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Randi Hjalmarsson & Matthew J. Lindquist

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Department of Economics
School of Business, Economics and Law at University of Gothenburg
Vasagatan 1, PO Box 640, SE 405 30 Göteborg, Sweden
+46 31 786 0000, +46 31 786 1326 (fax)
www.handels.gu.se info@handels.gu.se

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

Randi Hjalmarsson[†]
University of Gothenburg and CEPR

Matthew J. Lindquist[††]
SOFI, Stockholm University

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Abstract

This paper uses detailed individual register data to identify the causal effect of mandatory peacetime military conscription in Sweden on the lives of young men born in the 1970s and 80s. Because draftees are positively selected into service based on their draft board test performance, our primary identification strategy uses the random assignment of potential conscripts to draft board officiators who have relatively high or low tendencies to place draftees into service in an instrumental variable framework. We find that military service significantly increases post-service crime (overall and across multiple crime categories) between ages 23 and 30. These results are driven primarily by young men with pre-service criminal histories and who come from low socioeconomic status households. Though we find evidence of an incapacitation effect concurrent with conscription, it is unfortunately not enough to break a cycle of crime that has already begun prior to service. Analyses of labor market outcomes tell similar post-service stories: individuals from disadvantaged backgrounds have significantly lower income, and are more likely to receive unemployment and welfare benefits, as a result of service, while service significantly increases income and does not impact welfare and unemployment for those at the other end of the distribution. Finally, we provide suggestive evidence that peer effects may play an important role in explaining the unintended negative impacts of military service.

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

JEL: H56, J08, K42.

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[†] University of Gothenburg, Department of Economics, Vasagatan 1, SE 405 30, Gothenburg, Sweden; randi.hjalmarsson@economics.gu.se
[††] Swedish Institute for Social Research (SOFI), Stockholm University, Universitetsvägen 10F, 106 91 Stockholm, Sweden; matthew.lindquist@sofi.su.se.

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).[2] 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. We complement this analysis by applying the same research design to legitimate labor market outcomes, including education, income, and welfare and unemployment benefits, as well as work-related health outcomes (sick days and disability benefits).

Mandatory military conscription in Sweden 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 Swedish male citizens underwent an intensive drafting procedure upon turning 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 likelihood of serving generated by the randomly assigned officiator that our analysis utilizes in an instrumental variable framework to identify the causal effect of conscription on crime. In addition, for a subset of cohorts for whom we know the exact dates

[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.

[2] Though the U.S. moved to an all-volunteer military in 1973, young men ages 18 to 26 are still required to register for the draft. Today, the US is debating extending this requirement to young women. http://www.nbcnews.com/news/us-news/military-officials-women-should-register-draft-n509851

of service, we also utilize a difference-in-difference matching framework to identify the incapacitation effects of service.

There are a number of channels, many of which are opposing in nature, through which military conscription may affect both contemporaneous and future criminal behavior. With respect to the contemporaneous impacts, conscription may decrease criminal behavior through an incapacitation channel, i.e. keeping young men otherwise engaged and isolated from mainstream society. On the other hand, conscripted young men are not under 24-hour supervision, and may still have the opportunity to commit crimes 'after hours'; the increased extent of social interactions that occur during conscription could even feasibly result in an increased propensity to commit crimes that are highly 'social' in nature, like violent crimes.[3]

If conscription does "incapacitate" potential criminals, then this could potentially result in a new path of lower criminal intensity; that is, post-service crime could be reduced simply as a result of a decrease in crime contemporaneous with service. Alternatively, the promotion of democratic values, which is one of the stated goals of mandatory conscription, and the obedience and discipline training that one receives, may decrease post-service criminal tendencies by helping to focus young men at this high risk age. Others argue that exposure to weapons and desensitization to violence, especially during wartime conscription, may exacerbate an individual's criminal tendencies, particularly with respect to violent crime (Grossman, 1995). Military conscription may also positively or negatively impact crime through its impact on education and labor market outcomes. If conscription extends a young males' social networks, is viewed as a positive signal of quality by employers, or improves his marketable skills (e.g. training as mechanics, truck drivers, cooks and medics), health, or physical fitness, thereby improving labor market outcomes, then this could decrease the propensity for crime (Becker, 1968). However, post-service crime may increase if

[3] This parallels the school crime literature, where Jacob and Lefgren (2003) and Luallen (2006) have found an incapacitation effect of schooling on property crime but an exacerbating effect on violent crime, which they argue results from increased social interactions.

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; such peer effects could even lead to social multiplier effects of conscription.

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 non-draftees.[4] 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.[5] Bingley et al. (2014) find large earnings losses for high ability men in Denmark. Two papers find a positive effect of conscription on labor market outcomes in the Swedish context (Hanes et al. (2010) and Grönqvist and Lindqvist (2016)), but only the latter, which focuses on officer training, seriously addresses the biases arising from the endogenous selection process.[6, 7]

Few papers study the effect of conscription on crime in a quasi-experimental setting.[8] Those studying Vietnam Veterans in the U.S. (taking advantage of differential exposure

[4] Likewise, Siminski's (2013) study of Australian Vietnam draftees finds a negative employment effect.
[5] Maurin and Xenogiani (2007) use the abolition of conscription in France to study the effect of schooling on wages.
[6] Using a regression discontinuity design, Grönqvist and Lindqvist (2016) find that officer training in the military significantly increases the probability of becoming a civilian manager.
[7] Albrecht et al. (1999) also report regression coefficients that (in some specifications) show a positive return to military service in Sweden. But as their paper is about explaining the negative returns to time spent out of work (due to, e.g., maternity leave) and not about the effects of military service on earnings, they do not comment on these coefficients nor do they specifically address the issue of selection into military service.
[8]Beckerman and Fontana's (1989) survey of the early criminology and psychology literature finds that Vietnam veterans do not have higher arrest rates than non-veterans. A handful of studies find a positive effect of being a

across cohorts to the draft (Rohlfs, 2010) or the draft board lottery (Lindo and Stoecker, 2012)) find some evidence that conscription causes an increase in violent crimes. Yet, Siminski et al (2016) do not find an effect on violent crime in Australia during Vietnam era conscription. Two papers consider the effects of 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 year in which they begin military service, which typically lasts from 3 to 12 months). However, their data do not allow them to cleanly estimate an incapacitation effect separately from a post-service effect.

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, nearly all previous research focuses on the Vietnam War or cohorts born in the 1950s and 1960s.[9] Second, detailed individual register data allow us to study crimes committed when the men are young adults (i.e. before the age of 30 for all cohorts), whereas most other papers have been limited to studying crime committed after age 40 (Siminski et al, 2013; Galiani et al, 2011). Given that crime has declined for many years on the age-crime profile by age 40 and the evidence that the earnings gap for draftees after the Vietnam War closes by age 50 (Angrist and Chen, 2011; Angrist, Chen, and Song, 2011), it is essential to study criminal

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Vietnam Vet on violent crime, but these are often restricted to those in combat or individuals with mental health problems. See Yager et al (1984), Resnick et al (1989), and Yesavage (1983).

[9] A recent exception is Anderson and Rees (2015), who study those deployed in the Iraq war from Fort Carson, Colorado from 2001 to 2009; they conclude that never-deployed units have a greater impact on crime and public safety than units recently returned from combat. Bingley et al. (2014) also study more recent cohorts of Danish men born 1976-1983. But their paper focuses on the labor market returns to military service. They do, however, report one interesting finding about crime when discussing the mechanisms that might drive the large negative effect on earnings that they find. They report a zero effect of military service on non-vehicular crime at the extensive margin for men aged 26-35. This zero holds for all quartiles of the ability distribution.

behavior in the short- and medium-terms rather than the long-term. Third, the Swedish register data not only allows for more refined crime outcomes, but also one of the most comprehensive analysis to date of the impact of peacetime service on a wide range of labor market and health outcomes. Methodologically, as not all countries assign individuals to service using a lottery system, we apply a new research design in the context of this literature to identify the causal effects of service on crime. Finally, and in contrast to all of the existing literature, information on the exact dates of service allows us to directly estimate the incapacitation effects of service.

We begin with a sample non-immigrant males born between 1964 and 1990 from Sweden's Multigenerational Register (representing about 70% of the population), to which we matched on longitudinal administrative records concerning income, education, tax records, geographical location, criminal convictions, and draft board data. For individuals testing from 1990 to 1996, we can identify the officiators who reviewed the battery of draft board test results and subsequently placed the draftees into service. Though we cannot identify officiators for the 1997 to 2001 test cohorts, we do know their exact dates of service.

These key data features guide the two stages of our empirical analysis. The goal of the first stage is to cleanly identify the causal effect of service on post-service crime. To deal with the fact that conscripts are 'selected' into service on the basis of their draft board test performance, we instrument for whether an individual serves with whether he is assigned to an officiator with a high annual service rate (that is, an officiator whose annual share of testees who serve is greater than the national share who serve in that year). We argue that this instrument 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. With regards to the latter, we demonstrate that assignment to a high service rate officiator increases the likelihood of service by almost eight percentage points, with an appropriately high first-stage F-statistic. We also provide evidence that the necessary monotonicity assumption holds.

The baseline results are striking: military service significantly increases both the likelihood of crime and the number of crimes between ages 23 and 30; as all individuals in our IV sample have completed service by age 23, this is a cleanly measured post-service effect. Such a positive effect is actually seen across all crime categories, with especially significant effects for violent crime, theft, drug and alcohol offenses, and other offenses. The estimated effects are quite large, oftentimes more than twice the mean of the dependent variable, and are driven by those with a criminal history prior to service or from low socioeconomic status households. This heterogeneous impact of service is also seen with respect to labor market outcomes. Individuals from disadvantaged backgrounds have significantly lower income, and are more likely to receive unemployment and welfare benefits. In contrast, military service significantly increases income and does not impact welfare and unemployment for those at the other end of the distribution. There is no effect of service on the likelihood of higher education. The only positive effect of service we see, at least for those from disadvantaged backgrounds, is a decrease in disability benefits and the number of sick days; these effects are in fact seen for all subsamples. We demonstrate the robustness of our results to an alternative instrument (the leave out annual mean), a large set of observable controls, and a falsification test that considers crime committed *prior* to service.

To isolate incapacitation in the IV framework, we consider crimes committed at ages 19 and 20 for those who tested at 18. Though these results suggest an incapacitating effect of service, it cannot be ruled out the crime outcomes include some pre- or post-service crimes. Thus, the second stage of the empirical 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. We then use the exact dates of service for the treated individual to construct the counterfactual time of incapacitation for the control group and apply a difference-in-difference estimator to identify the incapacitation effects of military service. We find large and significant incapacitation effects for drug and alcohol offenses at both the extensive and intensive margins. For traffic crimes, we find a large and significant incapacitation effect at the intensive margin, especially for those with prior criminal convictions. Taken together, our difference-in-difference and instrumental variable estimates imply that there are significant incapacitation effects of military service. Unfortunately, our analysis also suggests that these effects are not large enough to break a cycle of crime that has already begun prior to service.

Finally, we demonstrate that individuals with criminal histories prior to service 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 criminal history prior to service 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. Another possible mechanism is that low skilled men are harmed by their delayed entry into the labor market during years when unemployment among young adults is unusually high.

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 high service rate officiator instrumental variable strategy to identify the post-service effect of conscription. Section 4 presents the instrumental variable results for crime and non-crime outcomes as well as heterogeneity and sensitivity analyses. Section 5 presents the difference-in-difference matching framework to isolate incapacitation and results. Section 6 discusses the 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, endurance, strength, 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.[10] 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 who were deemed fit to serve and placed in service categories and the share who actually served in the military. Roughly 95% of all men born before 1979 were called and tested; the remaining 5% were typically non-citizens or individuals with severe mental or physical problems that exempted them from military service. At that time, noncompliance was punishable by jail and this law 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 meant to avoid testing those most likely to be

[10] Military training was typically 7 to 15 months long, but could vary between 60 and 615 days.

exempted for mental and physical health reasons.[11] In 2007, *Pliktverket* began using an online tool to further pre-screen potential conscripts – only a small share of the most suitable, willing and able were called for testing, and even fewer were placed in service. Finally, on July 1, 2010, Sweden adopted an all voluntary military service, though male citizens must still register with the official recruitment office (*Rekryteringsmyndigheten*) upon turning 18. This is one of the most significant policy changes in recent Swedish history; yet, its consequences have not 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 test date was based only on his month and year of birth, municipality of residence at age 17, and, in some cases, the expected date of high school graduation.[12] In 1990, there were six regional test offices in Sweden, each serving a specific geographic catchment area; one of these test offices closed in 1995. Test offices were required to fill troop orders placed by the military. Each office filled orders from all branches of the service, including both local military units and specific military units in other parts of 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. It was not uncommon for one group to come early in the day and another group to arrive somewhat later. Conscripts started the test day with an information meeting, at which they 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,

[11] On July 1, 1995 a new law concerning the total defense of Sweden came into force. It encompassed both military and civil service (*lagen om totalförsvarsplikt* 1994:1809) and stated that those clearly unable to participate in military or civil service should not be called to the testing days.
[12] Carlsson et al. (2015) discuss the assignment of test dates and test offices in great detail.

verbal, spatial, logical and technical ability, (ii) a telegraph test, and (iii) a series of medical and physical tests, examining their hearing, vision, strength, height, weight, blood pressure, physical condition, etc. They had an examination with a medical doctor, and (typically on day two) met with one of the test office's psychologists for an interview. The results from each test were entered into the test office's computer system and 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, discussed alternative options, and decided whether they were exempt from service (for instance, due to mental or physical problems). Those not exempted (the vast majority) were assigned a specific service category and preliminary start of service date by this officiator.

Placement into service categories was based on an individual's full range of tests 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. 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] 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 still be called to serve since they were assigned to a different 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.*

[13] Test officiators used computers to help make the first match between a candidate's scores and a set of suitable service categories. The officiator decided the exact service category assignment after interviewing the conscript.

Importantly, since the military was continually downsizing during the time period we study, 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 discretionary judgement on the part of the officiators when determining the most suitable candidates. Officiators discussed these decisions both within and between test offices on a regular basis throughout the year (typically four times a year). Thus, the test officiator can affect the probability of service of the marginal draftee by (i) assigning a draftee to a service category in high demand and (ii) advocating the case of his or her favorite candidates for the position.[14]

As such, the test officiator plays a significant role in the assignment of men to active military service during the period when not all men are required to do military service, i.e. when officiator discretion is relevant; the variation across individual test officiators in their propensity to assign men to service (both within and across test offices) is a key component of our identification strategy. 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 time period. The story is quite simple. Draftees arrived at the center, and were led through a series of test stations, some 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 test station (information is also entered into the computer system). The conscript arrived in a waiting room outside the officiators' offices (there are always multiple test officiators in each office) and placed his folder on the top of a pile in a box. The next available officiator removed the folder from the bottom of the pile and met with that conscript. This match is as good as random once we condition on test year and county fixed effects.[15]

[14] 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.
[15] Recall that test office is determined by geographical location, i.e. by county.

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. The test office received orders from the military for troops (number and type) and 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 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.

2.3.1. The Draft Data

We have draft board data from The Swedish Military Archives and The Swedish Military Recruitment Office, though we only use the latter in our empirical analyses. The former was used to help characterize historical trends in testing and service (see Figure 1).

Our main analysis uses the test officiator id as an instrument for military service for potential conscripts who tested between 1990 and 1996 for two reasons. First, the officiator

[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.

IV strategy does not work for earlier cohorts, since almost everyone who tested before 1990 served; that is, there is little room for officiator discretion. Second, the officiator id is missing from the recruitment office's records for cohorts tested between 1997 and 2001.[17]

We cannot use the draft board data to assign treatment status, since their data are incomplete when it comes to identifying which young men actually served in the military. Thus, to identify treatment status, i.e. those who served at least two months, 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 only see that a payment was received during the year, and not when it was received, we can only use this tax register data to identify individuals who were enlisted soldiers. Thus, for individuals who tested between 1990 and 1996, we cannot identify the exact dates of service. We do, however, have exact service dates for those who tested between 1997 and 2001, which we use to perform an alternative (non IV) incapacitation analysis, since we do not have a test officiator id for these test cohorts.

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, and whether or not the person was assigned to a service category. Health scores and our measure of physical capacity are both summary measures based on a series of underlying tests.

2.3.2. Outcome Variables – Crime

We study the causal effect of mandatory military service on crime and a number of legitimate labor market outcomes. To this end, our data set was matched with the official crime register

[17] The officiator variable re-appears in the data in 2002. But by this time, less than half of all Sweden born males were placed into service categories and less than half of those were enlisted in the military (see Figure 1). Although it was still illegal to refuse military service, it had become more or less optional for young men.

(*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 when using 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 the date that the offense was committed. We study crime at both the extensive and intensive margin, for *Any Crime* and by six specific crime categories: *Weapons*, *Violent*, *Traffic*, *Theft*, *Other*, and *Drugs & Alcohol*. Our extensive margin variables are dichotomous and equal to one if the individual has at least one conviction in the appropriate category while our intensive margin variables equal the number of convictions overall and in each category. 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.

2.3.3. Background and Control Variables

We also use a number of background and control variables from register data held by Statistic's Sweden. We make use of *Birth Month*, *Birth Year*, and *County* of residence at age 17.[18] 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.[19]

3. Identifying a Post-Service Effect

The empirical analysis is conducted in two stages. The first 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

[18] We also have parish and municipality at age 17.
[19] 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.

using an instrumental variable design that capitalizes on the random assignment of potential conscripts to officiators who assign more or less individuals to service.[20] Though we can use this design to identify post-service crime effects that are not muddied by incapacitation, we cannot directly identify the incapacitation effect itself for this sample given the unavailability of the exact dates of service. Thus, Section 5 uses the 1997 to 2001 test cohorts, for whom we know the exact dates of service, in a difference-in-difference matching framework to further isolate incapacitation. This section describes the use of the officiator as an instrument.

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, $Conscript_i$.

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

Even with a large set of observable controls, X, whether an individual is conscripted 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 test results (like family background, innate ability, performance under pressure, etc.), affect the likelihood of service as well as criminal behavior 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 a dummy variable indicating whether the individual is assigned to a '*high service rate officiator*'. This variable is equal to one if the annual share of testees assigned to the officiator who serve is greater than the

[20] In spirit, 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.

national share who serve in that test year.[21] That is, is individual *i* assigned to an officiator with a relatively high service rate in test year *t*, compared to the average service rate in test year *t*? As such, we are utilizing 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, rather than variation that arises due to some officiators working at the beginning of the sample period (with somewhat higher service rates) versus the end.

Though there are clearly alternative ways to define the instrument, such as officiator fixed effects (i.e. the leave out mean), we use a dichotomous variable because of its simplicity and ease of interpretation. In contrast to other papers using harsh judges as instruments for sentence severity (Aizer and Doyle, 2015; Kling, 2006), the first stage is strong enough to allow for such a simple specification; however, we also demonstrate the robustness of our results to an alternative instrument – namely the leave out annual mean.

To isolate the post-conscription effect of service from incapacitation (and be certain that the outcomes are observed after service is completed), we (i) define our primary outcome variables by age, emphasizing crime that occurred 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. Before turning (in Section 3.3) to the relevance and validity of our proposed instrument, we briefly discuss how our analysis sample is created and descriptive statistics.

3.2. Sample Creation and Descriptive Statistics

Our baseline data set consists of 231,707 non-immigrant males born from 1964 to 1990, who tested from 1990 to 1996. Recall that we use this restricted set of test years to allow for the creation of the instrument. We then systematically drop observations which a priori make the

[21] We create this variable based on our baseline sample of all non-immigrant males who tested each year, and not the final analysis sample.

treated (service) and control (non-conscripted) individuals more comparable. See Appendix Table 1 for details. We keep just those 190,520 individuals who are assigned to service categories; this does not mean that they serve, but rather that they are eligible to serve. We also drop testees with officiators who see less than 100 cases in their test year (about 7,500 observations) and those with missing health group information or who are assigned to health groups that never serve in a given year (about 9,500 observations). Finally, we drop 4,783 individuals who are 23 or older in the year they finish service (or for whom the year finished is unknown).[22] The final analysis sample consists of 168,818 non-immigrant males tested between 1990 and 1996.

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 are not stationed in a single office but rotate across test offices in a given year. In fact, just 42 percent of officiators are stationed in a single office each year; 19 percent in two, 17 percent in three, and the remaining 22 percent in four or more.

Table 1 provides summary statistics for the analysis sample, overall and by service. During this period, 75 percent of the sample that was eligible to serve actually served. Looking across test years in our analysis sample, Figure 2 shows that service rates begin to decline after the 1993 test year. (As we have already conditioned our sample on being eligible to serve, service rates for the entire sample would be even lower.)

Table 1 also demonstrates that despite coming from comparable birth and test cohorts, those who serve are 20 percentage points more likely to have been assigned a high service rate officiator than those who do not serve. This pattern is clearly suggestive of the first stage relationship that we intend to use in the instrumental variable analysis.

[22] Though we do not know the exact dates of service for these test cohorts, we do know the last year in which they are observed receiving income from the military according to the tax records. Thus, we drop those individuals who are 23 or older in this last year.

The middle panels of Table 1 characterize the potential conscript's criminal history and socioeconomic status prior to testing, as well as his performance on the test day. The service sample is positively selected in all dimensions: they have less criminal history, come from more educated families, are more likely to attend three versus two year high schools, and have higher ability and physical capacity test scores. Though those who serve are less likely to have a criminal history, having a criminal history does not disqualify one from military service. In fact, 13 percent of the service sample has at least one conviction prior to age 18. Similar patterns are also seen when looking at the service and no-service samples separately for high and low service rate officiators; those who serve are slightly positively selected in terms of family background and criminal history, regardless of who assigns them to service – i.e. high or low service rate officiators.[23]

Finally, the bottom of Table 1 considers the main crime outcomes. Overall, we see that 10 percent of the sample is convicted of at least one crime between ages 23 and 30, and that the average number of crimes convicted (including zeroes) is 0.31. At both the extensive and intensive margins, the largest crime category is traffic offenses; six percent of the sample has at least one traffic conviction. The other crime categories have much lower conviction rates: one percent for weapons, two percent for violent, one percent for theft, two percent for drugs, and two percent for other offenses. We also see that for almost every crime measure, the crime rate is higher for the sample that does not serve compared to the sample that does. 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

[23] Available from the authors upon request.

A good instrument is one that is relevant and valid, and for which the assumption of monotonicity holds. In the current context, this implies that (i) assignment to an officiator with a higher than average annual service rate significantly increases the likelihood of serving, (ii) whether a testee is assigned a high service officiator is unrelated to testee unobservable characteristics, and (iii) officiators do not dramatically change their behavior and switch from being a high service officiator to a low service officiator (or vice versa) depending on the type of testee they meet.[24] We argue that the proposed instrument meets all three criteria.

With regards to validity, it is important to recall that testees are randomly assigned an officiator after completing their battery of tests (as described in Section 2). Such random assignment implies that both observable and unobservable testee characteristics should be uncorrelated with officiator characteristics, including whether they are a high service rate officiator. However, since officiator characteristics may vary *across* test offices or test 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 officiator to which a testee is assigned is as good as random. Because assignment to test offices is based on where one lives, we use county fixed effects to control for this geographic variation in test office assignment.[25]

As a first crude test of random assignment, we simply compare the raw differences of individuals assigned to officiators with high and low annual service rates in Table 2. While the percent difference in the share serving is 16 percentage points (almost 19 percent) across these two groups, the raw percent difference in the observable characteristics is in all cases

[24] All testees must be at least weakly more likely to serve when facing a high service rate officiator. See Mueller-Smith (2015) for an in depth discussion of the monotonicity assumption in the context of the judge fixed effects identification strategy.
[25] 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.

but one less than five percent and in many instances less than one percent.[26] Though the observable differences are small, it is not surprising that some differences do exist, as the analysis does not condition on time and geography.

We more formally test for random assignment by regressing assignment to a high service rate officiator on the full set of test day (height, weight, bmi, ability score, physical capacity score) and pre-test day (crime before 18, mother and father schooling, mother and father income, and 2-year versus 3-year high school) characteristics in Table 3. Note that all tests of significance are based on standard errors clustered at the officiator level. To summarize the results, we tabulate the number of significant coefficients and present the p-values for F-tests of the joint significance of the (i) pre characteristics, (ii) test day characteristics, and (iii) all characteristics. In column (1), with no additional controls, we find that these variables are jointly significantly different than zero; this is driven by significant coefficients on criminal history and parent socioeconomic status. But, even in this raw data, there is already strong evidence of random assignment. First, these significant coefficients actually point in opposite directions: it is not that those with better backgrounds, for instance, are consistently more likely to be assigned a high service rate officiator. Second, given that 50 percent are assigned a high service rate officiator, the magnitudes of these estimates are relatively small. Finally, all of these controls only explain one percent of the variation in the assignment to a high service rate officiator.

Nevertheless, we proceed by demonstrating that the observed relationships get an order of magnitude smaller and become jointly insignificant when controlling for county and test year fixed effects. Specifically, adding county fixed effects in column (2) and test year fixed effects in column (3) indicate that both the vector of pre-characteristics and the vector of both pre- and test day characteristics are not jointly significant (p-values of 0.15 and 0.12

[26] The one exception is the share in a 2-year high school, but as 2-year programs are greatly trending down in this period (and 3-year programs trending up), this raw comparison may not account for this variation.

respectively). Adding county by test year fixed effects in column (4) further reduces the magnitude of the coefficients. Though the test day characteristics are still jointly significant, this is driven by significant coefficients for the ability and physical capacity scores that are not economically significant and which are inconsistent in sign: a one point increase in the ability score (on a nine point stanine scale) decreases the chance of a high service officiator by 0.13 percentage points while a one point increase in the physical capacity score (on a nine point stanine scale) increases it by 0.15 percentage points. Adding test office and test office by test year fixed effects in Columns (5) and (6) has little impact on these relationships. Finally, Column (7) of Table 3 examines how that variation in the high service officiator assignment that is *unexplained* by county by test year fixed effects is related to the pre- and test-day characteristics; that is, the dependent variable is the residual from a regression of a high service officiator dummy on county by test year fixed effects. None of this unexplained variation is explained by the full set of observable controls.

We thus argue that officiator assignment is random when conditioning on county by test year fixed effects, and therefore define our baseline instrumental variable specification accordingly (i.e. include test year dummies, county dummies, and test year by county dummies). 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 first stage estimates are stable once county by test year fixed effects are included and that the instrumental variable estimates are not sensitive to a large set of controls.

Table 4 presents the results of the first stage analysis: does assignment to a high service rate officiator increase the likelihood of serving in the military? According to column (1) with no controls, assignment to a high service rate officiator significantly increases the likelihood of service by almost 16 percentage points. However, much of this is accounted for by regional and temporal variation in service rates. Column (4) includes test year by county fixed effects – in this case, assignment to a high service rate officiator increases the likelihood of service by 7.9 percentage points, with an associated F-statistic of 32 (well above the weak instrument threshold). The remaining columns add controls for test office fixed effects, test office by test year fixed effects, pre-test day characteristics, and test day characteristics. This has little impact on the magnitude of the first stage effect and associated F-statistic.

To investigate the assumption of monotonicity, we re-calculated our high service officiator dummy for each year and officiator using samples of (i) only men from low SES families, (ii) only men from high SES families, (iii) only men with prior convictions, and (iv) only men without prior convictions. We then examine whether any officiator (in a particular year) changes from a high to low service status (or vice versa) when facing one of these four types of men. They do not. Only a few year by officiator dummies are re-categorized and no officiator is consistently re-categorized in multiple years when facing a particular type of young man.

4. High Service Rate Officiator Instrumental Variable Results

4.1. Baseline Results and Robustness Checks

Table 5 looks at the relationship between serving in the military and overall post service crime from ages 23 to 30; Panel A considers the extensive margin (at least one conviction) and Panel B considers the intensive margin (number of convictions). OLS estimates in Columns (1) – (3) find a negative correlation between service and overall crime at both the extensive and intensive margins. When including county by test year fixed effects in column (2), service is associated with a 1.7 percentage point (17%) reduction in the likelihood of at least one conviction and, on average, 0.13 (42%) fewer convictions. Though controlling for test office fixed effects and the full set of pre-test and test day characteristics (including having a conviction before 18) substantially reduces the magnitudes of these relationships, they remain negative and highly significant.

Column (4) presents the results of our baseline instrumental variable specification (i.e. with county by test year fixed effects). All instrumental variable specifications cluster the standard errors on the officiator. These results indicate that military service in fact has a strong positive causal impact on post-service crime from age 23 to 30 at both the extensive and intensive margins. Serving in the military increases the likelihood of post-service conviction by 8.9 percentage points and, on average, leads to an increase of 0.56 convictions at the intensive margin. Relative to the mean, these estimated effects are quite large – an increase of about 90 percent at the extensive margin and more than 100 percent at the intensive margin.Though the associated standard errors and confidence intervals are quite large, the IV estimates clearly indicate a significant positive effect of service on crime. One should also keep in mind that 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. Column (5) presents the IV results when controlling for test office and pre-test and test day characteristics; consistent with random assignment to officiators, this does not change the estimates.

Our baseline specification clusters on officiator. However, one could argue that it is more appropriate to cluster in other dimensions, such as test year or test year and officiator. Appendix Table 2 demonstrates that our results are robust to clustering the standard errors on county (column 2), both officiator and test year (column 3), and officiator by test year (column 4). Column (5) of Appendix Table 2 presents the results with an alternative instrument – the leave out annual mean; rather than using a binary indicator of whether the officiator is a high service rate officiator, column (5) instruments for service with the actual share of testees who serve in a given test year (excluding individual *i*). The results are quite comparable: military service leads to a 5.7 percentage point increase in the likelihood of conviction and, on average, an increase of 0.55 convictions. The intensive margin estimates are in fact almost identical to those seen with our more simple binary instrument.

These results clearly demonstrate a positive impact of military service on *overall* crime. But, one may ask whether such an effect is seen across crime categories or driven by a particular crime category. Table 6 addresses this question, and further demonstrates the robustness of these crime specific results to the inclusion of controls.[27] The dependent variable in the upper panel measures crime at the extensive margin (at least one conviction in the crime category indicated at the top of the column) while the lower panel measures the intensive margin (i.e. the number of convictions in each crime category); we decompose overall crime into weapons offenses, violent offenses, traffic offenses, theft, drugs and alcohol offenses, and other offenses. Row (a) of each panel presents the baseline specification. Column (1) aggregates all crimes together, and is equivalent to the baseline specification presented in column (4) of Table 5. At the extensive margin, we find that service significantly increases the likelihood of at least one weapons conviction between ages 23 and 30 by 1.7 percentage points and a violent offense conviction by 4.9 percentage points. At the intensive margin, service significantly increases the number of convictions for: violent offenses by 0.10, theft by 0.17, drugs and alcohol offenses by 0.12 and other offenses by 0.10. As we saw when looking at overall crime, these effects are quite large – often more than two times the mean. Though the estimates for the remaining crime categories are not significant, they are all positive and also quite large. Row (b) of Table 6 controls for having a criminal history (in the crime category corresponding to the dependent variable) while row (c) adds the full set of test office, pre-test and test day controls. This has minimal impact on the magnitude or precision

[27] Though not presented, these crime specific outcomes are also robust to how the standard errors are clustered and using the leave out annual mean as the instrument.

of the effects. Given that some variables, especially criminal history, are particularly strong predictors of future crime, the insensitivity of the IV estimates to their inclusion additionally supports of the identifying assumption that testees are randomly assigned to officiators.[28]

Finally, Table 7 presents the results of a falsification test. We re-estimate the baseline specification by crime category, but define the dependent variables to be convictions (at the extensive and intensive margin) prior to age 18, i.e. prior to service. If our previous results are capturing a causal impact on post-service crime, then we should not see a significant relationship between service and pre-service crime. This is indeed what we find. Though some of the point estimates are large, none are significant. And, in fact, many are an order of magnitude smaller than what is observed when looking at the effect on post-service crime.[29]

4.2. Heterogeneity Analyses

This section examines whether the results are heterogeneous across two dimensions of the testee's pre-test background: (i) whether they have a criminal history prior to age 18 and (ii) whether they come from a low socioeconomic status family, for which we use father's education as a proxy.[30] Table 8 splits the sample by criminal history, and finds that the significant impact of military service on post-service crime, overall and across crime categories, is almost completely driven by individuals with at least one pre-service conviction. Serving in the military appears to further criminalize those with a criminal history – it does not lead to a break in the path of crime on which they were already on, and if anything, it reinforces earlier criminal tendencies. For those with no history, it is only for the extensive

[28] Having any conviction prior to age 18 increases the likelihood of conviction between 23 and 30 by 16 percentage points; the corresponding estimates by crime category are 6 for weapons offenses, 15 for violent offenses, 12 for traffic, 6 for theft, 14 for drugs and alcohol, and 7 for other offenses. These relationships are very precisely estimated, with t-statistics ranging from 8 to 30. In addition, the results are robust to even a more demanding set of controls, including birth month by birth year fixed effects and test month by test year fixed effects.

[29] In addition, if one estimates these specifications using OLS, we find negative and significant relationships for almost all crime categories at both margins – again, demonstrating the selection into service.

[30] First-stage F-statistics for subsamples are reported in the tables and comparable to that for the whole sample.

margin violent offense category that we see a significant increase in post-service crime. Put in this perspective, the large point estimates make more sense. For those with a criminal history, serving in the military increases the likelihood of any convictions by 22 percentage points (or slightly less than 100%) and the number of convictions by 2.8, relative to a mean of 1.2. For this sample of highly criminal individuals, these large effect sizes do not seem unreasonable.

Similarly, when splitting the sample according to father's education (nine or less years, ten to 12 years, and more than 12 years), we find that individuals from the two lowest education categories (especially the less than nine years category) are driving the results. For individuals with high educated fathers (more than 12 years), there are no significant impacts of service on crime; in fact, the estimates are not even uniformly positive.[31]

4.3. Non-Crime Outcomes

This section considers the effect of service on outcomes other than crime. 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. It is natural to ask whether military service affects education and employment outcomes. To this end, we create a set of non-crime outcome variables from Statistics Sweden registers and the tax registers. From the latter, we have information on income and the use of benefits, including sickness, disability, welfare, and unemployment. *Income* is the log of average income between the ages 30 and 34.[32] The income concept we use is pre-tax total factor income. *Sick Days* is the

[31] We also split the sample according to assigned rank, and group the two lowest ranks (privates and corporals) and the two highest ranks (sergeants and 2nd lieutenants). The results are driven by the lower ranked individuals who make up more than 90 percent of the sample. Though the point estimates for the higher ranked individuals are in the same direction, they are an order of magnitude smaller and insignificant; however, first stage f-statistics for the high ranked individuals are only five, and so the estimates for this sub-sample may suffer from a weak instrument problem.

[32] 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). We would have preferred to use a measure of income averaged over a longer series of incomes measured and at later ages, e.g. 30 – 40. Unfortunately, our cohorts are simply too young. So we do not have the data to do this. Using income measured before age 30 would severely

number of days a person has received sickness benefits between the ages of 23 and 34. *Disability Pension* is the number of years in which a person has received a partial or full disability pension between the ages of 23 and 34.[33] *Unemployment Benefits* equals the number of years during which an individual has received at least one payment from the unemployment insurance system between the ages 23 and 34. *Months on Welfare* is the number of months a person has received welfare benefits between the ages of 23 and 34. We also consider the extensive margins for unemployment, welfare, and disability pensions. Lastly, we create a dichotomous variable for *Education*, which equals one if an individual has obtained at least some college by 2012 and zero otherwise.[34]

Table 10 presents the baseline instrumental variable results for the whole sample, the criminal history versus no history samples, and the father with nine or less and more than nine years of schooling samples. Overall, and for each subsample, we do not find any evidence of a causal effect of service on attaining more than 12 years of schooling. However, we do find consistent evidence that individuals from disadvantaged backgrounds (have a criminal history or low educated father) are made worse off in the labor market as a result of military service, while those from better backgrounds are either not affected or made better off. Specifically, with regards to income,military service significantly decreases log income between the ages of 30 and 34 by 2.3 percent for individuals with low educated fathers and increases incomes by 1.7 percent for those with high educated fathers; similar estimates (though not quite significant) are seen when splitting the sample by criminal history.[35] We also find that military service increases the likelihood of receiving any unemployment benefits from 23 to

bias our measure of 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.

[33] Full (partial) disability pensions are granted to those who have no (reduced) work capacity due to mental or physical health issues that are deemed permanent. That is, the disability pension system tends to be an absorbing state. Those with health issues deemed temporary receive sickness benefits when they are unable to work.

[34]We focus on legitimate labor market outcomes, as these are a natural complement to crime, or the illegitimate labor market. We considered family outcomes, but do not find any significant or consistent effects of service on partnerships or marital status. We have not studied fertility, since we do not observe completed fertility.

[35] The standard deviation of income is 0.78. Thus, the difference between these two estimated effects (0.21+|-0.29| = 0.5) is equal to 64% of a standard deviation.

34 by 36 percent for those with a criminal history and 28 percent for those with low educated fathers; there is no effect for those with no history or higher educated fathers. The same pattern is seen when looking at the number of years of unemployment, though the estimates are generally insignificant. Military service also significantly increases the number of months of welfare receipt from ages 23 to 34 by more than eight months for those with a criminal history and almost five months for those with low educated fathers; these effects are quite large relative to the respective sample means of 4.2 and 1.98 months.

In contrast, when looking at disability benefits and the number of sick days, we see that military service actually leads to an improvement in outcomes for everyone. Service decreases the likelihood of receiving any disability pensions from age 23 to 34 by six percentage points for those with a criminal history and 1.6 percentage points for those without. Service also significantly decreases the number of sick days by 108 days on average for those with a criminal history and 53 days for those without; for both groups, relative to the mean, service decreases the number of sick days by a little more than 100 percent.

Taken together, these results suggest that peacetime military conscription increases participation in the illegitimate labor market and decreases participation in the legitimate labor market, particularly for those from the most disadvantaged part of the distribution. 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. Unfortunately, it is not possible to empirically disentangle the channels through which these effects occur. That is, serving in the military clearly impacts both post-service crime and labor market outcomes; but it is not clear whether military service has a direct effect on one of these outcomes, which indirectly affects the other.

On the other hand, there is some evidence that all individuals are healthier as a result of peacetime conscription, perhaps because of the physical training received. Of course, one should recall that this sample is already positively selected on health status. This, not surprisingly perhaps, contrasts previous research that finds detrimental effects of Vietnam Era service on a number of morbity measures (Johnston, Shield, Siminski, forthcoming) and disability receipt (Autor, Duggan, and Loyle, 2011; Angrist, Chen, Frandsen, 2010).[36]

4.4. Isolating Incapacitation

This section uses the same instrumental variable approach to identify incapacitation, i.e. whether individuals commit less crime *during* military service. Because we do not know the exact dates of service for our IV sample, we (i) restrict the analysis to individuals who tested in the year they turned 18, almost all of whom serve when 19 and/or 20, and (ii) create measures of crime convictions at 19 and 20. But, we cannot rule out that some of these crimes occurred before or after service, given that service is not for the whole two year period.

Table 11 presents the results for the whole sample, and by criminal history. For the whole sample, service decreases the likelihood and number of convictions (of any type) at age 19 and 20, though these estimates are not significant. Looking across crime categories, about half of the estimates are positive and the rest are negative (in contrast to all positive coefficients in the post-service analysis); there is even a marginally significant negative effect of service on violent crime convictions. This 'incapacitating' violent crime effect is seen for those with and without a criminal history. Individuals with a criminal history are also significantly less likely to be convicted of traffic and other offenses. Individuals with no history are significantly less likely to be convicted of drugs and alcohol offenses (though the conviction rate is extremely low for this sample) and more likely to be convicted of traffic

[36] Other papers that look at the impact of military service on health outcomes include Dobkin and Shabani (2009) and Bedard and Deschenes (2006).

offenses; the latter is the only significant positive estimate.[37] The results generally suggest that military service has some incapacitating effect on crime.

Appendix Table 3 supports this by looking at the effects of service on crimes from ages 23 to 30 for this selected subsample of individuals tested at age 18. We still see positive coefficients for 11 of 12 crime specific regressions, and large, significant effects for violent offenses, theft, and other offenses; the negative effects observed at ages 19 and 20 cannot be attributed to the sample of 18-year old testers being non-representative of the whole sample.[38]

5. Incapacitation Analysis: Difference-in-Difference Matching Framework

Section 4.4 finds evidence suggestive of incapacitation in our instrumental variable framework. As we do not have exact dates of service for this sample, it cannot be ruled out that these crime outcomes include some pre- or post-service crimes. In this section, we take advantage of the fact that we have exact dates of service for the cohorts tested between 1997 and 2001, and apply a difference-in-difference strategy to a matched sample to cleanly estimate the potential incapacitation effect of military service.

5.1. Sample and Matching

Our strategy for estimating a clean incapacitation effect involves creating a matched sample. Treated individuals are each matched to one specific control individual. Each control individual is then assigned the exact same service dates as his treated counterpart. This enables us to construct the counterfactual time of incapacitation for the control group. We then use this matched sample in a difference-in-difference (DiD) framework to identify the incapacitation effect of military service.

[37] This could potentially be capturing post-service effects on traffic offenses, or even traffic offenses committed during service, if individuals drive to and from service (on a weekly basis in some instances).
[38] The only category for which the results substantively differ compared to the whole sample is weapons: for the whole sample, there is strong positive effect whereas we see a zero effect for the incapacitation subsample.

Our matched sample is constructed as follows. We start with a sample of 125,888 men tested between 1997 and 2001. Then we keep only those men who served in the military (48,453) 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 the conscription, each person is assigned a health and physical aptitude category. At this time, only those with categories A, B, D, E, F, and J actually served. We, therefore, exclude those who were assigned some other health category. This reduces the size of the potential control group from 72,763 individuals to 36,401 individuals. We drop 105 individuals from the treatment group because of obvious mistakes in their service dates. Lastly, we drop 183 men who either serve for less than two months or more than 24 months (the latter have chosen to stay on as professional military officers). This leaves us with 28,263 treated individuals who served in the military.

We then estimate a propensity score for military service using a logit model that includes: municipality of residence at age 17, mother's and father's education and income, enrollment in a 2-year or 3-year high school program, verbal and general ability tests scores, bmi, physical capacity, health group, test month, test year, and test office.

We first match exactly on birth month and birth year, to insure that the treated and control individuals in each matched pair are the same age. We then use the estimated propensity score to conduct a 1 to 1 nearest neighbor matching (within each birth month by birth year cell) without replacement using a caliper of 0.02.[39] This exercise produces a sample of 15,041 matched pairs of treated and controls (and a total sample of 30,082 individuals).

In Figure 3, we plot the age crime profiles of the treated and control groups for crime at the extensive and intensive margins. Similar plots are shown for our six crime categories in Figures 4 and 5. Note that for the moment we are not making use of exact service dates; we

[39] All observable variables balance once the caliper is reduced to 0.02 (or smaller). We also trimmed the sample to exclude observations with propensity scores lower than 0.01 or higher than 0.94. This insures that all matches are made using individuals with propensity scores that lie on the common support.

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 red lines in each graph.

In Figure 3, we see that those who serve are clearly positively selected in terms of their pre-service criminal convictions. We saw this in our IV sample as well. However, despite this difference in levels, the pre-service trends of treated and controls are quite similar, which is important given the pre-service parallel trends assumption that must hold to interpret any measured effect from our DiD estimates as causal. In Figures 4 and 5, we see that the treated and controls are actually quite similar in terms of their pre-service convictions for drug and alcohol related crimes and traffic crimes. These figures also provide descriptive evidence of an incapacitation effect for these two crime types, since the trends between the treated and controls clearly diverge at age 19 – the age when many young men begin military service.

The age-crime profiles presented in Figures 3-5 for the 1997 to 2001 test cohorts also highlight two other important phenomena. First, crime rates for these cohorts are lower (for both the treated and control groups) than the test cohorts used in the instrumental variable analysis because (i) crime is trending down for everyone over this time period and (ii) there is even more positive selection on who is eligible to serve in the military at this time – the quotas decreased and the selection thresholds correspondingly increased. A second important takeaway is that identifying the incapacitation effect of conscription using a within individual analysis over time (i.e. comparing convictions pre-service to those during service) will likely be biased given the downward sloped age-crime profile. Likewise, simply comparing crime during the service period across treated and control individuals will yield biased estimates due to the selection into service. To deal with these two likely sources of bias, we apply a difference-in-difference framework (DiD) to our matched sample.

5.2. Difference-in-Difference Incapacitation Results

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:

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

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 $\hat{\delta}$. Figures 2 – 4 provide evidence in favor of the parallel trends assumption that allows us to interpret $\hat{\delta}$ as causal.

Estimates of the incapacitation effect are shown in Table 12. As in our IV incapacitation experiment (see Table 11), the estimated incapacitation effects for overall crime at both margins are negative. At the extensive margin, the effect is driven by drug and alcohol related crime. At the intensive margin, the effect is driven by both drug and alcohol related crime and traffic crime. Interestingly, we find significant incapacitation effects for both those with and without previous convictions. Taken together, our DiD and IV estimates imply that there are significant incapacitation effects associated with military service.

5.3. Difference-in-Difference Post Service Results

For completeness, we also use our DiD framework to estimate the post-service effect in our matched sample. We estimate an equation similar to Equation (2) after first redefining the two time periods. The pre-service period is defined as crime committed between the ages of 15 and 17. The post-service period is crime committed between the ages of 23 and 30.

DiD estimates of the post service crime effect at the extensive margin are shown in Panel A of Table 13. The post-service period intensive margin effects are shown in Appendix Table 4. The estimated post-service effects for Any Crime, Weapons, Violent, Traffic, and Other are all close to zero and insignificant. The coefficient associated with theft, however, is positive, significant and large, with an effect size of 85%. However, we also see a significant decrease in drug and alcohol related crime. In comparison, the OLS estimates presented in Panel B of Table 13 all show large protective effects. These are clearly biased, despite the use of a number of background control variables (including lagged crime) and test-day information. We argue that our DiD goes a long way to remove these large biases.[40]

So, why are not all of the DiD estimates large and positive for cohorts tested between 1997 and 2001? First, crime in Sweden is trending down at the time when these men are in their most crime prone years. Second, the experience of military service (i.e. the treatment) is undergoing change. As fewer young men do service, then the more active they are kept doing meaningful tasks. Also, the professional officer to conscript ratio rose. These and other changes may have improved the experience of military service. Third, since the service rate is falling substantially during this period, those who serve are more positively selected. That is, the marginal conscripts in our DiD and IV exercises are not of the same quality. When the effect of military service is heterogeneous (as we demonstrated above), and when service has a more detrimental effect on those from low SES and/or high crime backgrounds, then as the quality of the marginal conscript goes up, the detrimental effect should go down.

We illustrate this idea in Panel C of Table 13. Here, we show our extensive margin DiD post-service effects broken down by quartiles of our estimated propensity score for

[40] Because the difference-in-difference specification yields substantially different results for crime compared to OLS (with controls), we do not study the non-crime labor market outcomes for this matched sample, as there is no pre-period that we can define.

service.[41] Those in quartile 4 have observable characteristics that lead to a high probability of service, while those in quartile 1 have characteristics that yield a low probability of service. The positive effect of military service on post-service theft is driven by men in the lowest quartiles, and mainly by those in quartile 1. This pattern is even clearer at the intensive margin where the effect on theft changes monotonically from zero to quite large as we move down through the quartiles. At the same time, men in quartile 1 receive none of the beneficial reduction in drug and alcohol crime that we see for the other men.

6. Discussion of Potential Mechanisms Including the Role of Peers

Military service affects the lives of young men in many different ways. In our paper, we have shown that it significantly lowers the number of sick days and disability claims among all men who serve regardless of social background. This may be due to the additional physical activity and training young men receive in the military. We also saw significant and positive income effects for young men from advantaged social backgrounds. This finding echoes that of Grönqvist and Lindqvist (2016), who show that officer training can raise the probability of becoming a manager later in life and improve wages.[42] They argue that officer training improves leadership-specific human capital. They also find positive effects on educational attainment among those who take officer training and argue that this 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 of young men at a critical stage in their life, when they are actively choosing whether to pursue higher education.

Unfortunately, we also see that military service has a number of negative effects, especially on those from less advantaged backgrounds. They perform worse in the labor market and are more engaged in crime, despite being incapacitated while conscripted. How

[41] Intensive margin results are reported in Panel C of Appendix Table 4.
[42] They study cohorts tested between 1970 and 1988. At this time, all men who were fit were required to serve.

does service generate such negative effects? Could negative peer influence be part of the explanation?

To study this potential mechanism, we take our IV sample and group those who actually serve into "units". These are not the actual units (or platoons) in which they serve, as we do not have access to data with platoon identifiers.[43] Instead, our units are created by grouping men by test year, regiment and rank. There are 114 regiments in our data and four ranks – private, corporal, sergeant and 2nd lieutenant. We drop units with less than 10 members to allow for more accurate measures of average peer characteristics within a unit – namely pre-service crime. The median unit size is 142; the mean is 162 and the maximum is 589. Although, these are not a conscript's true platoon mates, this is the pool from which platoon mates are drawn.

For each conscript, we then calculate the leave-out average pre-service crime rate of all other men in his unit; that is, this average is based solely on the pre-service crime of each young man's peers, excluding himself. Figure 6 shows the distribution of the leave-out average pre-service number of crimes across units, reported separately by rank. This figure clearly shows that pre-service crime is 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 fathers with nine or less years of schooling, compared to 19 percent for corporals, and 12 percent for sergeants and 2nd lieutenants. Thus, one unintentional side-effect of the recruitment and placement process is that high crime, low SES men are concentrated together into smaller units, with intensive exposure over a long period of time.

But could this exposure then lead to peer effects in crime? In Table 14, we estimate potential peer effects by regressing own post-service crime (at the ages 23-30) on the leave-

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

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. The first specification looks at only the baseline relationship between peer criminal history and an individual's post-service crime. The remaining three columns interact peer criminal history with whether the individual has a low educated father, has a criminal history themselves, and is a private or corporal, respectively. Columns (1) – (4) present the extensive margin, while Columns (5) – (8) present the intensive margin. The estimated peer effect for men from advantaged backgrounds (high educated fathers or no criminal history) or with higher military ranks is zero; exposure to peers with a criminal history does not increase post-service crime for individuals with a low risk of crime to start with. In contrast, the results in Table 14 are indicative of strong peer influences for individuals from disadvantaged backgrounds; increased exposure to peers with a criminal history prior to service is associated with higher post-service crime for conscripts assigned to lower military ranks, from lower SES households, and especially, with a criminal history prior to service themselves. 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 likelihood of post service crime by an additional three percentage points. In this way, peer effects appear to be reinforcing in nature – exposure to peers with a criminal history reinforces the criminal path that individuals are already on.[44] Such non-linear peer effects imply that how conscripts are allocated to a unit can affect post-service crime. Thus, one way

[44] This is consistent with the findings of reinforcing peer effects in juvenile correctional facilities by Bayer, Hjalmarsson, and Pozen (2009), though those were crime-specific in nature.

to limit the potential negative (unintended) effects of military service may be to not group all "bad apples" together.[45]

Taken together, the concentration of high crime men into the same units of service and potential strong peer effects make negative peer effects one plausible mechanism behind the negative effects that we find for low SES men. Quantifying this effect, however, is quite difficult, since 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. Not only is the composition of the counterfactual peer group difficult to identify, but also the intensity of exposure – military service not only changes the composition of peers but also the intensity of the peer interactions. In this way, even if the average peer characteristics are not that different, peer effects of service could be quite strong since the nature of the interactions has changed.

Of course, there are other potential mechanisms that could also be at work. In particular, we would like to point out the fact that our cohorts of men were leaving service and entering into an environment with rather high unemployment among young adults. One conjecture is that among low skilled workers, those who do not undertake military service are able to establish themselves on the labor market more quickly. As we saw in Table 10, the marginal conscript experiences more unemployment as a young adult.

Finally, it has been suggested that a desensitization to violence and weapons can exacerbate post-service crime. In an attempt to get at this potential channel, we classify individuals' service categories as combatant (e.g. infantry soldier) versus noncombatant (e.g. cook). For the sample that serves, we then run OLS regressions of post-service crime on combatant status, with and without the full set of observable controls.[46] In the raw data, we find that individuals who serve in combat positions are less likely to commit post-service

[45] 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.
[46] Available from the authors upon request.

crime; with the full set of observable controls, there are no significant differences in post-service crime rates. One clearly cannot assign a causal interpretation to these results, as service category is not randomly assigned; however, given these findings, it is hard to believe that desensitization to violence (via combat training) is the mechanism underlying the large effect of service on post-service crime.[47]

7. Conclusion

With the end of the Cold War, numerous countries in Europe abolished mandatory conscription. With no imminent military threat, and with the security of NATO or EU membership, it became hard for politicians to both justify the financial costs of such a large-scale national policy and to 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 the 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. Despite providing the first evidence that individuals are incapacitated from committing crimes while conscripted (in both the IV design and difference in difference analysis), we find that conscription increases post-service crimes from ages 23 to 30 at both the extensive and intensive margins and across

[47] Note that we cannot use our IV specifications on combat and noncombat samples; first stage f-statistics are not large enough to justify such a specification.

a number of crime categories, including violent crimes and thefts. Thus, any long term crime reducing effects of incapacitation are more than offset by other ways in which the military negatively affects crime. In addition, these detrimental effects of service are driven by relatively 'high risk' populations with respect to future crime (i.e. those with a criminal history prior to service and/or from low socioeconomic status backgrounds). 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. We find little evidence in support of a desensitization to weapons and violence channel, but cannot rule out a weaker labor market attachment as an explanation of the results. Unfortunately, regardless of the mechanism, these results contradict the idea that military service may be a way to 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 improves the health of all young men and can have other positive post-service effects for populations at low-risk for crime. In the instrumental variable analysis, there is no 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, our supplementary difference-in-difference matching analysis suggests that service can lead to a long lasting reduction of drug and alcohol related crime. But, this is for a positively selected sample, with low crime rates to start with. Even in this sample, however, there is still evidence of a detrimental effect of service on post-service crime (mainly property crime) when zooming in on the lower end of the distribution.

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.

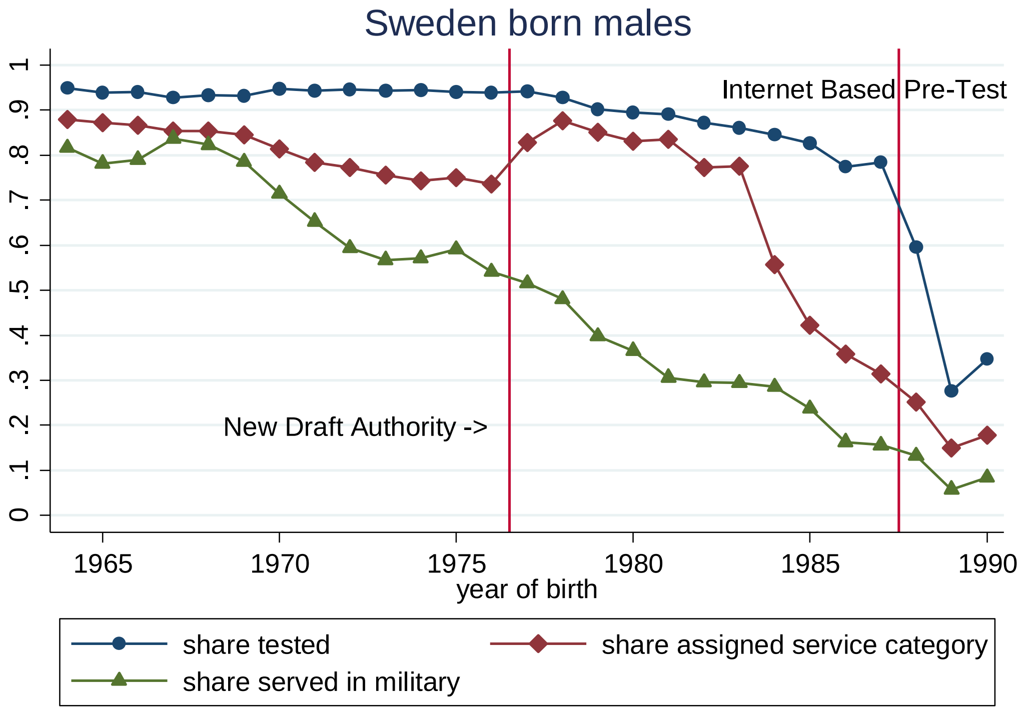
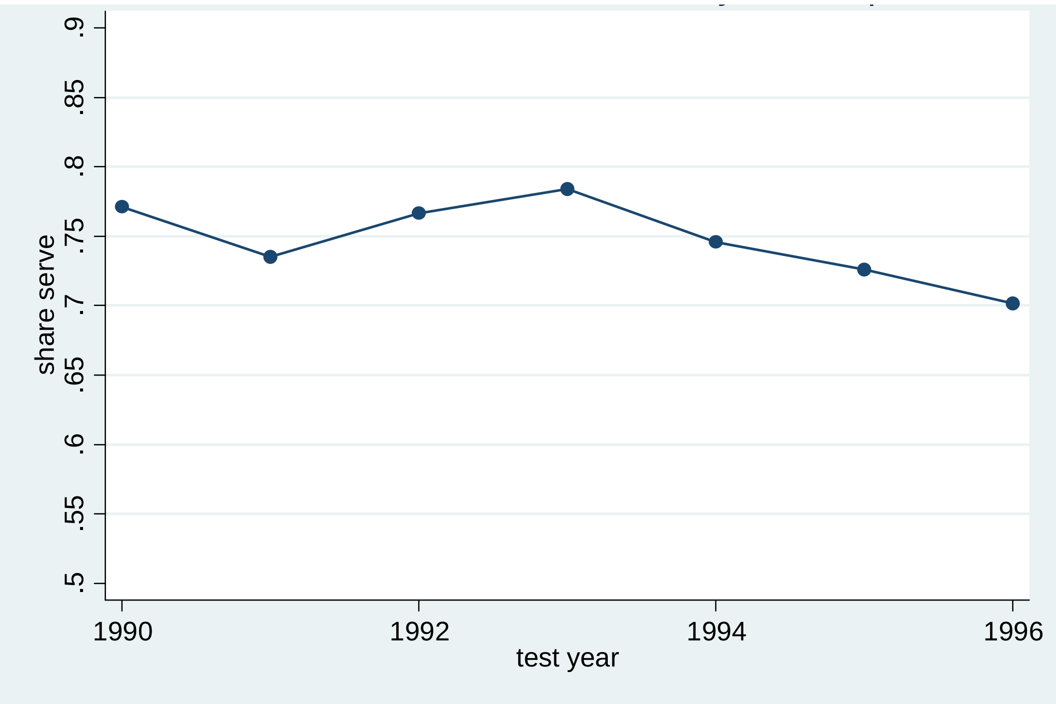
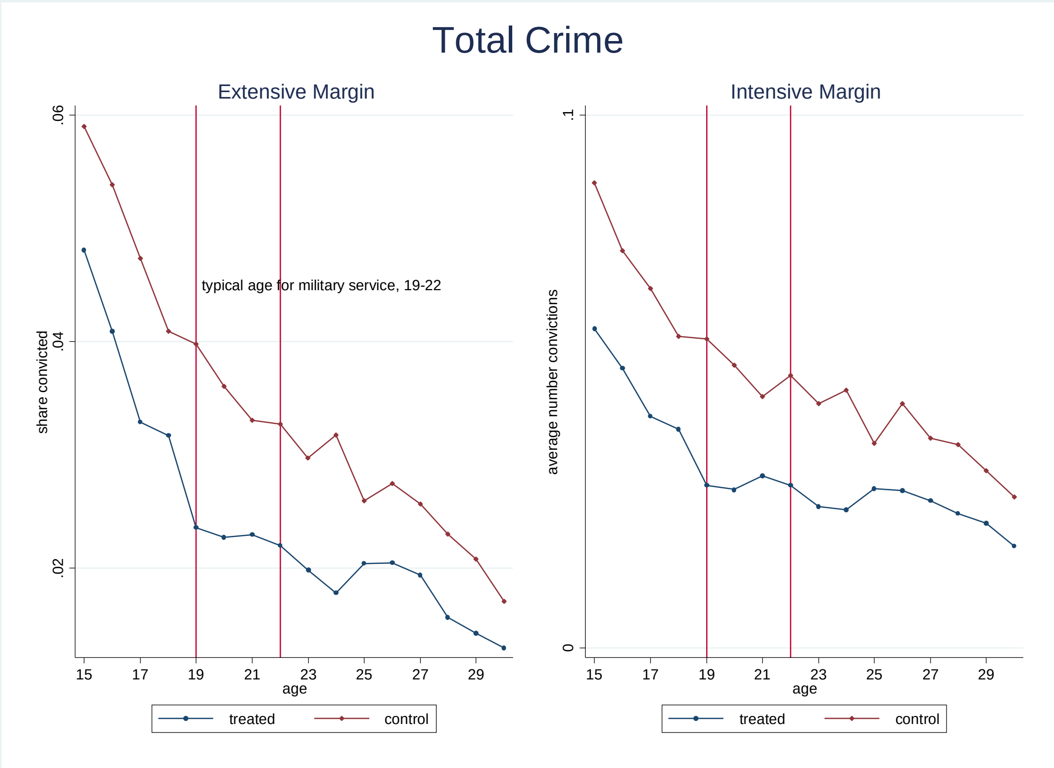


Figure 2. Share Serve from 1990 to 1996 Test Cohorts



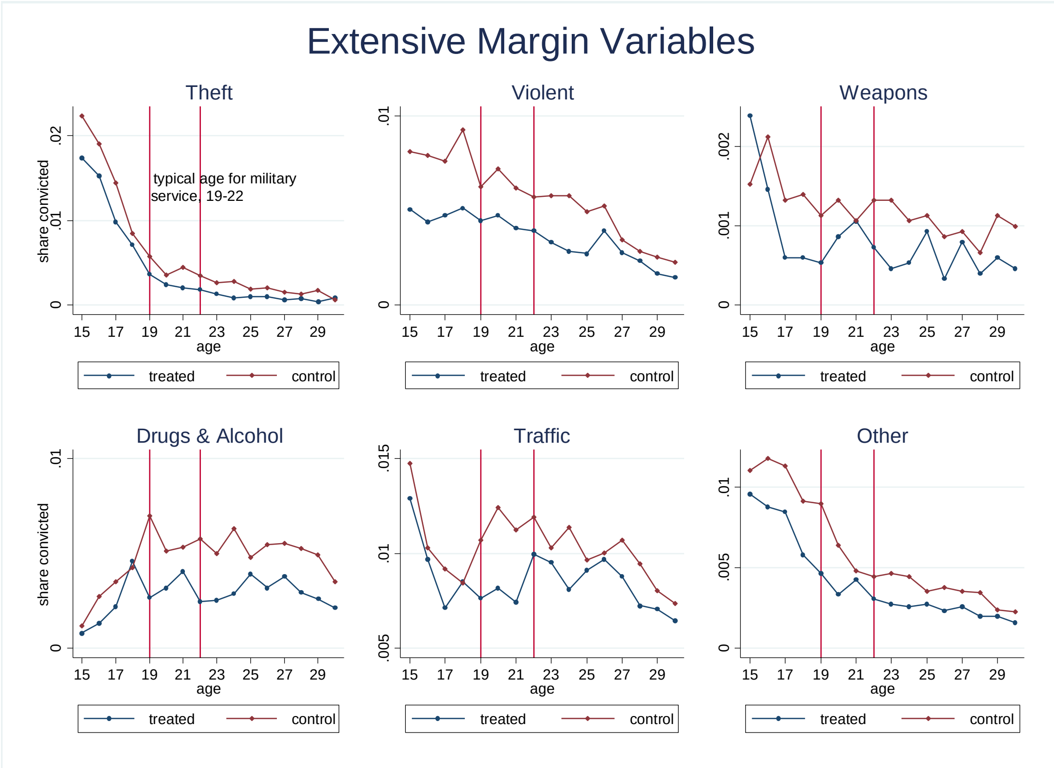
Note – This graph is based on the sample used in the instrumental variable analysis. That is, it is the share that serve of the sample of non-immigrant males who tested in 1990 to 1996 and were eligible to serve. For more details on the sample criteria, see Appendix Table 1.

Figure 3. Crime Trends by Age for Treated and Controls in our Matched Difference-in-Difference Sample.



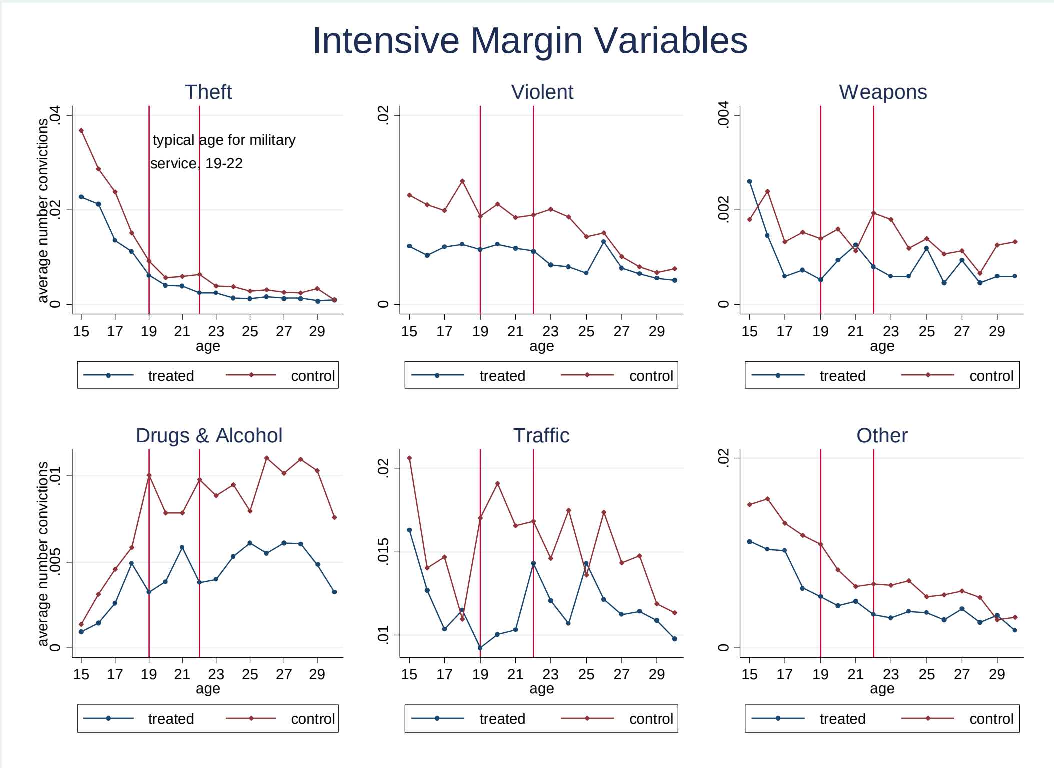
Note – This graph is based on our matched DiD sample, which includes men tested between 1997 and 2001.

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



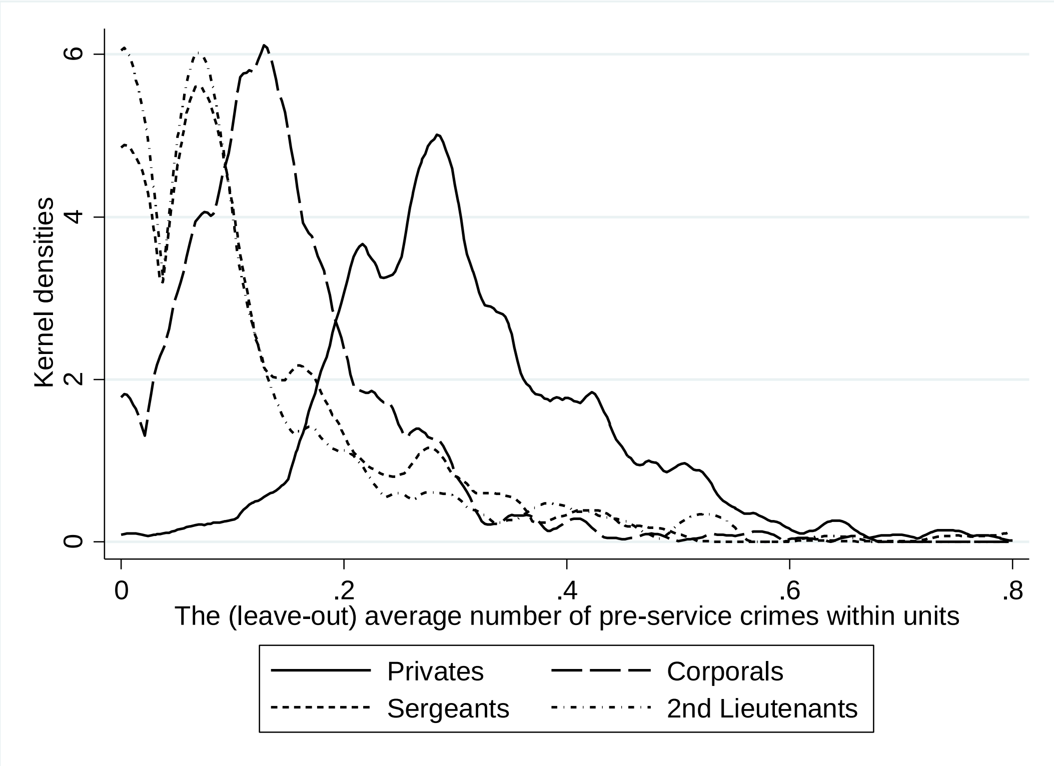
Note – This graph is based on our matched DiD sample, which includes men tested between 1997 and 2001.

Figure 5. Trends in Crime Type by Age for Treated and Controls in our Matched Difference-in-Difference Sample, Intensive Margin.



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

Figure 6. 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

Variable	All Individuals (N = 168,818) Mean	SD	Service = 1 (N = 126,550) Mean	SD	Service = 0 (N = 42,268) Mean	SD
military service (tax records)	0.75	0.43	1.00	0.00	0.00	0.00
high service rate officiator	0.50	0.50	0.56	0.50	0.35	0.48
test_year	1992.76	1.98	1992.71	1.96	1992.88	2.03
birth_year	1974.56	2.03	1974.55	2.01	1974.62	2.07
Pre-test day controls						
Any crime < 18?	0.13	0.34	0.13	0.34	0.15	0.35
schooling father	11.14	2.65	11.16	2.63	11.08	2.73
schooling mother	11.55	2.35	11.57	2.32	11.49	2.42
income father	12.32	0.40	12.32	0.40	12.30	0.41
income mother	11.87	0.39	11.87	0.38	11.85	0.40
2 year school?	0.18	0.39	0.18	0.38	0.19	0.39
3 year school?	0.76	0.43	0.77	0.42	0.72	0.45
Test Day Controls						
Height	179.75	6.44	179.77	6.41	179.66	6.50
Weight	71.59	10.53	71.65	10.48	71.40	10.67
Bmi	22.14	2.89	22.15	2.88	22.10	2.93
Ability Score	5.17	1.84	5.22	1.80	5.05	1.94
Physical Capacity Score	6.26	1.45	6.35	1.43	5.98	1.48
Crime Outcomes						
Any crimes 23-30?	0.10	0.31	0.10	0.30	0.11	0.32
# crimes 23-30	0.31	2.33	0.28	2.10	0.40	2.91
Any weapons 23-30	0.01	0.07	0.01	0.07	0.01	0.08
Any violent 23-30	0.02	0.15	0.02	0.15	0.03	0.16
Any traffic 23-30	0.06	0.25	0.06	0.24	0.07	0.25
Any theft 23-30	0.01	0.11	0.01	0.11	0.02	0.13
Any other 23-30	0.02	0.15	0.02	0.15	0.03	0.17
Any drugs 23-30	0.02	0.13	0.01	0.12	0.02	0.14
# weapons 23-30	0.01	0.21	0.01	0.17	0.01	0.30
# violent 23-30	0.05	0.44	0.04	0.41	0.06	0.52
# traffic 23-30	0.13	1.00	0.12	0.90	0.16	1.25
# theft 23-30	0.04	0.60	0.03	0.55	0.05	0.74
# other 23-30	0.05	0.44	0.04	0.42	0.06	0.52
# drugs 23-30	0.05	0.59	0.04	0.55	0.06	0.70

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

Table 2. Raw Comparison of Pre-Characteristics Across High Service Rate Officiator Assignment

	High Service Rate Officiator = 1 (N=85,138)		High Service Rate Officiator =0 (N=83,668)			
Variable	Mean	Std.	Mean	Std.	Difference	% Difference
military service	0.83	0.38	0.67	0.47	0.16	18.8
test_year	1993.04	2.03	1992.47	1.88	0.56	0.0
birth_year	1974.86	2.10	1974.27	1.91	0.59	0.0
any crime < 18	0.14	0.34	0.13	0.34	0.01	4.9
schooling father	11.33	2.64	10.95	2.65	0.38	3.4
schooling mother	11.71	2.33	11.38	2.35	0.32	2.7
income father	12.33	0.42	12.30	0.38	0.03	0.3
income mother	11.90	0.39	11.83	0.38	0.07	0.6
2 year shcool?	0.16	0.37	0.20	0.40	-0.04	-22.4
3 year school?	0.77	0.42	0.74	0.44	0.03	4.5
height	179.74	6.43	179.75	6.44	-0.01	0.0
weight	71.55	10.49	71.63	10.56	-0.08	-0.1
bmi	22.13	2.88	22.15	2.90	-0.02	-0.1
ability score	5.22	1.83	5.13	1.85	0.09	1.7
physical capacity score	6.29	1.36	6.22	1.53	0.08	1.2

Table 3. Test of Random Assignment to Officiators

	(1)	(2)	(3)	(4)	(5)	(6)	(7) Unexplained High Service
	Dependent Variable: High Service Rate Officiator						
# (of 21) significant 5% (including missing dummies)	9	4	3	2	4	4	0
# (of 12) significant 5% (excluding missing dummies)	4	3	2	2	4	4	0
pvalue prechar (variables)	0	0.05	0.15	0.12	0.17	0.18	0.85
pvalue test day char (variables)	0.13	0.01	0.04	0.02	0.03	0.04	0.22
pvalue pre and test day char (variables)	0	0.12	0.12	0.1	0.12	0.15	0.37
county FE	no	Yes	yes	yes	yes	yes	no
test year FE	no	No	yes	yes	yes	yes	no
county x test year FE	no	No	no	yes	yes	yes	no
test office FE	no	No	no	no	yes	yes	no
test office x test year FE	no	No	no	no	no	yes	no
PreCharacteristics							
any crime < 18	0.02468***	0.00657*	0.00251	0.00227	0.00209	0.00203	0.00229
	[0.00814]	[0.00389]	[0.00262]	[0.00245]	[0.00249]	[0.00248]	[0.00314]
schooling father	0.00812***	0.00044*	-0.00039	0.00008	0.00006	0.00008	0.00009
	[0.00125]	[0.00025]	[0.00027]	[0.00020]	[0.00019]	[0.00019]	[0.00050]
schooling mother	0.00330***	0.00218***	0.00053	-0.00001	-0.00003	-0.00003	-0.00001
	[0.00117]	[0.00067]	[0.00040]	[0.00034]	[0.00033]	[0.00033]	[0.00047]
income father	0.00226	-0.00979***	-0.00118	-0.00244	-0.00207	-0.00215	-0.00235
	[0.01944]	[0.00350]	[0.00182]	[0.00158]	[0.00163]	[0.00161]	[0.00458]
income mother	0.09168***	0.00272	-0.00024	0.00171	0.00136	0.00142	0.00178
	[0.01633]	[0.00216]	[0.00201]	[0.00150]	[0.00149]	[0.00149]	[0.00415]
2 year school?	-0.05682*	-0.04488***	0.00919**	0.00422*	0.00457**	0.00418**	0.00404
	[0.03109]	[0.01374]	[0.00363]	[0.00225]	[0.00214]	[0.00203]	[0.01103]
3 year school?	-0.01375	0.01969*	0.01340***	0.00523*	0.00579**	0.00562**	0.00518
	[0.02486]	[0.01090]	[0.00476]	[0.00267]	[0.00252]	[0.00248]	[0.00959]
Test Day Characteristics							
height	-0.00017	-0.00016	-0.00042	-0.00093	-0.00097	-0.00105	-0.00091

	[0.00219]	[0.00075]	[0.00077]	[0.00077]	[0.00075]	[0.00076]	[0.00089]
weight	-0.00056	0.00019	0.00069	0.00123	0.00129	0.0014	0.00122
	[0.00256]	[0.00094]	[0.00098]	[0.00095]	[0.00093]	[0.00094]	[0.00110]
bmi	0.00265	0.00071	-0.00216	-0.00438	-0.00452	-0.00486	-0.00434
	[0.00768]	[0.00334]	[0.00329]	[0.00313]	[0.00308]	[0.00309]	[0.00355]
ability score	-0.00379	-0.00434***	-0.00173*	-0.00127**	-0.00132**	-0.00129**	-0.00127
	[0.00255]	[0.00127]	[0.00097]	[0.00057]	[0.00055]	[0.00054]	[0.00106]
physical capacity	0.0053	-0.00084	0.00337*	0.00148***	0.00144**	0.00138**	0.0014
	[0.00906]	[0.00208]	[0.00187]	[0.00056]	[0.00054]	[0.00056]	[0.00299]
Observations	168806	168806	168806	168806	168806	168806	168806
R-squared	0.01	0.62	0.66	0.75	0.75	0.75	0

Robust standard errors (clustered at officiator)

The dependent variable in column (7) is the residual from a regression of assignment to a high service rate officiator on test year * county fixed effects, i.e. the variation in officiator assignment not explained by geographical and test year fixed effects.

Table 4. First Stage Regressions of Military Service on Assignment to High Service Rate Officiator

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Dependent Variable = military_service_tax_record							
High Service Rate Officiator	0.15513***	0.06859***	0.10229***	0.07933***	0.07780***	0.07713***	0.07686***	0.07596***
	[0.01999]	[0.00931]	[0.01166]	[0.01398]	[0.01387]	[0.01405]	[0.01403]	[0.01385]
F-statistic	60	54	77	32	31	30	30	30
county FE	no	yes	yes	yes	yes	yes	yes	yes
test year FE	no	no	yes	yes	yes	yes	yes	yes
county x test year FE	no	no	no	yes	yes	yes	yes	yes
test office FE	no	no	no	no	yes	yes	yes	yes
test office x test year FE	no	no	no	no	no	yes	yes	yes
pretest day characteristic	no	no	no	no	no	no	yes	yes
test day variables	no	no	no	no	no	no	no	yes
Observations	168806	168806	168806	168806	168806	168806	168806	168806
R-squared	0.03	0.05	0.06	0.07	0.07	0.07	0.07	0.08

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

Table 5. Baseline OLS and IV Estimates for Overall Crime

	(1) OLS	(2) OLS	(3) OLS	(4) IV	(5) IV
Panel A: Dependent Variable = Any Crimes from Age 23-30 (Mean = 0.10)					
military_service	-0.01233***	-0.01661***	-0.00663***	0.08908*	0.08072*
	[0.00283]	[0.00277]	[0.00220]	[0.04779]	[0.04478]
Panel B: Dependent Variable = # Crimes from Age 23-30 (Mean = 0.31)					
military_service	-0.12176***	-0.13289***	-0.06648***	0.55856**	0.54746**
	[0.01775]	[0.01965]	[0.01682]	[0.25192]	[0.22346]
First Stage F-Statistic				32	31
county x test year fixed effects	no	yes	yes	yes	yes
Test office Fixed Effects	no	no	yes	no	yes
Pre-test and Test day Controls	no	no	yes	no	yes

Columns (1) – (3) present the results of regressing crime from 23-30 (at the extensive margin in Panel A and the intensive margin in Panel B) on military service and the indicated set of controls. Note that county x test year fixed effects implies the inclusion of 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 (4) and (5) instrument for military service with assignment to a high service rate officiator. Robust standard errors, clustered by county in columns (1) - (3) and officiator in columns (4)-(5). *** 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
extensive margin 23-30							
Baseline	0.08908*	0.01650**	0.04934***	0.02943	0.01967	0.01886	0.01664
	[0.04779]	[0.00823]	[0.01609]	[0.03362]	[0.01322]	[0.02053]	[0.01527]
+ <18 crime specific control	0.08053*	0.01597*	0.04881***	0.02308	0.01898	0.02019	0.01566
+ test office, pre testday and test day controls	0.08072*	0.01550*	0.04691***	0.02309	0.01876	0.01959	0.01463
Mean Dependent Variable	*0.10*	*0.006*	*0.023*	*0.064*	*0.013*	*0.025*	*0.016*
intensive margin 23-30							
Baseline	0.55856**	0.01272	0.10193**	0.05209	0.17040***	0.10175*	0.11969**
	[0.25192]	[0.02072]	[0.04728]	[0.15086]	[0.05596]	[0.05626]	[0.05645]
+ <18 crime specific control	0.50565**	0.01135	0.10050**	0.02744	0.16752***	0.10542*	0.11429**
+ test office, pre testday and test day controls	0.51767**	0.00879	0.08812*	0.05188	0.15907***	0.11326**	0.11413**
Mean Dependent Variable	*0.31*	*0.01*	*0.046*	*0.13*	*0.038*	*0.046*	*0.047*

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, and is equivalent to the specification presented in column (4) of Table 5. These controls are also included in all other regressions. All specifications instrument for military service with assignment to a high service rate officiator. Just the coefficient on military service is presented. For the baseline specifications, the first stage F-statistic is 32. It remains at 32 when controlling for having at least one conviction in the crime category that corresponds to the dependent variable, and is equal to 31 when adding the full set of controls.

Table 7. Falsification Test: Military Service and Pre-Service Crime

	Dependent Variable:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
extensive margin: convictions less than age 18							
Military Service	0.05208	0.00829	0.00352	0.05467	0.0111	-0.01838	0.0069
	[0.05100]	[0.00880]	[0.01421]	[0.03343]	[0.02463]	[0.02462]	[0.00603]
Mean Dependent Variable	*0.13*	*0.006*	*0.019*	*0.055*	*0.059*	*0.038*	*0.004*
intensive margin: convictions less than age 18							
Military Service	0.23332	0.01465	0.007	0.03298	0.18977	-0.02942	0.01834
	[0.20724]	[0.01850]	[0.03701]	[0.04871]	[0.12263]	[0.05111]	[0.01159]
Mean Dependent Variable	*0.32*	*0.013*	*0.028*	*0.078*	*0.13*	*0.059*	*0.005*

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. N = 168,806 (corresponding to 1990-1996 test years). Presents the results for estimating the baseline regression (test year dummies, county dummies and test year x county dummies). The dependent variable is having at least one conviction or the number of convictions in the crime category listed at the top of column prior to age 18, i.e. before military service. All specifications instrument for military service with assignment to a high service rate officiator. Just the coefficient on military service is presented. The first stage F-statistic is 32.

Table 8. IV Estimates of Post-Service Crime Effect: Heterogeneity by Criminal History

	Dependent Variable:						
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Panel A: extensive margin from age 23 to 30							
Sample: At least one crime < 18 (n = 22590)	0.22203**	0.08171	0.10332*	0.09591	0.12952**	0.09816	0.07183
	[0.10382]	[0.05014]	[0.05762]	[0.07425]	[0.06512]	[0.08492]	[0.06502]
Mean Dep Variable	*0.24*	*0.023*	*0.072*	*0.15*	*0.048*	*0.071*	*0.06*
Sample: No crimes < 18 (n=146216)	0.05617	0.00243	0.03660**	0.0135	-0.00372	0.00058	0.00336
	[0.03987]	[0.00656]	[0.01497]	[0.03232]	[0.01220]	[0.01522]	[0.01111]
Mean Dep Variable	*0.082*	*0.008*	*0.015*	*0.052*	*0.008*	*0.017*	*0.009*
Panel B: intensive margin from Age 23 to 30							
Sample: At least one crime < 18 (n = 22590)	2.79733*	0.10884	0.41801*	0.62365	0.77635**	0.28647	0.58402**
	[1.46966]	[0.12904]	[0.22971]	[0.71503]	[0.38001]	[0.23522]	[0.28557]
Mean Dep Variable	*1.18*	*0.047*	*0.17*	*0.42*	*0.17*	*0.16*	*0.21*
Sample: No crimes < 18 (n=146216)	0.0344	-0.0099	0.0292	-0.08491	0.03978	0.05128	0.00896
	[0.17234]	[0.01456]	[0.03219]	[0.08382]	[0.03790]	[0.04293]	[0.04037]
Mean Dep Variable	*0.18*	*0.004*	*0.026*	*0.08*	*0.017*	*0.028*	*0.021*

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. Military Service is instrumented for with the high service rate officiator dummy. F-statistic on first stage regressions: 27 for sample with criminal history and 30 for sample without criminal history. Each regression controls for test year dummies, county dummies and test year x county dummies.

Table 9. IV Estimates of Post-Service Crime Effect: Heterogeneity by Father Schooling

	Dependent Variable:						
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Panel A: extensive margin from age 23 to 30							
Father School <= 9 years (N = 43904)	0.10682*	0.02667*	0.08606**	0.02519	0.04018	0.03325	0.05055**
	[0.06044]	[0.01458]	[0.03580]	[0.04161]	[0.02724]	[0.03382]	[0.02493]
Father School > 9 & <= 12 years (N=90346)	0.07447	0.01188	0.03778	0.01162	0.00817	0.03095	-0.00138
	[0.05435]	[0.01345]	[0.02400]	[0.03915]	[0.01797]	[0.02803]	[0.02323]
Father School > 12 years (n = 34466)	0.10481	0.01014	0.01864	0.08786	0.01251	-0.03635	0.00603
	[0.08351]	[0.01164]	[0.03059]	[0.06685]	[0.01802]	[0.02762]	[0.02115]
Panel B: intensive margin from Age 23 to 30							
Father School <= 9 years (N = 43904)	1.12559**	0.04135	0.14591	0.39832	0.26554**	0.08606	0.18841**
	[0.44410]	[0.04246]	[0.09486]	[0.28446]	[0.11226]	[0.08601]	[0.09039]
Father School > 9 & <= 12 years (N=90346)	0.3969	-0.00344	0.10325	-0.13782	0.18118**	0.13810*	0.11564
	[0.35510]	[0.03283]	[0.07445]	[0.19729]	[0.08263]	[0.08049]	[0.09713]
Father School > 12 years (n = 34466)	-0.00161	0.00259	0.01807	-0.04469	-0.01394	0.03356	0.0028
	[0.23377]	[0.01924]	[0.06835]	[0.10082]	[0.03610]	[0.07399]	[0.07810]

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. Military Service is instrumented for with the high service rate officiator dummy. F-statistic on first stage regressions: 40.6 for father school <=9, 24 for father schooling >9 and <=12, 22 for father shcooling > 12. Each regression controls for test year dummies, county dummies and test year x county dummies.

Table 10. 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 = log average income 30-34*				
Military Service	-0.05729	0.0034	-0.05884	-0.04076	-0.06957	0.04554	-0.29046	0.12098	-0.29374**	0.20616**
	[0.07003]	[0.10675]	[0.07487]	[0.07045]	[0.09449]	[0.07855]	[0.23359]	[0.07394]	[0.13010]	[0.09475]
Mean Dep Variable	0.42	0.22	0.45	0.27	0.47	12.38	12.25	12.4	12.35	12.4
	Dep Var = any years unemployment benefit 23-34					*Dep Var = # years unemployment benefit 23-34*				
Military Service	0.019	0.20030**	-0.02611	0.14210*	-0.03593	0.0748	0.26205	0.00124	0.59495*	-0.16777
	[0.06547]	[0.09499]	[0.07547]	[0.07570]	[0.08121]	[0.28268]	[0.54141]	[0.33678]	[0.31594]	[0.35119]
Mean Dep Variable	0.46	0.55	0.45	0.5	0.044	1.55	2.04	1.48	1.79	1.47
	Dep var = any welfare from 23-34					*Dep Var = months welfare 23-34*				
Military Service	0.0486	0.12019	0.02677	0.10136	0.0241	1.75942*	8.28290**	0.31953	4.98632**	0.22014
	[0.04615]	[0.10392]	[0.04122]	[0.07911]	[0.05258]	[0.99210]	[3.81385]	[0.77772]	[2.27897]	[1.06789]
Mean Dep Variable	0.13	0.25	0.11	0.15	0.12	1.56	4.2	1.15	1.98	1.41
	Dep var = any_disability pension 23-34					*Dep Var = # years with full/partial disability pension*				
Military Service	-0.02178**	-0.06129**	-0.01564*	-0.02628	-0.0186	-0.10712*	-0.13709	-0.11117**	-0.07816	-0.11637
	[0.01041]	[0.02876]	[0.00946]	[0.02171]	[0.01446]	[0.05570]	[0.16275]	[0.05527]	[0.11669]	[0.07078]
Mean Dep Variable	0.012	0.024	0.01	0.014	0.011	0.057	0.113	0.048	0.067	0.053
	Dep Var = # sick days 23-34									
Military Service	-59.734***	-107.672*	-52.910***	-44.001	-65.736***					
	[21.16941]	[55.38455]	[19.99828]	[42.55824]	[24.10553]					
Mean Dep Variable	48.8	83.7	43.5	59.1	45.2					

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. First stage f-stats: 32 for whole sample, 27 for preservice crime = 1, 30 for no pre service crime, 41 for father <=9 years school, 27 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 assignment to high service rate officiator. For all dependent variables, N= 168,806 for all, 22,590 for criminal history, 146, 216 for no criminal history, 43,904 for less than 9 years father schooling, 124,902 for father schooling more than 9 years. The only exception is income, for which about 3000 individuals had missing income.

Table 11. IV Estimates of a Potential Incapacitation Effect: Crime at 19 and 20 for 18-year Old Testees

	Dependent Variable:						
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Panel A: extensive margin crime at age 19 and 20							
Sample: all 18 year old testees	-0.01972	0.00076	-0.02590*	0.01743	0.00541	-0.00299	-0.01086
(N = 138799)	[0.02825]	[0.00442]	[0.01497]	[0.02093]	[0.01302]	[0.01552]	[0.00801]
Sample: 18 year old testees with criminal history	-0.1185	0.02526	-0.05726	-0.10307*	0.06234	0.02823	0.00991
(N = 18558)	[0.11841]	[0.02034]	[0.05448]	[0.06046]	[0.05937]	[0.06804]	[0.02739]
Mean	*0.2*	*0.013*	*0.056*	*0.068*	*0.064*	*0.072*	*0.018*
Sample: 18 year old testees with no history	-0.00076	-0.00392	-0.02127*	0.04161*	-0.00472	-0.00815	-0.01527**
(N=120241)	[0.02988]	[0.00555]	[0.01115]	[0.02364]	[0.01179]	[0.01448]	[0.00741]
Mean	*0.056*	*0.002*	*0.009*	*0.021*	*0.012*	*0.018*	*0.003*
Panel B: intensive margin crime at age 19 and 20							
Sample: all 18 year old testees	-0.1156	0.00325	-0.03648	0.00157	-0.02085	-0.0537	-0.00939
(N = 138799)	[0.10013]	[0.00542]	[0.02308]	[0.02845]	[0.05068]	[0.03581]	[0.01337]
Sample: 18 year old testees with criminal history	-0.41004	0.04497	-0.04737	-0.22169**	-0.0592	-0.17721*	0.05046
(N = 18558)	[0.37057]	[0.04286]	[0.08579]	[0.09662]	[0.21162]	[0.10008]	[0.06420]
Mean	*0.52*	*0.02*	*0.094*	*0.12*	*0.16*	*0.11*	*0.03*
Sample: 18 year old testees with no history	-0.05525	-0.00473	-0.03555*	0.04922*	-0.01537	-0.02749	-0.02133**
(N=120241)	[0.06567]	[0.00761]	[0.01843]	[0.02878]	[0.03655]	[0.02635]	[0.00964]
Mean	*0.087*	*0.003*	*0.013*	*0.026*	*0.02*	*0.023*	*0.004*

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. To isolate incapacitation, this table focuses on the sample who took the test at age 18 (and likely served from ages 19-20) and looks at crime outcomes at age 19 and 20. As individuals do not serve for all of this period, however, these crime outcomes clearly include some pre- and post-service crime. Military Service is instrumented for with the high service rate officiator dummy. F-statistic on first stage regressions: 26 for all 18 year old testees, 24 for those with a criminal history, and 23 for those with no history. Each regression controls for test year dummies, county dummies and test year x county dummies.

Table 12. Difference-in-Difference Estimates of the Incapacitation Effect of Military Service Using Exact Service Dates

	Dependent Variable:						
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Panel A: extensive margin							
Matched sample	-0.0043*	0.0003	0.0005	-0.0015	-0.0007	-0.0001	-0.0037***
(N = 30,082)	[0.0022]	[0.0004]	[0.0011]	[0.0013]	[0.0010]	[0.0011]	[0.0010]
Mean	0.020	0.001	0.004	0.007	0.004	0.005	0.004
Matched sample with criminal history	0.0060	0.0016	0.0049	-0.0011	0.0034	0.0103	-0.0124**
(N = 3,153)	[0.0119]	[0.0028]	[0.0066]	[0.0072]	[0.0053]	[0.0065]	[0.0055]
Mean	0.066	0.005	0.017	0.020	0.011	0.015	0.015
Matched sample with no history	-0.0058***	0.0002	-0.0001	-0.0016	-0.0013	-0.0015	-0.0027***
(N = 26,929)	[0.0020]	[0.0003]	[0.0009]	[0.0012]	[0.0009]	[0.0010]	[0.0008]
Mean	0.015	0.000	0.003	0.005	0.002	0.003	0.002
Panel B: intensive margin							
Matched sample	-0.0085	0.0000	0.0015	-0.0066***	0.0014	-0.0001	-0.0046***
(N = 30,082)	[0.0053]	[0.0006]	[0.0019]	[0.0024]	[0.0027]	[0.0018]	[0.0014]
Mean	0.034	0.001	0.006	0.010	0.005	0.007	0.005
Matched sample with criminal history	-0.0230	-0.0002	0.0091	-0.0366**	0.0177	0.0075	-0.0205**
(N = 3,153)	[0.0367]	[0.0045]	[0.0142]	[0.0176]	[0.0152]	[0.0120]	[0.0102]
Mean	0.154	0.007	0.030	0.044	0.020	0.029	0.025
Matched sample with no history	-0.0071*	0.0001	0.0005	-0.0030*	-0.0008	-0.0011	-0.0027***
(N = 26,929)	[0.0039]	[0.0004]	[0.0013]	[0.0016]	[0.0024]	[0.0014]	[0.0010]
Mean	0.020	0.000	0.003	0.006	0.003	0.004	0.003

Standard errors [in brackets] are robust. *** 1%, ** 5%, * 10%. Criminal history == 1 if the individual has at least one conviction between the ages of 15 and 17

Table 13. Difference-in-Difference Estimates of Extensive Margin Post-Service Crime Effect for Men Who Were Tested 1997-2001, Baseline and by Propensity Score Quartile

	Dependent Variable: Extensive Margin						
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Panel A: BaselineDiD							
Matched sample	-0.0007	-0.0012	0.0002	-0.0001	0.0068***	-0.0003	-0.0053***
(N = 30,082)	[0.0045]	[0.0011]	[0.0021]	[0.0032]	[0.0025]	[0.0024]	[0.0018]
Mean	0.096	0.005	0.022	0.057	0.008	0.020	0.021
Panel B: OLS with controls							
Matched sample	-0.0139***	-0.0015*	-0.0064***	-0.0030	-0.0038***	-0.0057***	-0.0075***
(N = 30,082)	[0.0033]	[0.0008]	[0.0016]	[0.0026]	[0.0010]	[0.0016]	[0.0016]
Mean	0.096	0.005	0.022	0.057	0.008	0.020	0.021
Panel C: DiD by pscore quartile							
Quartile 4 (high probability to serve)	0.0090	-0.0030	0.0002	0.0049	0.0064	0.0024	-0.0056
(N = 5,912)	[0.0095]	[0.0021]	[0.0042]	[0.0066]	[0.0052]	[0.0049]	[0.0036]
Quartile 3	-0.0088	0.0010	-0.0000	-0.0089*	0.0010	-0.0029	-0.0070**
(N = 9,891)	[0.0076]	[0.0018]	[0.0036]	[0.0054]	[0.0043]	[0.0039]	[0.0030]
Quartile 2	0.0041	-0.0022	0.0012	0.0060	0.0099**	0.0008	-0.0059*
(N = 10,758)	[0.0079]	[0.0020]	[0.0037]	[0.0057]	[0.0044]	[0.0042]	[0.0032]
Quartile 1 (low probability to serve)	-0.0073	-0.0011	-0.0012	-0.0029	0.0161**	-0.0006	0.0005
(N = 3,521)	[0.0139]	[0.0036]	[0.0068]	[0.0104]	[0.0080]	[0.0080]	[0.0062]

OLS regressions include all controls including lagged crime. Standard errors [in brackets] are robust. *** 1%, ** 5%, * 10%.

Table 14. 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)	(7)	(8)
Unit pre-service crime	0.029	0.0079	0.024	-0.027	0.053**	-0.037*	0.007	-0.019
	[0.0194]	[0.0191]	[0.0213]	[0.0452]	[0.0250]	[0.0194]	[0.0251]	[0.0323]
Own pre-service crime = 1	0.115***	0.089***	0.115***	0.115***	0.454***	0.2442***	0.455***	0.454***
	[0.0037]	[0.0109]	[0.0037]	[0.0037]	[0.0249]	[0.0470]	[0.0249]	[0.0249]
Father education <= 9 years			-0.002				-0.042**	
			[0.00061]				[0.0205]	
Private or corporal = 1				-0.007				-0.023**
				[0.0050]				[0.0113]
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]	
Unit pre-service crime * Private or corporal = 1				0.068				0.083*
				[0.0502]				[0.0430]
Mean dependent variable	0.093	0.093	0.093	0.093	0.213	0.213	0.213	0.213
Mean unit crime	0.123	0.123	0.123	0.123	0.262	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	
If private or corporal = 1 / = 0				0.128/0.067				0.274/0.116
Observations	102,085	102,085	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.

Appendix Table 1. Sample Restrictions for Baseline Instrumental Variable Analysis

Sample Restriction	*Sample Size*
Non-immigrant males born 1964-1990, and tested 1990-1996	231,707
Excluding if missing county identifier	231,583
Keep only those who are *assigned to service*	190,520
Drop testees with officiators who see less than 100 cases in their test year	183,065
Drop those missing health group information or assigned to health groups that 'never' serve in that year	173,601
Drop those with unknown end year of service and those 23 or older in year finish service	168,818

This figure provides sample criteria used in creating the baseline sample used in the high service rate officiator instrumental variable analysis.

Appendix Table 2. Sensitivity Analysis of Baseline Specification to Standard Error Clustering and Alternative Instrument

	(1)	(2)	(3)	(4)	(5)
Panel A: Dependent Variable = Any Crimes from Age 23-30 (Mean = 0.10)					
military_service	0.08908*	0.08908**	0.08908	0.08908*	0.05707**
	[0.04779]	[0.04104]	[0.05673]	[0.04731]	[0.02613]
Panel B: Dependent Variable = # Crimes from Age 23-30 (Mean = 0.31)					
military_service	0.55856**	0.55856**	0.55856**	0.55856**	0.54830***
	[0.25192]	[0.24409]	[0.28332]	[0.26652]	[0.18881]
Instrument	high service officiator	high service officiator	high service officiator	high service officiator	leave out annual mean
Cluster Unit	officiator	county	2 way: officiator and test year	officiator x test year	officiator
Number of clusters	67	24	67 and 7	203	67
First Stage F-Statistic	32	116	45	39	79
county x test year fixed effects	yes	yes	yes	yes	yes
Test office Fixed Effects	no	no	no	no	no
Pre-test and Test day Controls	no	no	no	no	no

Columns (1) - (4) present the results of instrumenting for service with a high service rate officiator dummy while column (5) uses the leave out annual mean, i.e. the actual share of testees assigned to an officiator in a given year, excluding the candidate under consideration, who are assigned to service. 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 3. Post-Service (23-30) Crime for IV Incapacitation Analysis Sample

	Dependent Variable:						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
extensive margin: crimes 23-30							
Sample: all 18 year old testees	0.06037	0.00473	0.04644**	0.00923	0.00219	0.00984	0.00981
(N = 138799)	[0.04448]	[0.01096]	[0.01898]	[0.03285]	[0.01597]	[0.02344]	[0.01773]
Mean Dependent Variable	*0.1*	*0.005*	*0.023*	*0.063*	*0.013*	*0.024*	*0.016*
intensive margin: crimes 23-30							
Sample: all 18 year old testees	0.44435	-0.02081	0.10243*	0.02306	0.11297*	0.13449*	0.09222
(N = 138799)	[0.36139]	[0.02658]	[0.05771]	[0.18322]	[0.06258]	[0.06936]	[0.08653]
Mean Dependent Variable	*0.3*	*0.009*	*0.045*	*0.12*	*0.036*	*0.045*	*0.046*

Robust standard errors, clustered on officiator. *** 1%, ** 5%, * 10*. This table focuses on the sample who took the test at age 18 (and likely served from ages 19-20); i.e. the sample used to isolate incapacitation. But rather than looking at crimes comitted potentially during service, this table looks at post-service crimes (age 23-30) to demonstrate the comparability of the incapacitation sample to the entire sample. Military Service is instrumented for with the high service rate officiator dummy. F-statistic on first stage regressions: 26 for all 18 year old testees. Each regression controls for test year dummies, county dummies and test year x county dummies.

Appendix Table 4. Difference-in-Difference Estimates of Intensive Margin Post-Service Crime Effect for Men Who Were Tested 1997-2001, Baseline and by Propensity Score Quartile

	Dependent Variable: Intensive Margin						
	Any Crime	Weapons	Violent	Traffic	Theft	Other	Drugs/Alc
Panel A: Baseline DiD							
Matched sample	-0.0789***	-0.0035**	-0.0051	-0.0129	0.0198***	-0.0043	-0.0310***
(N = 30,082)	(0.0203)	(0.0016)	(0.0048)	(0.0089)	(0.0068)	(0.0052)	(0.0074)
Mean	0.262	0.008	0.040	0.104	0.017	0.034	0.059
Panel B: OLS with controls							
Matched sample	-0.1011***	-0.0039***	-0.0177***	-0.0202**	-0.0111***	-0.0153***	-0.0328***
(N = 30,082)	(0.0201)	(0.0014)	(0.0043)	(0.0085)	(0.0037)	(0.0040)	(0.0073)
Mean	0.262	0.008	0.040	0.104	0.017	0.034	0.059
Panel C: DiD by pscore quartile							
Quartile 4 (high probability to serve)	-0.0830**	-0.0039	-0.0007	-0.0156	0.0012	-0.0087	-0.0260**
(N = 5,912)	(0.0343)	(0.0026)	(0.0085)	(0.0136)	(0.0157)	(0.0086)	(0.0114)
Quartile 3	-0.0703**	-0.0000	-0.0044	-0.0161	0.0197	-0.0008	-0.0298***
(N = 9,891)	(0.0273)	(0.0025)	(0.0070)	(0.0121)	(0.0123)	(0.0070)	(0.0106)
Quartile 2	-0.0853**	-0.0054*	-0.0066	-0.0014	0.0251**	-0.0064	-0.0396***
(N = 10,758)	(0.0348)	(0.0031)	(0.0082)	(0.0152)	(0.0102)	(0.0103)	(0.0131)
Quartile 1 (low probability to serve)	-0.0881	-0.0074	-0.0098	-0.0390	0.0400*	-0.0001	-0.0226
(N = 3,521)	(0.0970)	(0.0066)	(0.0216)	(0.0437)	(0.0212)	(0.0206)	(0.0345)

OLS regressions include all controls including lagged crime. Standard errors [in brackets] are robust. *** 1%, ** 5%, * 10%.