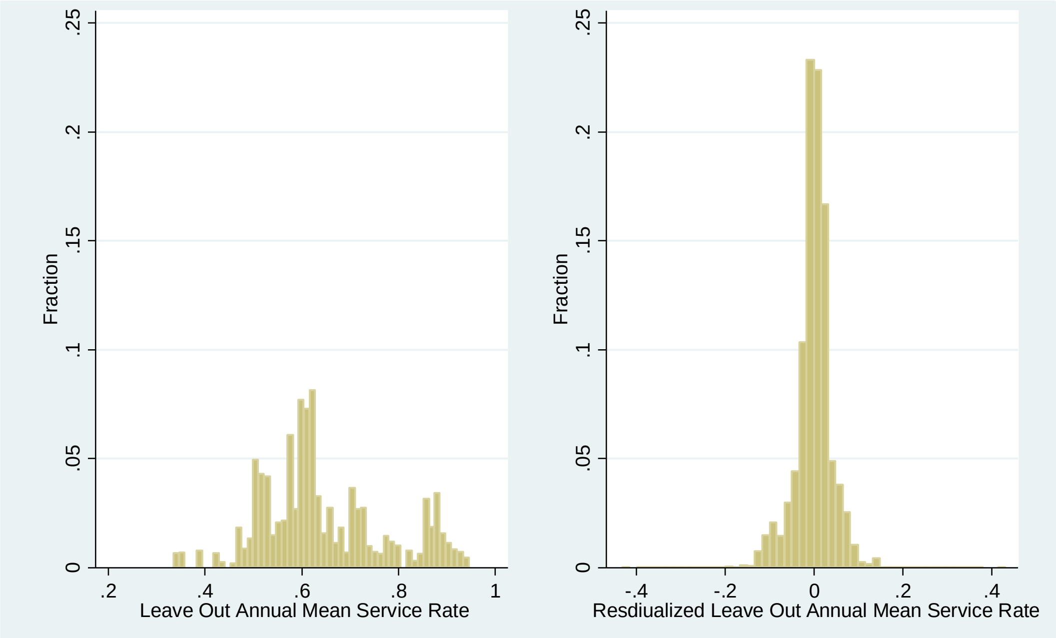
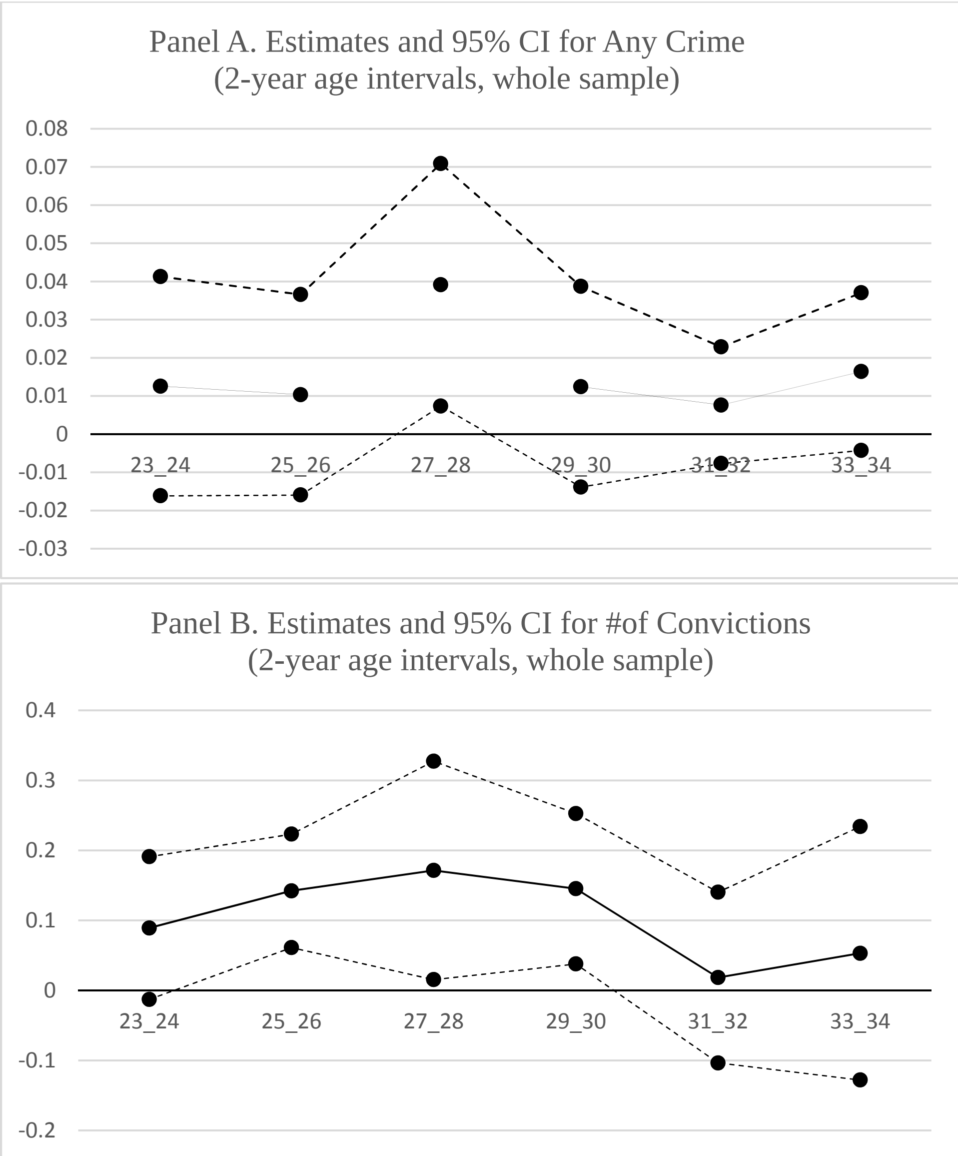


Figure 2. Variation in Officiator Annual Service Rate



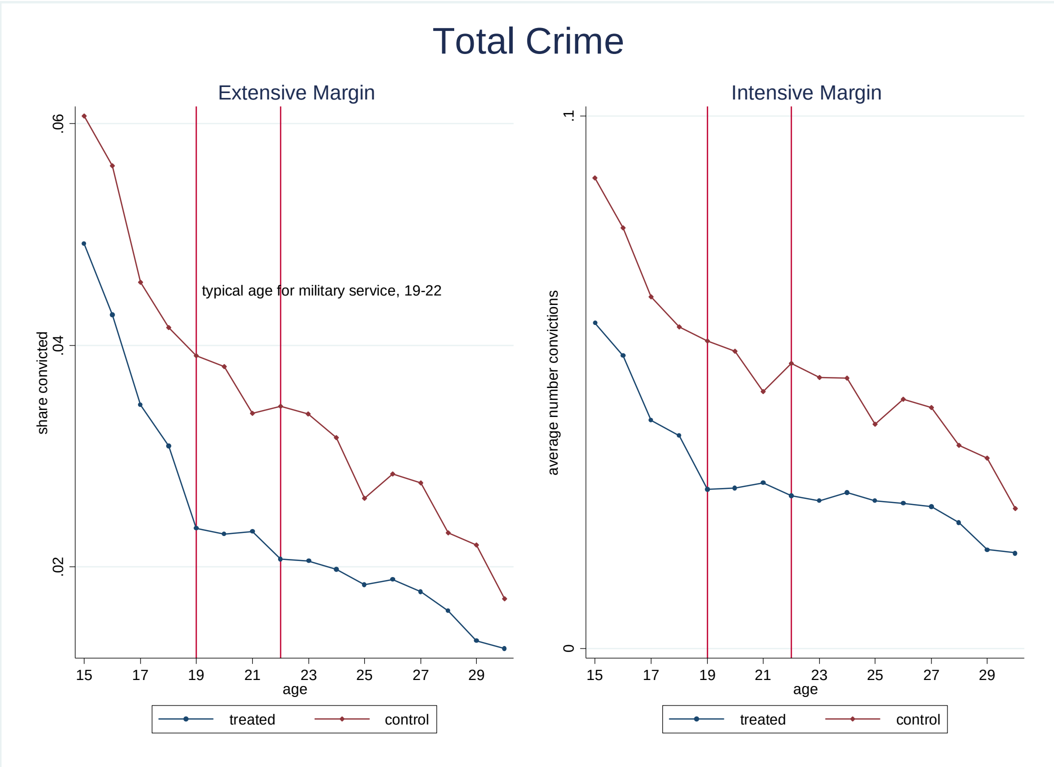
Note – The residualized leave out annual mean is from a regression on county x test year fixed effects.

Figure 3. Path Dynamics of the Effect of Service on Crime.



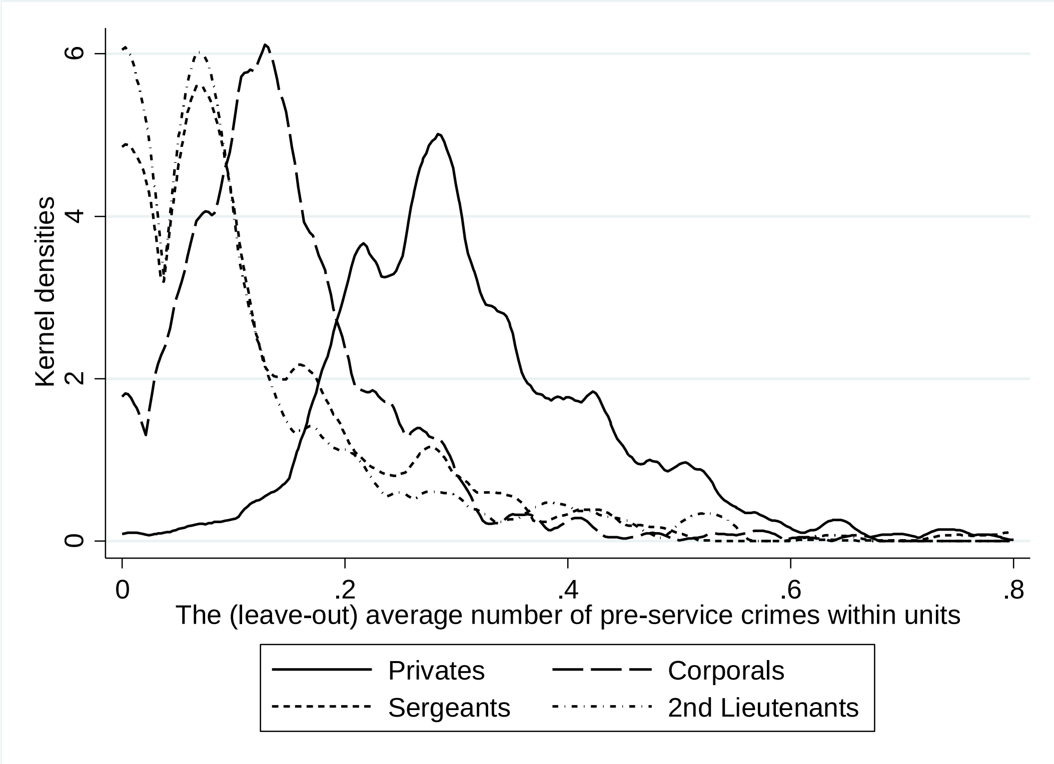
Note – The solid line represents the point estimates of effect of service on the chance of conviction (Panel A) and number of convictions (Panel B) for each two-year age interval.

Figure 4. Crime Trends by Age for Treated and Controls in our Matched Difference-in-Difference Sample, Extensive Margin.



Note – This graph is based on our matched DiD sample, which includes men tested between 1997 and 2001.

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 N = 168806		Service = 1 N=126540	Service = 0 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	12.22	0.52	12.23	12.20
income mother	11.75	0.49	11.76	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	12.41	0.72	12.42	12.36
Any unemployment benefits 23-34?	0.46	0.50	0.46	0.45

Note - missing observations are replaced with sample means (for the control variables only).

Table 2. Summary Statistics by Leave Out Annual Mean Service Rate Tertile: Test of Conditional Random Assignment

	Sample Mean by Tertile			% Difference (vs Bottom)		p-values:compare middle and top tertiles to bottom tertile: with no controls		with county x test year FE	
	Bottom	Middle	Top	Middle	Top	Middle	Top	Middle	Top
Variable	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
leave out annual mean	0.53	0.63	0.78	18.9%	48.0%	0.000	0.000	0.000	0.000
test_year	1992.8	1992.8	1992.7	0.0%	0.0%	0.985	0.958		
birth_year	1974.5	1974.6	1974.6	0.0%	0.0%	0.917	0.956	0.684	0.917
Any weapons < 18?	0.005	0.007	0.008	28.8%	50.9%	0.116	0.067	0.995	0.393
Any violent < 18?	0.017	0.020	0.019	18.5%	12.3%	0.042	0.342	0.421	0.473
Any traffic < 18?	0.058	0.058	0.050	-1.2%	-14.6%	0.709	0.000	0.027	0.118
Any theft < 18?	0.054	0.056	0.068	4.3%	27.0%	0.157	0.000	0.519	0.085
Any other < 18?	0.036	0.037	0.041	3.3%	14.7%	0.311	0.000	0.882	0.019
Any drugs< 18	0.003	0.004	0.003	11.4%	-2.6%	0.364	0.808	0.287	0.042
schooling father	10.92	11.10	11.40	1.6%	4.4%	0.083	0.000	0.965	0.511
schooling mother	11.37	11.52	11.76	1.3%	3.4%	0.091	0.000	0.306	0.406
income father	12.21	12.22	12.24	0.1%	0.2%	0.338	0.185	0.751	0.817
income mother	11.71	11.74	11.80	0.3%	0.8%	0.089	0.000	0.941	0.759
2 year school?	0.19	0.18	0.18	-2.2%	-3.5%	0.900	0.829	0.132	0.196
3 year school?	0.75	0.76	0.76	0.7%	1.1%	0.871	0.801	0.871	0.414
Height	179.79	179.73	179.72	0.0%	0.0%	0.498	0.402	0.367	0.409
weight	71.75	71.53	71.48	-0.3%	-0.4%	0.255	0.188	0.218	0.109
BMI	22.18	22.12	22.11	-0.2%	-0.3%	0.329	0.301	0.019	0.094
Ability Score	5.14	5.16	5.23	0.5%	1.7%	0.526	0.021	0.711	0.999
Physical Capacity Score	6.18	6.27	6.32	1.5%	2.3%	0.116	0.078	0.400	0.597
Psychological Test Score	5.25	5.42	5.39	3.3%	2.8%	0.003	0.033	0.819	0.084

This table compares the sample means for those with leave out annual means in the lowest, middle and top tertiles, i.e. with officiators with low, middle, and high service rates in the year tested. The thresholds for each tertile (33 and 66 percentile) are defined separately by year, such that the leave out annual mean is compared to the appropriate percentile in that year. For the bottom, middle, and top tertiles respectively, N = 58205, 55634 and 54967. Columns (1), (2), and (3) present the sample means. (Note that some variables, particularly parent schooling and income, are missing a little less than 20% of observations. Any regressions with these controls include dummies indicating missing values.) Columns (4) and (5) present the percentage difference of the middle and top tertiles compared to the bottom, respectively. Columns (6) and (7) present p-values of tests of whether the middle and top tertile means differ from the bottom; these are obtained from regressions of the 'variable' on dummies for the middle and top tertiles, clustering on officiator. Columns (8) and (9) 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.

Table 3. First Stage Regressions of Military Service on Leave Out Annual Mean Service Rate

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent variable = military service							
	Full Sample				Prior Criminal History	No History	Father <=9 yearsschool	Father >9 years school
Panel A: Continous Instrument: Leave Out Annual Mean								
leave_out_annual_mean	0.75145***	0.71279***	0.72065***	0.68964***	0.74885***	0.71727***	0.77392***	0.68594***
	[0.04818]	[0.06296]	[0.08110]	[0.07561]	[0.10938]	[0.08040]	[0.09771]	[0.09150]
F-Statistic	*243.28*	*128.16*	*78.96*	*83.2*	*46.87*	*79.58*	*62.74*	*56.2*
R-squared	0.05	0.06	0.07	0.1	0.07	0.07	0.08	0.07
Panel B: Dichotomous Instrument: Leave Out Annual Mean > Annual Mean service rate								
High Service Rate Officiator	0.15513***	0.10229***	0.07933***	0.07464***	0.10149***	0.07601***	0.09326***	0.07287***
	[0.01999]	[0.01166]	[0.01398]	[0.01348]	[0.01948]	[0.01389]	[0.01464]	[0.01387]
F-Statistic	*60.24*	*76.97*	*32.21*	*30.67*	*27.15*	*29.93*	*40.59*	*27.59*
R-squared	0.03	0.06	0.07	0.09	0.07	0.07	0.07	0.06
Observations	168806	168806	168806	168806	22590	146216	43904	95073
Mean Dependent Variable	0.75	0.75	0.75	0.75	0.73	0.75	0.73	0.76
county FE	no	yes	yes	yes	Yes	yes	yes	yes
test year FE	no	yes	yes	yes	Yes	yes	yes	yes
county x test year FE	no	no	yes	yes	Yes	yes	yes	yes
test office FE	no	no	no	yes	No	no	no	no
test office x test year FE	no	no	no	yes	No	no	no	no
pre-test day characteristics	no	no	no	yes	No	no	no	no
test day variables	no	no	no	yes	No	no	no	no

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

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

Dependent Variable:	Military service	Service category: army	navy	air force	private	corporal	sergeant or 2nd lieutenant	combatant
	(1)	(2)	(1)	(3)	(4)	(5)	(6)	(7)
Panel A1: Continuous Instrument: Leave Out Annual Mean								
Leave out annual mean	0.72065***	-0.14652	0.15667**	-0.03506	-0.1129	0.08633	0.02706	-0.13539*
	[0.08110]	[0.09067]	[0.05929]	[0.03796]	[0.08979]	[0.07031]	[0.04532]	[0.07560]
F-statistic	78.96	2.61	6.98	0.85	1.58	1.51	0.36	3.21
Panel A2: Continuous instrument with controls for share(s) assigned to service category X by officiator j in year t								
Leave out annual mean	0.69688***	0.03622	-0.04363	0.02306	-0.03455	0.03525	-0.00042	0.01303
	[0.07539]	[0.03215]	[0.02861]	[0.02013]	[0.04350]	[0.03425]	[0.01423]	[0.01659]
F-statistic	85.44	1.27	2.33	1.31	0.63	1.06	0.00	0.62
Panel B1: Dichotomous Instrument: Leave Out Annual Mean > Annual Mean service rate								
High service rate officiator	0.07933***	-0.00817	0.01068*	-0.00399	-0.00704	0.00944	-0.0023	-0.01415
	[0.01398]	[0.00920]	[0.00591]	[0.00746]	[0.01466]	[0.01060]	[0.00622]	[0.01024]
F-Statistic	32.21	0.79	3.27	0.29	0.23	0.79	0.14	1.91
Panel B2: Dichotomous instrument with controls for share(s) assigned to service category X by officiator j in year t								
High service rate officiator	0.08124***	-0.00431	-0.00094	0.00458	-0.00539	0.00556	0.00312	-0.00677
	[0.01480]	[0.00805]	[0.00384]	[0.00648]	[0.00818]	[0.00760]	[0.00328]	[0.00856]
F-Statistic	30.12	0.29	0.06	0.50	0.43	0.54	0.91	0.63
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
Observations	168806	165928	165928	165928	168806	168806	168806	152158

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

Table 5. OLS, Reduced Form, and IV 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)								
military_service	-0.01661***	-0.00471**	0.04113**	0.03521**	0.05707**	0.05066**	0.08908*	0.08528*
	[0.00277]	[0.00202]	[0.01849]	[0.01617]	[0.02613]	[0.02373]	[0.04779]	[0.04515]
Panel B: Dependent Variable = # Crimes from Age 23-30 (Mean = 0.31)								
military_service	-0.13289***	-0.04729***	0.39513***	0.37730***	0.54830***	0.54294***	0.55856**	0.56626**
	[0.01965]	[0.01647]	[0.11018]	[0.10218]	[0.18881]	[0.18249]	[0.25192]	[0.23392]
Panel C: Dependent Variable = # Crimes from Age 23-30 >=2 (Mean = 0.042)								
military_service	-0.01294***	-0.00434***	0.01717	0.01629	0.02383	0.02345	0.04028	0.04024
	[0.00151]	[0.00108]	[0.01378]	[0.01240]	[0.01973]	[0.01837]	[0.02713]	[0.02555]
Panel D: Dependent Variable = # Crimes from Age 23-30 >=5 (Mean = 0.013)								
military_service	-0.00610***	-0.00192**	0.01334*	0.01087	0.01851*	0.01565	0.02014	0.01833
	[0.00084]	[0.00074]	[0.00693]	[0.00659]	[0.01068]	[0.01023]	[0.01447]	[0.01406]
First Stage F-Statistic					78.96	87.13	32.21	31.95
County x Test Year FE	yes	yes	yes	yes	yes	yes	yes	yes
Test office Fixed Effects	no	yes	no	yes	no	yes	no	yes
Pre-test and Test day Controls	no	yes	no	yes	no	yes	no	yes

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. For the ease of presentation, just the coefficient on military service is reported. Columns (3) and (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, clusterd by county in columns (1) and (2) and officiator in columns (3) - (8). *** significant 1%, ** significant 5%, * significant 10%. N= 168806

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

	Dependent Variable:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Panel A: extensive margin 23-30							
(a) Baseline	0.05707**	0.01105*	0.01893*	0.03528*	0.00821	0.01309	0.01213
	[0.02613]	[0.00609]	[0.00991]	[0.01804]	[0.00892]	[0.01271]	[0.00947]
(b) +test office, pre testday and test day controls	0.05066**	0.01039*	0.01681*	0.02940*	0.00712	0.01373	0.01112
	[0.02373]	[0.00564]	[0.01012]	[0.01653]	[0.00905]	[0.01171]	[0.00750]
Mean Dependent Variable	*0.1*	*0.0056*	*0.023*	*0.064*	*0.013*	*0.025*	*0.016*
Panel B: # crimes 23-30							
(a) Baseline	0.54830***	0.03250**	0.06415**	0.18665**	0.10810**	0.0332	0.12371***
	[0.18881]	[0.01349]	[0.03124]	[0.09316]	[0.04855]	[0.03612]	[0.03845]
(b) +test office, pre testday and test day controls	0.54294***	0.02905**	0.05399*	0.19167**	0.10417**	0.03915	0.12491***
	[0.18249]	[0.01287]	[0.02968]	[0.09451]	[0.05133]	[0.03717]	[0.03388]
Mean Dependent Variable	*0.31*	*0.01*	*0.046*	*0.13*	*0.038*	*0.046*	*0.047*
Panel C: 2 or more crimes 23-30?							
(a) Baseline	0.02383	0.00687**	0.00452	0.0158	0.00749	0.01308*	0.01904**
	[0.01973]	[0.00293]	[0.00696]	[0.00975]	[0.00621]	[0.00750]	[0.00956]
(b) +test office, pre testday and test day controls	0.02345	0.00588**	0.00316	0.01578	0.00693	0.01330*	0.01854**
	[0.01837]	[0.00244]	[0.00704]	[0.00964]	[0.00602]	[0.00771]	[0.00851]
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 = 168,806. The baseline regressions control just for test year dummies, county dummies and test year x county dummies. 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 78.96 in the baseline (no controls) and 87.13 with the full set of controls.

Table 7. Heterogeneity: By Criminal History and Father Schooling

	Dependent Variable: At Least One Conviction from age 23-30 of						
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Panel A: Heterogeneity By Criminal History							
Sample: At least one crime < 18 (n = 22590)	0.11406	0.07386*	0.04106	0.10046	0.11596*	0.07369	0.08628
	[0.12719]	[0.04442]	[0.05118]	[0.09298]	[0.06150]	[0.06029]	[0.06037]
Mean Dep Variable	*0.24*	*0.023*	*0.072*	*0.15*	*0.048*	*0.071*	*0.06*
Sample: No crimes < 18 (n=146216)	0.03547	-0.00025	0.01086	0.01834	-0.01262	-0.00044	-0.0038
	[0.02212]	[0.00421]	[0.00947]	[0.01575]	[0.00793]	[0.00982]	[0.00814]
Mean Dep Variable	*0.082*	*0.0028*	*0.015*	*0.052*	*0.0076*	*0.017*	*0.0093*
Panel B: Heterogeneity by Father Schooling							
Sample: Father Schooling <= 9 years (n = 43904)	0.02653	0.02324**	0.04573*	0.02315	0.01523	0.00309	0.04520**
	[0.04295]	[0.01171]	[0.02595]	[0.03982]	[0.01834]	[0.02012]	[0.02094]
Mean Dep Variable	*0.12*	*0.0074*	*0.03*	*0.074*	*0.017*	*0.032*	*0.02*
Sample: Father Schooling > 9 years (n=95073)	0.06848*	0.00238	0.01555	0.03398	0.00786	0.01893	-0.00528
	[0.03641]	[0.00753]	[0.01410]	[0.02354]	[0.00912]	[0.01631]	[0.01559]
Mean Dep Variable	*0.096*	*0.0048*	*0.019*	*0.06*	*0.012*	*0.021*	*0.015*

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. F-statistic on first stage regressions: 46.87 for sample with criminal history, 79.58 for sample without criminal history, 62.74 for low education fathers and 56.2 for high education fathers. Each regression controls for test year dummies, county dummies and test year x county dummies.

Table 8. Falsification Test – Alternative Test of Random Assignment

	(1)	(2)	(3)	(4)
	Dependent Variable: Any Conviction Age 23-30?			
	Instrument = Leave Out Annual Mean Service Rate		Instrument = High Service Rate Officiator Dummy	
Panel A: Main Analysis Sample				
Instrument	0.04113**	0.03521**	0.00707*	0.00642*
	[0.01849]	[0.01617]	[0.00401]	[0.00358]
Mean Dependent variable	0.1	0.1	0.1	0.1
Observations	168806	168806	168806	168806
Panel B: Falsification Sample - Omitted Health Category Y				
Instrument	0.0713	-0.00317	0.0065	-0.00348
	[0.05757]	[0.03135]	[0.00883]	[0.00604]
Mean Dependent Variable	0.19	0.19	0.19	0.19
Observations	30809	30809	30809	30809
County x Test Year fixed effects	yes	yes	yes	yes
Test office Fixed Effects	no	yes	no	yes
Pre-test and Test day Controls	no	yes	no	yes

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	all	at least 1 pre service crime	no pre service crime	father schooling <= 9 years	father schooling > 9 years	All	at least 1 pre service crime	no pre service crime	father schooling <= 9 years	father schooling > 9 years
	Dep var = schooling > 12 years					*Dep Var = income_30_34*				
Military service	-0.04281	-0.18982**	-0.00094	-0.02337	-0.10433*	0.12691**	-0.15305	0.18173***	-0.07859	0.25940***
	[0.03820]	[0.07488]	[0.03705]	[0.04107]	[0.05963]	[0.05187]	[0.18045]	[0.04861]	[0.09791]	[0.05531]
Mean Dep Variable	0.42	0.22	0.45	0.26	0.5	12.41	12.28	12.43	12.37	12.41
Observations	168806	22590	146216	43904	95073	165297	21927	143370	43027	92699
	Dep Var = any years unemployment benefit 23-34					*Dep Var = # years unemployment benefit 23-34*				
Military service	-0.03847	-0.08462	-0.04047	0.06615	-0.07593	0.04471	-0.37996	0.07118	0.64869**	-0.40318**
	[0.04011]	[0.11262]	[0.03974]	[0.08478]	[0.05228]	[0.15338]	[0.47138]	[0.15222]	[0.31720]	[0.17584]
Mean Dep Variable	0.46	0.55	0.45	0.5	0.45	1.55	2.04	1.48	1.79	1.49
Observations	168806	22590	146216	43904	95073	168806	22590	146216	43904	95073

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. First stage f-stats: 78.96 for whole sample, 46.87 for preservice crime = 1, 79.58 for no pre service crime, 62.74 for father <=9 years school, 56.20 for father >9 years school. Each regression controls for test year dummies, county dummies and test year x county dummies, and instruments for military service with the leave out annual mean service rate of the assigned officiator.

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dep. Var = log income During Age X				Dep. Var = Any Unemployment During Age X			
	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	0.02828	0.15069*	0.13441	0.34927***	-0.10907	-0.00462	0.04761	-0.04272
24	0.23767	-0.02302	-0.06122	0.20599	-0.07347	-0.02033	0.03626	-0.07763**
25	0.09823	0.06694	0.1165	0.17276	-0.02602	-0.03509	0.04036	-0.07477**
26	-0.10037	0.14524	0.03014	0.21979**	0.01858	-0.02029	0.09373	-0.05660**
27	-0.18014	0.18864*	0.08458	0.21728	-0.05171	0.05228***	0.07586	0.00802
28	-0.40212**	0.23856***	0.10646	0.25098*	-0.06352	0.03364	0.08500*	-0.02835
29	0.01636	0.16223**	0.0687	0.30581***	-0.14530**	0.01404	0.07121*	-0.06937**
30	0.07948	0.09840*	0.04703	0.18686*	-0.03765	0.01249	0.06277*	-0.03557
31	-0.1609	0.16501***	-0.02481	0.17940**	0.00113	0.02437	0.03154	0.00574
32	-0.35957	0.19311***	-0.12697	0.27852***	-0.00951	0.02643**	0.08887***	-0.00258
33	-0.37301	0.28625***	0.02005	0.34812***	0.00907	-0.00212	0.0144	-0.02726
34	-0.15887	0.22288***	-0.17048**	0.42738***	0.08228	-0.00757	0.00906	-0.00776

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*. For unemployment, the sample sizes = 22590 for criminal history = 1, 146216 for criminal history = 0, 43904 for low education fathers, and 95073 for high education fathers. The income regressions have smaller samples (for all subsamples) since individuals with missing income are excluded. Missing income does not necessarily correspond to zero income.

Table 11. Difference-in-Difference Estimates of the Incapacitation Effect of Military Service Using Exact Service Dates and Exact Crime Dates, Extensive Margin.

	Dependent Variable:						
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Matched sample	-0.0081***	-0.0005	-0.0012	-0.0029**	-0.0010	-0.0003	-0.0039***
(N = 26,506)	[0.0024]	[0.0005]	[0.0011]	[0.0015]	[0.0010]	[0.0012]	[0.0010]
Mean	0.020	0.001	0.004	0.008	0.003	0.005	0.004
Matched sample with criminal history	-0.0063	-0.0024	0.0050	-0.0050	0.0008	0.0039	-0.0127**
(N = 2,820)	[0.0127]	[0.0035]	[0.0072]	[0.0077]	[0.0059]	[0.0068]	[0.0056]
Mean	0.068	0.007	0.018	0.020	0.013	0.018	0.015
Matched sample with no history	-0.0086***	-0.0002	-0.0020**	-0.0028**	-0.0013	-0.0008	-0.0029***
(N = 23,686)	[0.0022]	[0.0004]	[0.0009]	[0.0014]	[0.0009]	[0.0010]	[0.0009]
Mean	0.015	0.000	0.002	0.006	0.002	0.003	0.002
Matched sample father ed. <= 9 years	-0.0069	-0.0016	-0.0007	-0.0010	-0.0016	0.0003	-0.0046**
(N = 6,072)	(0.0053)	(0.0013)	(0.0029)	(0.0030)	(0.0024)	(0.0027)	(0.0018)
Mean	0.024	0.002	0.005	0.007	0.005	0.007	0.004
Matched sample father ed. > 9 years	-0.0085***	-0.0001	-0.0014	-0.0035**	-0.0008	-0.0005	-0.0037***
(N = 20,434)	(0.0027)	(0.0005)	(0.0012)	(0.0017)	(0.0011)	(0.0013)	(0.0012)
Mean	0.019	0.001	0.004	0.008	0.003	0.004	0.004

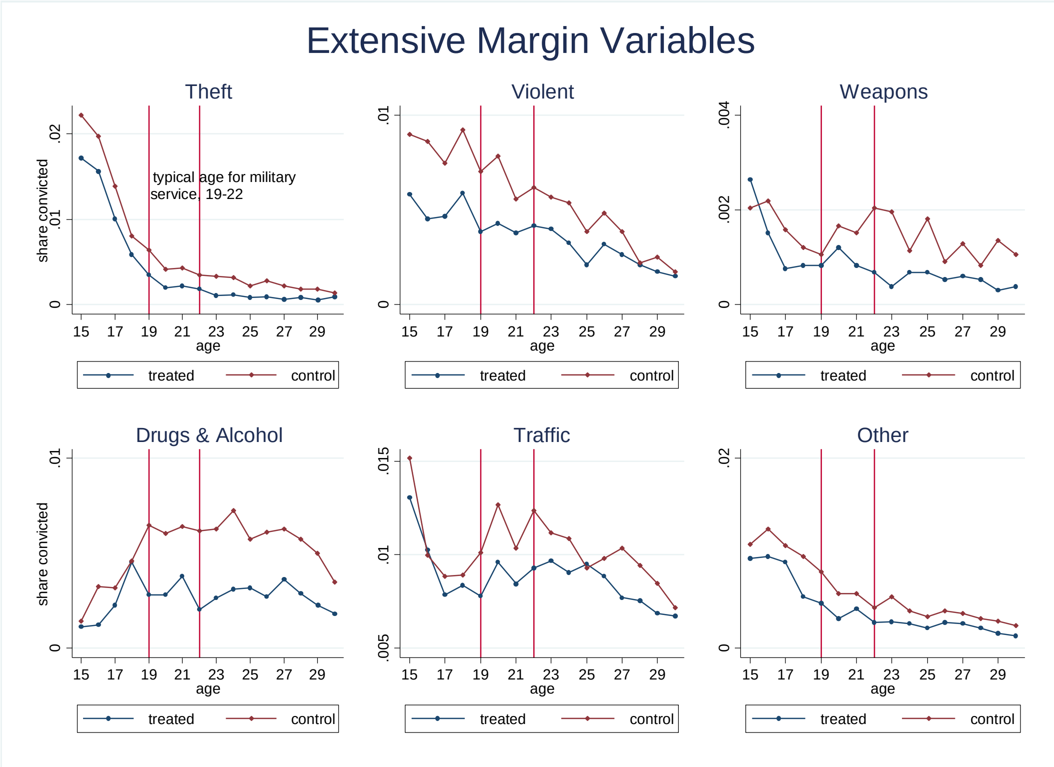
Standard errors [in brackets] do not account for the fact that the propensity score is estimated. *** 1%, ** 5%, * 10%. Criminal history = 1 if the individual has at least one conviction between the ages of 15 and 17. Mean is the mean of the crime variable in period 2 (when the treated are in service).

Table 12. Potential 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	0.029	0.0079	0.024	0.053**	-0.037*	0.007
	[0.0194]	[0.0191]	[0.0213]	[0.0250]	[0.0194]	[0.0251]
Own pre-service crime = 1	0.115***	0.089***	0.115***	0.454***	0.2442***	0.455***
	[0.0037]	[0.0109]	[0.0037]	[0.0249]	[0.0470]	[0.0249]
Father education <= 9 years			-0.002			-0.042**
			[0.00061]			[0.0205]
Unit pre-service crime * Own pre-service crime = 1		0.201**			0.748***	
		[0.0809]			[0.1832]	
Unit pre-service crime * Father education <= 9 years			0.022			0.192**
			[0.0456]			[0.0790]
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

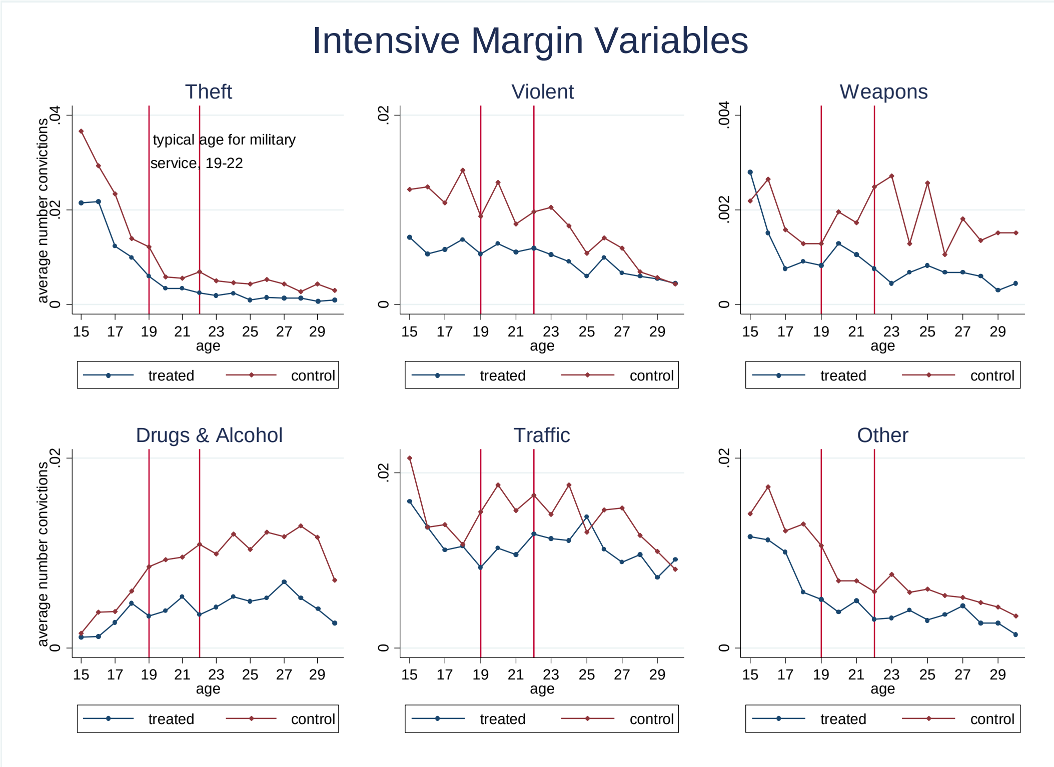
Robust standard errors in brackets; *** p<0.01, ** p<0.05, * p<0.1. This utilizes sample of individuals who tested from 1990-1996 (i.e. the IV sample) who served. All specifications include the full set of test day and pre-test day controls as well as birth month, birth year, municipality and test year by test office fixed effects. 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) nad the number of crimes in columns (4)-(6).

Appendix Figure 1. Trends in Crime Type by Age for Treated and Controls in our Matched Sample, Extensive Margin.



Note – This graph is based on our matched DiD sample, which includes men tested between 1997 and 2001.

Appendix Figure 2. Trends in Crime Type by Age for Treated and Controls in our Matched Sample, Intensive Margin.



Note – This graph is based on our matched DiD sample, which includes men tested between 1997 and 2001.

Appendix Table 1. Sensitivity Analysis of Baseline Specification to Standard Error Clustering

	(1)	(2)	(3)	(4)
Sample	All	All	All	All
Panel A: Dependent Variable = Any Crimes from Age 23-30 (Mean = 0.10)				
military_service	0.05707**	0.05707**	0.05707	0.05707*
	[0.02613]	[0.02512]	[0.03864]	[0.02996]
Panel B: Dependent Variable = # Crimes from Age 23-30 (Mean = 0.31)				
military_service	0.54830***	0.54830***	0.54830***	0.54830**
	[0.18881]	[0.16775]	[0.17458]	[0.21600]
Instrument	leave out annual mean	leave out annual mean	leave out annual mean	leave out annual mean
Cluster Unit	officiator	county	2 way: officiator and test year	officiator x test year
Number of clusters	67	24	67 and 7	203
First Stage F-Statistic	78.96	175.9	81.29	129.1
county x test year fixed effects	yes	yes	yes	yes
Test office Fixed Effects	no	no	no	no
Pre-test and Test day Controls	no	no	no	no

Columns (1) - (4) present the results of instrumenting for service with the leave out mean annual service rate of the assigned officiator. Standard errors are clustered, and first stage f-statistics calculated accordingly, according to the notes in the table. *** significant 1%, ** significant 5%, * significant 10%. N= 168806

Appendix Table 2. Difference-in-Difference Estimates of the Incapacitation Effect of Military Service Using Exact Service Dates and Exact Crime Dates, Intensive Margin.

	Dependent Variable:						
	Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Matched sample	-0.0101**	-0.0008	-0.0022	-0.0052**	0.0018	0.0006	-0.0044***
(N = 26,506)	(0.0048)	(0.0006)	(0.0019)	(0.0021)	(0.0025)	(0.0017)	(0.0014)
Mean	0.034	0.001	0.006	0.010	0.005	0.006	0.005
Matched sample with criminal history	-0.0227	-0.0037	0.0005	-0.0195	0.0099	0.0067	-0.0166*
(N = 2,820)	(0.0279)	(0.0039)	(0.0138)	(0.0126)	(0.0105)	(0.0089)	(0.0086)
Mean	0.147	0.008	0.033	0.033	0.024	0.024	0.023
Matched sample with no history	-0.0087**	-0.0003	-0.0025**	-0.0036**	0.0007	-0.0002	-0.0029**
(N = 23,686)	(0.0042)	(0.0005)	(0.0012)	(0.0018)	(0.0025)	(0.0015)	(0.0012)
Mean	0.020	0.001	0.003	0.007	0.003	0.004	0.003
Matched sample father ed. <= 9 years	-0.0188*	-0.0033*	-0.0017	-0.0066	-0.0020	0.0007	-0.0059**
(N = 6,072)	(0.0103)	(0.0019)	(0.0047)	(0.0051)	(0.0042)	(0.0037)	(0.0028)
Mean	0.044	0.003	0.008	0.011	0.009	0.009	0.005
Matched sample father ed. > 9 years	-0.0075	-0.0000	-0.0023	-0.0048**	0.0029	0.0006	-0.0039**
(N = 20,434)	(0.0055)	(0.0005)	(0.0020)	(0.0022)	(0.0030)	(0.0018)	(0.0016)
Mean	0.030	0.001	0.005	0.010	0.004	0.005	0.005

Standard errors [in brackets] do not account for the fact that the propensity score is estimated. *** 1%, ** 5%, * 10%. Criminal history = 1 if the individual has at least one conviction between the ages of 15 and 17. Mean is the mean of the crime variable in period 2 (when the treated are in service).