Abstract:

Draft lottery data combined with Danish longitudinal administrative records show that military service can reduce criminal activity for youth offenders. For this group property crime is reduced for up to five years from the beginning of service, and the effect is not only a result of incapacitation. Among youth offenders we find no effect of service on violent crimes, educational attainment and unemployment, and we find negative effects on earnings. These results suggest that military service does not upgrade productive human capital directly, but rather impacts criminal activity through other channels, for example by changing the attitudes to criminal activity.
1. Introduction

Crime is costly for society and there is ongoing debate about how to reduce youth crime. The aim of this paper is to measure the effect of peacetime military service by conscription on the propensity to commit crime during and after service. Peace time conscription is widespread. For example, many NATO countries have peace time conscription. One of the objectives of conscription is to support democracy by improving civil-military relations and by educating youth by offering them a new chance in life, introducing them to other segments of the population, and informing them about important civil values, Sørensen (2000). By teaching obedience and discipline military service may also provide skills that are potentially directly relevant in the labor market and, thereby, make labor market activity more attractive relative to criminal activity. Furthermore, the fact that military service occupies the time of the conscripts while in service can potentially also contribute to reducing crime. Military service can thus potentially impact criminal behavior by incapacitation, by affecting productive human capital, and by socializing conscripts towards being better citizens, i.e. shaping their attitude towards criminal activity. Military service can, however, also enhance criminal behavior by delaying labor market entry and education thereby worsening labor market opportunities. What is more, training in the use of weapons may stimulate criminal activity. Finally, conscription is associated with close and long term interaction with new peers, and this can affect criminal behavior both positively and negatively depending on the quality of the peers.

The literature about the effect of peacetime military service on crime is sparse. Galiani et al. (2011) estimate the effect of military service by conscription in Argentina, during war as well as peace time, on the propensity to commit crime. Identifying the causal effect by exploiting the randomization of eligibility inherent in the draft lottery, they find that military service
increases the propensity to develop a subsequent criminal record and that service has a detrimental effect on subsequent labor market performance. Effects are more adverse for individuals having served during war time. A related strand of literature has examined the association between war veteran status and subsequent criminal activity, see MacLean and Elder (2007) for an overview. The evidence from this literature is mixed and seems to depend on the context. A number of papers show that military service can impact other important aspects of people’s lives. Angrist (1990) exploits the Vietnam War draft lottery to show that Vietnam veterans earn less than otherwise similar men who were not drafted. Follow-up studies have found earnings effects to be short lived, Angrist, Chen and Song (2011) and Angrist and Chen (2011), although the latter study finds that the GI Bill generated schooling gains for veterans. Angrist (1998) shows evidence that voluntary military service can have positive effects on post-service employment. Card and Cardoso (2012) show evidence that peace time conscription increases the earnings of low-skilled Portuguese men.

A range of studies have tried to quantify the effect of various policy initiatives on reducing crime. Some policy measures have obvious short-lived effects. For example, imprisonment takes the criminal out of criminal activity (at least outside the prison) and increased police effort also seems to lower criminal activity, Chalfin and McCrary (2013). Much of the previous evidence about how to reduce crime has focused on the effect of schooling and social policies. The literature about the effect of schooling is much too large to give a full account of here and we refer to Lochner (2011) who surveys effects of schooling and job training programs on crime. Schooling can generally affect crime directly by upgrading human capital and changing opportunity costs of crime or by occupying time and thereby reducing crime through incapacitation. It can also affect crime by changing the attitude towards criminal activity by affecting the psychic rewards, preferences for risk taking or
patience, or by affecting social interaction. There appears to be evidence that human capital upgrading has lasting effects. For example, in a recent paper Machin, Marie, and Vujic (2011) exploit an increase in the minimum schooling age from age 15 to 16 in 1972-73 in England and Wales and find that the effect of one year of additional schooling reduces property crime by 20-30% over the period 1972-1996. There is also evidence that incapacitation plays a role. Jacob and Lefgren (2003) examine juvenile crime rates on days when school is not in session with days when it is and find that crime rates are higher when school is not in session. Similarly, Luallen (2006) finds that crime rates are higher when teachers strike. A number of studies have also found that pre-school programs can have sizeable long run effects, Lochner (2011). Social policies may also affect the propensity to commit crime. For example, in a recent study Fallesen et al. (2012) investigate the effect of labor market programs activating unemployed workers on crime and find that activation reduces criminal activity significantly and that the effect is not only the result of incapacitation by reducing leisure hours since criminal activity is also reduced on weekends when leisure hours are not affected. Educational and social programs are, however, difficult to design so as to reach high risk groups such as youth offenders, and peace time military service by conscription may be a way of reaching such groups.

This paper focuses on the effect of peacetime military service on criminal activity. To identify the causal effect of military service we exploit the fact that all young men in Denmark upon reaching the age of 18 are liable to participate in a draft lottery for military service. Exploiting this source of randomization we ensure that we are not considering a particular type of young men in military service, i.e. our estimates are not plagued by self-selection into service. This research design is similar to the design applied by Galiani et al. (2011) and Angrist and coauthors. Our data cover an extract of the 1964 cohort, and are longitudinal so that we are
able to follow individuals from the year the individuals turn 16 and the next 20 years forward. The data set includes information about convictions, schooling, labor market attachment, earnings and family background. By observing pre-conscription convictions we are able to identify youth offenders and to estimate effects separately for this group. The longitudinal data also allow us to characterize the dynamic effects of military service on subsequent criminal activity.

Our results show that peace time military service lowers the propensity to commit property crime among youth offenders, i.e. young people who had committed crime before they entered military service. For this group property crime is reduced for up to five years from the beginning of service, and the effect is, therefore, not only a result of incapacitation while enrolled. Criminal activity in Denmark peaks at age 18 and is reduced to about half of this by age 25\(^1\) and most efforts to reduce crime target people aged 16-25. Our results suggest that military service can reduce crime for youth offenders for a significant fraction of the criminal intensive age interval. We do not find any effects on violent crimes. We speculate that this is because violent crimes are acts of impulse while property crimes often are the result of unsentimental planning, Jacob and Lefgren (2003). For the group of youth offenders we do not find any effects of military service on post-service educational attainment or on employment, but we find evidence suggesting that service leads to lower earnings for up to five years after completed service. These results suggest that military service does not upgrade productive human capital directly, but rather impacts criminal activity through other channels, for example by changing the attitude to criminal activity for this group.

\(^1\) This is documented by Statistics Denmark, [http://www.statbank.dk/statbank5a/default.asp?w=1301](http://www.statbank.dk/statbank5a/default.asp?w=1301). Similar patterns are well-known for other countries, e.g. Imai and Krishna (2004) and Grogger (1998).
Our study provides new evidence about the impact of peace-time military conscription on criminal activity. The only previous study of peace-time conscription on criminal activity, Galiani et al. (2011), found that service boosted subsequent criminal activity. We find that military service has no effect on crime for the vast majority of conscripts. Unlike Galiani et al. (2011) we are able to identify youth offenders and to examine whether effects are different for this sub-group who are at risk of continuing a criminal career. Our results suggest that this is indeed the case. Youth offenders who are randomized into military service have lower crime rates for up to five years from the beginning of service than youth offenders who are not in service. This new evidence suggests that peacetime military service by conscription may be a way to reach a high-risk group that is otherwise difficult to reach using other policy measures.

The next section describes the Danish military service and draft lottery as well as the data and the ability of the draft lottery number to predict military service. The following section briefly outlines the methodology. Section 4 presents the results and section 5 sums up and concludes the analysis.

2. Military Service and data

Military service

All Danish men upon reaching the age of 18 become liable for conscription and participate in a draft lottery for national service. The draft lottery takes place in connection with a conscription examination where participants are also subjected to a health examination and an IQ test. The IQ test is used for identifying individuals who are not eligible for service, see
Teasdale (2009).² National service includes military and civil defense service. The military includes the air force, the army and the navy. The civil defense is an un-armed government agency whose role is to protect and assist the population in case of crises or catastrophes. The vast majority of inductees participate in military service, and we will simply refer to it as military service. The duration of service is between 3 and 12 months depending on where the service is done. In all cases, the salary compares to the level of an unskilled worker. Due to relatively high minimum wage rates and a high level of redistribution through the Danish tax system, the income during service provides for a relatively good standard of living for most young people. However, there are no provisions to protect employment and the career for workers who interrupt a job spell to enter service. The lottery has numbers corresponding to the size of the cohort attending the draft lottery in the entire country. Draft examinations are identical across examination stations and the lottery is designed to generate an identical risk of being inducted across the country. Men drawing a low number are inducted. The number of inductees needed depends on the staffing needs of the military and civil defense as well as on the number of volunteers. In practice, 40-50% of a cohort of males are recruited for service in the period considered. The exact number is, however, unknown at the time of the lottery.

In our sample about 16% are not eligible for military service. Some of these never participate in the lottery because they are obviously not qualified for service. This could, for example, be because they have physical disabilities or mental problems rendering them unable to participate in service. Some participate in the lottery but are subsequently assessed to be unfit for service because of health problems that did not disqualify them at first or because they have very low test scores. In fact, when we compare observed characteristics of the group of

² The test has been validated extensively, see Teasdale et al. (2011), and it has been shown to have satisfactory test-retest reliability, to correlate with other acknowledged IQ tests and to not be influenced by motivational effects from the conscription context.
ineligibles with those who are eligible, the test scores are the only factor for which we can trace significant differences between these two groups.\textsuperscript{3} In particular, there is no difference between the eligible and ineligible group in terms of youth crime.

The link between the lottery number and the execution of service is not deterministic because not all men who draw a number turn out to be eligible. There is also a small group of draft resisters, who, after having participated in the draft lottery, resist military service. Draft resisters are assigned to non-military service in various places, for example kindergartens, libraries, NGOs, or municipal administration/service. It is possible to volunteer, and volunteering for service can either be true volunteering or technical volunteering where participants can decide to volunteer after having drawn a low number that would obviously imply induction. Technical volunteering can be advantageous because volunteers can expect more influence on the service in terms of the type of service (army, navy, air force, civil defense), which could influence the length and nature, as well as the geographic location, of the service. Volunteering can, thereby, also affect the timing of service as some types of service have waiting lists. The timing of participation in the conscription examination and service can also be influenced by educational deferment. The lottery number thus randomizes entry into military service but does not randomize the timing of entry.

\textit{Data}

We use merged administrative data for this study. The core data set consists of draft lottery records for all men born in 1964 and residing in the eastern part of Denmark\textsuperscript{4} thereby covering 42\% of the 1964 cohort. The background for considering this sample is related to

\textsuperscript{3} We do not have information about health in our data apart from body height and body weight that are collected during the conscription examination.

\textsuperscript{4} Specifically, all males born in 1964 and residing in municipalities with a municipality code smaller than 400 according to Statistics Denmark’s official municipal code system.
resources associated with providing data for analysis. For our purpose it is important that we analyze data from a period where conscription rates are relatively high and where we are able to follow the conscripts’ crime rates for some years after military service. Based on this we decided to sample from the 1964 cohort. The drawback is that lottery numbers are not recorded electronically by the military but stored on physical files at local subsidiaries of The State Archives. The decision to archive conscription lottery numbers is made by the local subsidiaries, and only the archives covering the Eastern part of Denmark has archived the lottery numbers. The final sample therefore covers men who have attended the draft lottery in the Eastern part of Denmark. For this group, we have identified all individual files and typed the information available in the conscription examination records including the draft lottery number, information about body weight and height, about the result from the IQ test and the conscription examination, and the Central Person Registry (CPR) number. The CPR number is the key for recording information in all public administrative registers and we are therefore able to merge the lottery numbers with yearly information from a range of other administrative registers including crime registers and a host of other registers allowing us to characterize the individuals.

Participation in the conscription examination in our sample is concentrated in the years 1982-1984.\textsuperscript{5} We merge the conscription records including the lottery numbers with criminal records from the Central Crime Register. This data set holds information about all arrests made by the Danish police, the charges filed against individuals and subsequent verdicts. In the analysis, we consider an individual to have committed a crime if he is convicted, and we thus do not

\textsuperscript{5} Participation in the conscription examination is distributed as follows: 1982: 14\%, 1983: 57\%, 1984: 18\% 1985:7\%, later: 4\%.
consider charges leading to acquittal. The criminal records provide less detail in the early part of the sample period. For example, before 1990, only the year that the criminal act took place is recorded. The crime records allow us to distinguish between violent crimes and property crimes. We divide the sample according to whether the individuals committed crime before 1982, i.e. at the age of 15-17 years, which are the ages where youths have criminal responsibility but are not yet old enough to enter military service. We will refer to those committing crime in 1980-1981 as youth offenders.

We merge crime and conscription examination records with income tax records and with a range of other registers containing information about education and family background. As administrative registers are longitudinal we collect information for these individuals to cover the period 1980-1999. Unfortunately, we do not have direct recording of whether people have entered military service. In constructing the data we assign a given individual to military service if he has been recorded as a wage earner at a military facility during a given year. Draft examination records contain information about the semester in which a person attended the conscription examination, and we identify military service from employment at a military facility in the same or subsequent semesters. This procedure potentially involves some error in the measurement of military service and the relationship between military service and the lottery number will not be deterministic. Furthermore, military service lasts for 3-12 months for regular service. This implies that if military service begins towards the end of the year and extends into a new calendar year, then the conscript will be recorded as being in service for two consecutive years. The lack of exact information about the exact duration and timing

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6 Charges which the police withdraw due to lack of evidence are not considered, but when charges are withdrawn due to other considerations such as the age of the defendant we include them in the analysis.

7 A limited number of conscripts continue to become officers. In our sample 90 persons are recorded as being officers in our data. Out of these only three were youth offenders We have repeated all analyses omitting these individuals from the sample, and results were unchanged. These results are not reported.
of the service is unfortunate, but the information that we have does allow us to identify with certainty the first calendar year where service took place and to delimit the duration to that year and the following calendar year. As a consequence, any effect found that goes beyond the two first years after enrollment cannot be ascribed to incapacitation associated with serving.

We focus on individuals who participated in the lottery and who are assessed to be eligible for service. This leaves us with 11,563 individuals of which 4,548 entered military service. Table 1 presents descriptive statistics for the sample. The focus of the paper is on whether military service affects the propensity to commit crime. In the analysis we are going to separately consider youth offenders, i.e. individuals who have received a sentence before service, and individuals without a criminal record, and the table therefore presents descriptive statistics for these two groups separately. The idea is that youth offenders are at higher risk of committing crime again and the potential for reducing criminal activity is, therefore, greater for this group. Unsurprisingly, youth offenders are more likely to commit crimes than individuals who are not youth offenders, and most of the criminal activity is property crime. Compared to non-offending youths, youth offenders also have less education, their parents have less education, they are more likely to have criminal siblings, they receive more transfer income and score lower in the IQ test.

[Table 1 about here]

A critical part of our analysis is that the draft lottery number is random. To assess this we have compared the means of pre-conscription characteristics for individuals who drew high and low lottery numbers, and we find that individual characteristics are unrelated to the lottery
number beyond what should be expected from a random sampling. This table is relegated to the Appendix.

The timing of service is not fixed, among other things because of educational deferment. Figure 1 shows the distribution of the timing of service for our sample. Most individuals enter service between 19 and 22 years of age.

[Figure 1 about here]

3. Methods and first-stage

When estimating the effect of military service on crime using OLS, a major concern is that enrollment is correlated with omitted personal characteristics. This potential endogeneity arises because of the option to join the military voluntarily. To address this concern, we estimate the effect with 2SLS using the lottery number as an instrument for military service. One requirement for the lottery number to be a valid instrument is that it should be able to predict military service, such that a lower number is associated with higher probability of service. For draft lottery numbers to be valid instruments they must also be uncorrelated with potential outcomes in and out of military service. This assumption can plausibly be defended by alluding to the randomization by the draft lottery. Our military enrollment dummy is measured with error because we have to infer it from payroll records. Draft resisters are an example of this as we do not know whether they are recorded as enrolled or not. However, the number of draft resisters is small\(^8\) and we do not expect this to be quantitatively important for

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\(^8\) The total number of draft resisters in the full population was 466 in 1982 declining to 218 in 1986. Assuming that draft resisters are equally distributed across the country we would expect about 1-200 resisters in our sample. We assign military service from employment at a military facility. If resisters are recorded at a military
our analysis. Another requirement for the instrument to be valid is, therefore, that the lottery number is uncorrelated with this type of measurement error. The validity of the orthogonality assumption is not directly verifiable, but we conduct a test for validity of the overidentifying restrictions, and if the measurement error in military service does not have the required properties, then this test should be rejected. To estimate the causal effect of military service we are confined to the subsample consisting of eligibles having participated in the lottery. Under these assumptions our estimates can be interpreted as Local Average Treatment Effects (LATE). Our methodology is similar to the one applied by Angrist and Chen (2011). Estimates generated by using lottery number instruments are informative about the effect in the population of eligible men who draw lottery numbers. As a consequence we do not have much to say about the effect among true volunteers, for example.

Although the lottery number randomizes individuals between treatment and control groups, it does not randomize the timing of the military service. Therefore, we need to rely on an estimation strategy where we do not use the timing of enrollment. One possibility is to accumulate the committed crime over the period starting at the point of the draft examination, that is 1982-1990, and regress this on whether the individuals were serving in the military at some point, as well as some covariates measures before 1982. That is

\[
crime_{1982-1990,i} = \beta_0 + \beta_1 Military_{1982-1990,i} + X_{1980-1981,i}\beta_2 + u_i
\]  

(1)

facility they will be classified as being in service and if they are not then they will be recorded as not being in service, but we do not know this.
where \( i \) indexes the individual, \( crime_{1982-1990,i} \) is the accumulated crime, \( Military_{1982-1990,i} \) is a dummy variable taking the value one if individual \( i \) joins the military at some point between 1982 and 1990. \( X_{1980-1981,i} \) is a vector of covariates measured in 1980-1981, i.e. before any of the individuals in our sample enter military service. This estimation will only reveal whether military service has had an effect on criminal activity, but it does not reveal, for example, whether the military only has an incapacitation effect or whether military service has longer-lived effects.

To unfold the time pattern of the effect of military enrollment, we separately estimate a series of equations which make use of the panel dimension to estimate effects of military service for up to eight years after the point of enrollment. To focus on a homogenous group, our treatment group is delimited to individuals who join the military at the ages of 19-22 years, corresponding to 91 percent of the all persons eventually enrolled, and use all individuals aged 20-22 who have not been enrolled as controls. We then conduct an event study where we estimate the dynamic effects of military service centered around the year where military service is initiated.

\[
crime_{jt} = \beta_0 + \beta_1 Military_{jt+k} + X_{1980-1981,i} \beta_2 + D_t + u_{it} \tag{2}
\]

where \( k = (-2, ..., 7) \), i.e. the effect of military service on the propensity to commit crime is estimated for the period ranging from two years before the individuals first appear on the payroll of a military facility to seven years after. \( X_{1980-1981,i} \) is the same vector of pre-service covariates that was used in (1), and \( D_t \) is a vector of year dummies. Because we do not know
the exact calendar month at which service is initiated and we do not know the exact duration, except that is maximally 12 months, the effects estimated for the year where service is initiated and the first year after is potentially a mix of pre-service, service and post-service time. However, we can unequivocally identify two pre-service years and five post-service years, and a finding of significant post-service effects is hence evidence of effects of military service that can not simply be attributed incapacitation due to military service.

In practice we estimate equation (2) as a series of independent regressions for each year relative to the year where service is initiated. The dependent variable in equation (2) is a dummy variable and 2SLS therefore suffers from the usual problems associated with linear probability models that probabilities are not confined to the [0;1] interval. We therefore also present results from estimating the model using a bivariate probit estimator not suffering from this problem, but at the cost of imposing joint normality of the errors.

Figure 2 shows the relationship between the lottery number and the frequency of military service. 80-86% of all men with a lottery number lower than 7,000 end up in military service. For numbers between 7,000 and 20,000 the frequency of military service declines steadily and for numbers above 20,000 the frequency levels at approximately 20%. The military selects in people with the lowest numbers and stop when they have met their staffing needs. The plateau level of 20% for lottery numbers above 20,000 thus reflect the rate of volunteering. As noted above, there is not a deterministic link between the lottery number and our measure of military service and this is likely to be the reason that we do not observe complete participation even among people with low lottery numbers in our sample consisting of eligible men. The pattern in figure 2 is not affected by the inclusion of covariates (not reported).

When specifying the first-stage for estimating equation (1) we approximate the shape in figure 2 by a function composed of a constant in the interval [1;7000[, a second degree polynomial
in the interval [7000;20000] and a constant in the interval [20000; 35000]. This provides overidentifying restrictions that can be used to test the validity of the assumption that the lottery number is not correlated with the measurement error associated with the military enrollment variable.

[Figure 2 about here]

In the analysis, we consider youth offenders and non-offending youths separately, and the results from estimating the first stage regression using the polynomial described above are presented for these two groups separately in table 2. Results show that the draft lottery number clearly predicts enrollment for both groups.

[table 2 about here]

4. Results

Results from estimating equation (1) are given in Table 3. The dependent variable is accumulated criminal offenses 1982-1990, which cover the years where the 1964 cohort served in the military. We consider property crime and violence separately. The right hand side variable of interest is \( Military_{1982-1990,i} \), which is a dummy variable taking the value one if the individual was enrolled in the military in between 1982 and 1990.
For youth offenders, we see that military service reduces the number of property crime offenses both according to OLS and the 2SLS estimates. However, we find no significant effects for violent crime for any of the groups.

In general, the parameter estimates for the control variables have the expected signs. The effects of the control variables tend to be more significant for property crime than violent crime, so we will only briefly consider the control variables for property crime. First, human capital, whether in the form of education or test scores, decreases the likelihood of committing crime. Second, the individual’s criminal activity is also influenced by the family. If brothers committed crime in 1980-1981, this tends to increase the propensity to commit crime later on. For non-offending youths, we find that parental divorce contributes to increasing the likelihood of committing crime, but divorce does not seem to affect the propensity to commit crime for individuals who as youths have already committed crime. Finally, we find that living in rural areas is associated with a smaller propensity to commit crime.

We have performed tests for validity of the overidentifying restrictions in the case of 2SLS estimations and these tests are never rejected at any conventional level of significance. This suggests that the lottery number instruments do not violate the orthogonality condition and that the estimates presented in table 3 can plausibly be interpreted as causal effects. The magnitude of the parameter estimates suggests that over the course of 1982-1990 military service reduced criminal activity among youth offenders in the order of 0.4 crimes over the
course of the data period.\textsuperscript{9} Comparing this with the level of accumulated property crimes among youth offenders, c.f. table 1, this amounts to a reduction in criminal activity of about 30%. However, some individuals joined the military in the beginning of the sample period 1982-1990 whereas others enrolled at the end, and for some individuals the accumulated crime is therefore potentially committed prior to enrollment. Furthermore, the estimates in table 3 do not reveal whether, for example, the effect of military service on property crime is solely an incapacitation effect.

In table 4, we examine the time effect of military enrollment more closely. To accomplish this, we estimate a sequence of models of the form of equation (2) exploring the dynamic pattern of property crime for youth offenders, the only group for whom we found significant effects of military service on crime in table 3. Figure 1 showed that only a few men enter the military relatively late, but that the vast majority enters service at ages 19-22 and we choose to focus on these age groups in order to have relatively homogenous treatment and control groups from which to identify dynamic effects. Table 4 reports the parameter estimates from a sequence of regressions where the dependent variable is measured at different distances from the time of military service. The OLS estimates are small and insignificant throughout. 2SLS estimates are negative but larger in magnitude than OLS and significant for up to five years after the year of enrollment in military service, albeit the fifth year effect is only significant at the 10 percent level. As mentioned previously, the fact that we do not have information about the exact duration and timing of the service is unfortunate, but the information we have does allow us to identify with certainty the first calendar year where service took place and to delimit the duration to that year and the following calendar year since the service can at most

\textsuperscript{9} If we extend the sample period we get further away from the most common time of enrollment and our parameter estimate for military enrollment becomes insignificant, first at the 5 percent level and when we extend the sample further also at the 10 percent level. This is likely because crime generally decreases drastically as the people get older.
cover 12 months. Significant effects in year 3-5 therefore cannot be ascribed to incapacitation associated with serving. In other words, military service seems to reduce crime for youth offenders and the effect is present not only while in service but also for a considerable period after the service has been completed. Marginal effects are also economically significant as military service appears to lower property crime rates by 20 percent in the first year and about 10 percent in the fifth year. Results from applying probit and bivariate probit are qualitatively comparable to OLS and 2SLS, but marginal effects are typically lower.

In table 4, we also report placebo estimates of the effect of military service on crime one and two years before military service actually takes place. If the draft lottery truly randomizes people into the military independently of their criminal behavior, we would not expect to find any effects prior to service. Reassuringly, 2SLS estimates and bivariate probit estimates reported in table 4 confirm that this is in fact the case, suggesting that the estimated effect of military service is not spurious.

[Table 4 about here.]

[Table 5 about here.]

Table 5 repeats the estimation from table 4 but focuses on violent crimes for youth offenders. In table 3 we did not find any evidence that military service reduced the propensity to ever commit violent crimes. This result could, however, conceal significant short-term effects. The parameter estimates are insignificant throughout and we conclude that military does not seem to have any effect on violent crimes for youth offenders. As emphasized by Jacob and Lefgren (2003), property and violent crimes are very distinct as the latter more often is an act of
impulse. This may explain why we do not find any effect of the military service, especially as most violence occurs on the weekends when one is usually off-duty in the military. We note, however, that point estimates are negative, albeit small, and that the lack of significance may be related to the fact that the frequency of violent offenses in the sample is relatively low.

In tables 6 and 7 we turn to the subsample of individuals who are not youth offenders. Table 6 presents results for property crime for those entering service while 19-22 years old. 2SLS estimates suggests that there may be a positive effect in the second and fourth year after starting service, but the effect is only significant at the 5 percent level in the second year and none of them are significant when estimated by bivariate probit model. Overall, these results are clearly too fragile to conclude that military service has any effect on property crime for non-offending youths. For violent crimes, there seems to be a negative first year effect for non-offending youths although the effect is insignificant in the 2SLS estimation. Also for non-offending youths we are not able to reject that military service does not affect crime before service and we conclude that the data does not reject the hypothesis that the lottery number randomizes people into military service irrespective of their criminal behavior.

[Table 6 about here.]

[Table 7 about here.]

The only previous study of peacetime conscription in Argentina on criminal activity, Galiani et al. (2011), found that service boosted subsequent criminal activity. We find that military service has no effect on crime for the vast majority of conscripts and that criminal activity is reduced for youth offenders. There are many differences between the present study and the
study by Galiani et al. (2011) that can potentially explain the different findings. Conscripts included in their study are typically aged 18 when starting military service, i.e. a couple of years less than the individuals in our sample, and the duration of service is longer than in Denmark. Different effects could also be related to differences in the circumstances of the conscription set-up. For example, in Denmark conscripts received compensation comparing in level to that of unskilled workers in the period considered. The Danish minimum wage is relatively high by international standards and the wage distributions relatively compressed. This made conscription less costly in terms of foregone income than in many other countries.

The effect of military service on education, unemployment and earnings for youth offenders

The results presented so far suggest that military service reduces the property crime rate among youth offenders who start military service at ages 19-22. Criminal activity is reduced not only while in service but also for up to five years from the beginning of service, i.e. for three to four years after service is completed. This could, for example, be because the military has provided skills that are relevant for participating in the labor market, or because it has provided a set of skills that lower the costs of investing in post-service education. We therefore go on to investigate whether the reduction in criminal activity is associated with increased education, and/or a drop in post-service unemployment and whether it affects earnings. This analysis is confined to youth offenders because we found no clear effect of military enrollment for individuals not committing crime before the age of 18.

In table 8 we consider both the effect of military service on completion of education and on unemployment. With respect to education, we estimate a cross-sectional regression for completion of vocational or higher education by the age of 35. We consider a longer time
span for the analysis of educational attainment than we did in the analysis of criminal activity, because education is potentially completed at a later age than when criminal careers peak. We find a negative effect of military service on vocational and higher education, but this is only significant at the 10 percent level in the OLS and probit estimations and insignificant when considering 2SLS. In order to examine the effect of military service on unemployment, we estimate a series of equations similar to equation (1). The dependent variable is the number of years with unemployment in the last week of November for the period 1982-1990. Table 8 shows that we find no significant effects of military service on unemployment. This null result may conceal that conscripts could have been slightly unemployed in the period following service, as they are not recorded as being unemployed during service. However, overall there is no evidence that youth offending conscripts have been less affected by unemployment than youth offending non-conscripts.

[Table 8 about here]

[Table 9 about here]

Table 9 presents estimates of the effect of military service on log hourly wage rates and on log yearly wage incomes for youth offenders. When we consider the full-time employed, the 2SLS estimates suggest that military service has an adverse effect on log hourly wage rates for up to six years from the beginning of service. The adverse wage premium is 10-24 percent. We also provide estimates using log yearly wage income as the dependent variable. For the full-time employed, we only find significant effects for the fifth and the sixth year after military enrollment. When we focus on the sample that includes all persons who are not
recorded as students, the picture is qualitatively similar to the case of the log hourly wage rates, but estimates are insignificant and in the range of 4-16 percent. These results arguably reflect substantial sample selection in to being (full time) employed in the years following military service. Therefore, the estimates are more volatile and less reliable compared to the crime results.

Although we cannot control for selection into employment, these results do not suggest that there is any positive effect of military service on earnings. This is in line with findings in Angrist (1990), Imbens and Van der Klaauw (1995), and Galiani et al. (2011). However, we only find a temporary negative effect on wages. Thus, military seems to interrupt working careers or delay education completion, but the effect appears to be relatively short-lived and persons joining the military catch up within six years of beginning military service.

Summarizing the results, focusing on youth offenders, for whom we found effects of military service on the propensity to commit crime, we find no effect of military service on the propensity to complete post compulsory education and on the propensity to be recorded as unemployed. For earnings, we tend to find negative effects for youth offenders within the first six years of beginning service. Combining these findings suggests that military service does not equip youth offenders with productive human capital.

**Conclusion**

We have analyzed the effect of military service on the propensity to commit crime for the cohort of Danish men born in 1964. We address the self-selection into military service by exploiting draft lottery data, and measure the effect of military service by combining these data with longitudinal administrative records. The results show that military service reduces criminal activity for youth-offenders who enter service at ages 19-22. For this group property
crime is reduced for up to five years from the beginning of service, and the effect is therefore not only a result of incapacitation while enrolled. We find no effect of military service on violent crimes. We also find no effect of military service on educational attainment and unemployment for youth offenders, but we find negative effects of service on earnings. We find no effect of military service on the criminal activity of non-offending youths. Overall, these results suggest that military service has an effect on the criminal behavior of youth-offenders. The effect exists not because military service upgrades productive human capital directly but rather because military service does something else, for example changes their attitude towards criminal activity. Criminal activity peaks at age 18 and is reduced to about half by age 25. Most efforts to reduce crime are targeted at youths in the age span 16-25. Our results thus suggest that military service can reduce crime for youth offenders for a significant fraction of the criminal intensive age interval.

Many European countries have compulsory military service. One of the objectives of military service based on conscription is to inform conscripts about important civil values and to create a national community. The results presented here suggest that military service may have beneficial effects on youth-criminals through socialization, a positive effect that has been largely overlooked up to this point. The results from this study also have broader relevance in terms of fighting crime. Evidence about the effect of social policies is mixed. Our results suggest that youth offenders, a group that can be difficult to reach with school and social programs, can be reached with programs that share the features of peace time military service.
References


Figures

Figure 1. Distribution of age at enrollment

Figure 2. Enrollment probability and lottery number
Table 1. Sample means for 1990

<table>
<thead>
<tr>
<th>Variable</th>
<th>Youth offenders</th>
<th>Non-offending youths</th>
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<tbody>
<tr>
<td>Accumulated property crime, 1982-1990</td>
<td>1.233</td>
<td>0.203</td>
</tr>
<tr>
<td></td>
<td>(1.627)</td>
<td>(0.645)</td>
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<td>Accumulated violence, 1982-1990</td>
<td>0.220</td>
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<tr>
<td>High-school (0/1)</td>
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<td>0.226</td>
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<td>Height in centimeters/10</td>
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<td>(0.64)</td>
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<td>0.0321</td>
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<td>(10343.8)</td>
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Number of observations                     956            10607

Notes: All variables besides crime are measured before enrollment. Standard deviations are given in parentheses.
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<th>Youth offenders</th>
<th>Non-offending youths</th>
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<td>Lottery number (7,000-20,000) /1e3</td>
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<td>-0.110***</td>
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<td>(-7.09)</td>
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<td>Lottery number (7,000-20,000) squared /1e6</td>
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<td>0.003***</td>
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<td>(4.41)</td>
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<td>Lottery number &gt;= 20,000</td>
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<td>-1.192***</td>
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<td>(-4.31)</td>
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<td>0.046***</td>
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<td>(0.30)</td>
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<td>(-0.24)</td>
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<td>Criminal brother aged 21-25 years</td>
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<td>(0.91)</td>
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R-squared: 0.233 0.273
F-statistic: 73.192 1103.577
No. of obs.: 956 10607

All variables besides crime are measured before enrollment. Robust t-statistics in parenthesis. * p<0.10, ** p<=0.05, *** p<0.01
Table 3. Accumulated crime 1982-1990

<table>
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<th>Property crime</th>
<th>Youth offenders</th>
<th>OLS</th>
<th>2SLS</th>
<th>Non-offending youths</th>
<th>OLS</th>
<th>2SLS</th>
<th>Violent crime</th>
<th>Youth offenders</th>
<th>OLS</th>
<th>2SLS</th>
<th>Non-offending youths</th>
<th>OLS</th>
<th>2SLS</th>
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<tr>
<td>Military enrollment</td>
<td>-0.243** (-2.39)</td>
<td>-0.426** (1.29)</td>
<td>-0.016 (1.20)</td>
<td>0.03 (1.11)</td>
<td>-0.063 (-0.24)</td>
<td>-0.086 (-0.21)</td>
<td>-0.001 (-0.21)</td>
<td>-0.002 (-0.21)</td>
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<td>High-school</td>
<td>-0.439*** (-2.72)</td>
<td>-0.423*** (-2.00)</td>
<td>-0.080*** (-5.00)</td>
<td>-0.081*** (-4.99)</td>
<td>-0.100** (-2.35)</td>
<td>-0.093** (-2.16)</td>
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<td>-0.013** (-2.10)</td>
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<td>Vocational education</td>
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<td>-0.243 (1.44)</td>
<td>0.033 (1.38)</td>
<td>0.031 (1.87)</td>
<td>-0.011 (-0.05)</td>
<td>-0.002 (-0.11)</td>
<td>0.017** (2.17)</td>
<td>0.017** (2.17)</td>
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<tr>
<td>Parents, vocational</td>
<td>-0.171 (-1.52)</td>
<td>-0.174 (-2.09)</td>
<td>-0.034** (-2.00)</td>
<td>-0.032** (-2.00)</td>
<td>-0.069 (-2.39)</td>
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<td>-0.005 (-0.05)</td>
<td>-0.005 (-0.05)</td>
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<td>-0.055** (-2.00)</td>
<td>-0.13 (-2.33)</td>
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<td>-0.017** (-2.31)</td>
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<td>Weight/10</td>
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<td>-0.061 (-3.81)</td>
<td>-0.032*** (-3.72)</td>
<td>-0.031*** (-3.72)</td>
<td>0.095*** (2.94)</td>
<td>0.080** (2.57)</td>
<td>0.005* (1.68)</td>
<td>0.005* (1.68)</td>
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<tr>
<td>Height/10</td>
<td>-0.097 (-0.91)</td>
<td>-0.094 (-0.55)</td>
<td>-0.007 (-0.61)</td>
<td>-0.008 (-0.61)</td>
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<td>-0.037 (-1.02)</td>
<td>-0.004 (-0.95)</td>
<td>-0.004 (-0.95)</td>
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<tr>
<td>Copenhagen</td>
<td>-0.052 (-0.39)</td>
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<td>-0.112*** (4.84)</td>
<td>-0.167*** (-3.95)</td>
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<td>0.012 (1.62)</td>
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<tr>
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<td>0.018 (0.29)</td>
<td>0.031 (0.29)</td>
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<td>-0.003 (-0.07)</td>
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<td>Rural</td>
<td>-0.395*** (-3.16)</td>
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<td>-0.055*** (-4.04)</td>
<td>-0.087 (-1.64)</td>
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<tr>
<td>Received social assistance in 1980</td>
<td>0.799* (1.87)</td>
<td>0.822* (1.88)</td>
<td>0.037 (0.49)</td>
<td>0.036 (0.49)</td>
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<td>0.383* (1.72)</td>
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<td>-0.014 (-0.94)</td>
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<td>-0.057*** (-5.87)</td>
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<td>-0.012*** (-3.66)</td>
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<tr>
<td>Test score squared</td>
<td>0.058*** (3.07)</td>
<td>0.059*** (3.10)</td>
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<td>0.042*** (3.77)</td>
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<td>0.007 (1.57)</td>
<td>0.007 (1.57)</td>
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<tr>
<td>Criminal brother aged 16-20 years</td>
<td>0.302* (1.18)</td>
<td>0.303* (4.05)</td>
<td>0.236*** (4.05)</td>
<td>0.236*** (4.05)</td>
<td>0.086 (1.08)</td>
<td>0.073 (0.95)</td>
<td>0.02 (1.17)</td>
<td>0.02 (1.17)</td>
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<tr>
<td>Criminal brother aged 21-25 years</td>
<td>0.495* (1.76)</td>
<td>0.516* (1.86)</td>
<td>0.078 (1.32)</td>
<td>0.079 (1.34)</td>
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<td>0.013 (0.50)</td>
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<tr>
<td>Parents divorced by 1981</td>
<td>0.114 (0.85)</td>
<td>0.113 (0.84)</td>
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<td>0.126*** (5.48)</td>
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<td>0.019*** (2.70)</td>
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</tbody>
</table>

All variables besides crime are measured before enrollment. Robust t-statistics in parenthesis. * p<0.10, ** p<0.05, *** p<0.01

R²-squared 0.077 0.074 0.049 0.049 0.07 0.068 0.014 0.014
P-value, Hansen J-statistic 0.903 0.562 0.164 0.758
No. of obs. 956 956 10607 10607 956 956 10607 10607
Table 4. Property crime for youth offenders aged 19-22 at the time of enrollment

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<th>1st year</th>
<th>2nd year</th>
<th>3rd year</th>
<th>4th year</th>
<th>5th year</th>
<th>6th year</th>
<th>7th year</th>
<th>8th year</th>
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<td><strong>OLS</strong></td>
<td>-0.051**</td>
<td>0.005</td>
<td>-0.027</td>
<td>-0.025</td>
<td>-0.019</td>
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<td>-0.033**</td>
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<td>(0.24)</td>
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<td>(-1.18)</td>
</tr>
<tr>
<td><strong>2SLS</strong></td>
<td>-0.15</td>
<td>-0.127</td>
<td>-0.222**</td>
<td>-0.155**</td>
<td>-0.113**</td>
<td>-0.106**</td>
<td>-0.041</td>
<td>-0.009</td>
<td>0.010</td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-1.47)</td>
<td>(-1.40)</td>
<td>(-2.39)</td>
<td>(-2.24)</td>
<td>(-1.97)</td>
<td>(-2.02)</td>
<td>(-1.91)</td>
<td>(-0.72)</td>
<td>(0.21)</td>
<td></td>
</tr>
<tr>
<td><strong>Probit</strong></td>
<td>-0.049**</td>
<td>0.004</td>
<td>-0.027</td>
<td>-0.026</td>
<td>-0.021</td>
<td>-0.014</td>
<td>-0.034**</td>
<td>-0.020</td>
<td>-0.022</td>
<td>-0.019</td>
</tr>
<tr>
<td></td>
<td>(-2.29)</td>
<td>(0.18)</td>
<td>(-1.35)</td>
<td>(-1.23)</td>
<td>(-0.78)</td>
<td>(-2.11)</td>
<td>(-1.14)</td>
<td>(-1.23)</td>
<td>(-1.22)</td>
<td></td>
</tr>
<tr>
<td><strong>Bivariate probit</strong></td>
<td>-0.098</td>
<td>-0.081</td>
<td>-0.138**</td>
<td>-0.103**</td>
<td>-0.067</td>
<td>-0.083**</td>
<td>-0.035</td>
<td>-0.022</td>
<td>-0.021</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td>(-1.33)</td>
<td>(-1.37)</td>
<td>(-3.93)</td>
<td>(-2.46)</td>
<td>(-1.34)</td>
<td>(-2.28)</td>
<td>(-0.50)</td>
<td>(-0.46)</td>
<td>(-0.40)</td>
<td>(0.03)</td>
</tr>
</tbody>
</table>

No. of observations 2551 3162 3162 2843 2704 2641 2600 2576 2563 2545

Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 3 and time dummies. For the probit models we report the average partial effects. t-statistics are given in parenthesis. These are clustered at the person level and are bootstrapped for the probit models using 500 bootstrap samples. * p<0.10, ** p<0.05, *** p<0.01

Table 5. Violent crime for youth offenders aged 19-22 at the time of enrollment

<table>
<thead>
<tr>
<th></th>
<th>2-years before</th>
<th>1-year before</th>
<th>1st year</th>
<th>2nd year</th>
<th>3rd year</th>
<th>4th year</th>
<th>5th year</th>
<th>6th year</th>
<th>7th year</th>
<th>8th year</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>OLS</strong></td>
<td>0.016</td>
<td>-0.013</td>
<td>-0.010</td>
<td>-0.007</td>
<td>-0.008</td>
<td>-0.008</td>
<td>-0.009</td>
<td>0.000</td>
<td>-0.013</td>
<td>-0.012</td>
</tr>
<tr>
<td></td>
<td>(1.22)</td>
<td>(1.45)</td>
<td>(1.08)</td>
<td>(0.73)</td>
<td>(0.99)</td>
<td>(1.02)</td>
<td>(1.35)</td>
<td>(0.00)</td>
<td>(1.92)</td>
<td>(1.70)</td>
</tr>
<tr>
<td><strong>2SLS</strong></td>
<td>0.062</td>
<td>0.050</td>
<td>-0.047</td>
<td>-0.014</td>
<td>-0.014</td>
<td>-0.008</td>
<td>-0.020</td>
<td>-0.009</td>
<td>-0.006</td>
<td>-0.014</td>
</tr>
<tr>
<td></td>
<td>(1.18)</td>
<td>(1.04)</td>
<td>(-1.21)</td>
<td>(-0.43)</td>
<td>(-0.63)</td>
<td>(-0.40)</td>
<td>(-0.9)</td>
<td>(-0.44)</td>
<td>(-0.31)</td>
<td>(-0.74)</td>
</tr>
<tr>
<td><strong>Probit</strong></td>
<td>0.017</td>
<td>-0.010</td>
<td>-0.008</td>
<td>-0.006</td>
<td>-0.007</td>
<td>-0.008</td>
<td>0.000</td>
<td>-0.002</td>
<td>-0.012</td>
<td>-0.013</td>
</tr>
<tr>
<td></td>
<td>(1.36)</td>
<td>(-1.18)</td>
<td>(-1.06)</td>
<td>(-0.64)</td>
<td>(-0.96)</td>
<td>(-0.86)</td>
<td>(-1.16)</td>
<td>(0.07)</td>
<td>(-1.78)</td>
<td>(-1.95)</td>
</tr>
<tr>
<td><strong>Bivariate probit</strong></td>
<td>0.127</td>
<td>0.039</td>
<td>-0.023</td>
<td>-0.010</td>
<td>-0.018</td>
<td>-0.015</td>
<td>-0.008</td>
<td>-0.017</td>
<td>0.020</td>
<td>-0.017</td>
</tr>
<tr>
<td></td>
<td>(1.58)</td>
<td>(0.66)</td>
<td>(-0.58)</td>
<td>(-0.38)</td>
<td>(-0.89)</td>
<td>(-0.47)</td>
<td>(-0.24)</td>
<td>(-0.91)</td>
<td>(-1.55)</td>
<td>(-0.53)</td>
</tr>
</tbody>
</table>

No. of observations 2551 3162 3162 2843 2704 2641 2600 2576 2563 2545

Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 3 and time dummies. For the probit models we report the average partial effects. t-statistics are given in parenthesis. These are clustered at the person level and are bootstrapped for the probit models using 500 bootstrap samples. * p<0.10, ** p<0.05, *** p<0.01

Table 6. Property crime for non-offending youths aged 19-22 at the time of enrollment

<table>
<thead>
<tr>
<th></th>
<th>2-years before</th>
<th>1-year before</th>
<th>1st year</th>
<th>2nd year</th>
<th>3rd year</th>
<th>4th year</th>
<th>5th year</th>
<th>6th year</th>
<th>7th year</th>
<th>8th year</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>OLS</strong></td>
<td>0.003</td>
<td>0.003</td>
<td>0.003</td>
<td>0.003</td>
<td>0.002</td>
<td>0.004</td>
<td>0.001</td>
<td>0.002</td>
<td>-0.005**</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.60)</td>
<td>(1.07)</td>
<td>(1.15)</td>
<td>(0.92)</td>
<td>(0.73)</td>
<td>(1.35)</td>
<td>(0.59)</td>
<td>(0.74)</td>
<td>(-2.36)</td>
<td>(-0.40)</td>
</tr>
<tr>
<td><strong>2SLS</strong></td>
<td>0.024**</td>
<td>0.018</td>
<td>0.015</td>
<td>0.019**</td>
<td>0.012</td>
<td>0.013**</td>
<td>0.006</td>
<td>0.007</td>
<td>-0.005**</td>
<td>-0.002</td>
</tr>
<tr>
<td></td>
<td>(1.68)</td>
<td>(1.50)</td>
<td>(1.20)</td>
<td>(2.02)</td>
<td>(1.57)</td>
<td>(1.90)</td>
<td>(0.96)</td>
<td>(1.15)</td>
<td>(-0.77)</td>
<td>(-0.36)</td>
</tr>
<tr>
<td><strong>Probit</strong></td>
<td>0.002</td>
<td>0.003</td>
<td>0.003</td>
<td>0.003</td>
<td>0.002</td>
<td>0.003</td>
<td>0.002</td>
<td>0.002</td>
<td>-0.005**</td>
<td>-0.001</td>
</tr>
<tr>
<td></td>
<td>(0.72)</td>
<td>(1.08)</td>
<td>(1.26)</td>
<td>(0.94)</td>
<td>(0.79)</td>
<td>(1.44)</td>
<td>(0.74)</td>
<td>(0.83)</td>
<td>(-2.36)</td>
<td>(-0.37)</td>
</tr>
<tr>
<td><strong>Bivariate probit</strong></td>
<td>0.013</td>
<td>0.009</td>
<td>0.003</td>
<td>0.012</td>
<td>0.008</td>
<td>0.008</td>
<td>-0.001</td>
<td>0.007</td>
<td>-0.006*</td>
<td>-0.004</td>
</tr>
<tr>
<td></td>
<td>(0.91)</td>
<td>(0.82)</td>
<td>(0.29)</td>
<td>(1.20)</td>
<td>(1.03)</td>
<td>(1.23)</td>
<td>(-0.15)</td>
<td>(0.95)</td>
<td>(-1.68)</td>
<td>(-1.06)</td>
</tr>
</tbody>
</table>

No. of observations 29027 36335 36325 33013 31319 30537 30080 29820 29635 29485

No. of treated 3063 3820 3827 3822 3811 3807 3794 3774 3758 3745

Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 3 and time dummies. For the probit models we report the average partial effects. t-statistics are given in parenthesis. These are clustered at the person level and are bootstrapped for the probit models using 500 bootstrap samples. * p<0.10, ** p<0.05, *** p<0.01
Table 7. Violent crime for non-offending youths aged 19-22 at the time of enrollment

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>2SLS</th>
<th>Probit</th>
<th>Bivariate probit</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>2-years before</td>
<td>1-year before</td>
<td>1st year</td>
<td>2nd year</td>
</tr>
<tr>
<td>No. of observations</td>
<td>29027</td>
<td>36335</td>
<td>36325</td>
<td>33013</td>
</tr>
<tr>
<td></td>
<td>3063</td>
<td>3820</td>
<td>3827</td>
<td>3811</td>
</tr>
</tbody>
</table>

Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 3 and time dummies. For the probit models we report the average partial effects. t-statistics are given in parenthesis. These are clustered at the person level and are bootstrapped for the probit models using 500 bootstrap samples. * p<0.10, ** p<0.05, *** p<0.01

Table 8. The effect of military enrollment on unemployment and completion of education for youth offenders

<table>
<thead>
<tr>
<th></th>
<th>OLS</th>
<th>2SLS</th>
<th>Probit</th>
<th>Bivariate probit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Vocational and higher education, age 35</td>
<td>-0.059*</td>
<td>-0.01</td>
<td>-0.157*</td>
<td>-0.015</td>
</tr>
<tr>
<td></td>
<td>(-1.80)</td>
<td>(-0.15)</td>
<td>(-1.83)</td>
<td>(-0.10)</td>
</tr>
<tr>
<td>Higher education, age 35</td>
<td>-0.015</td>
<td>-0.009</td>
<td>-0.141</td>
<td>-0.144</td>
</tr>
<tr>
<td></td>
<td>(-0.82)</td>
<td>(-0.25)</td>
<td>(-1.05)</td>
<td>(-0.49)</td>
</tr>
<tr>
<td>Accumulated unemployment, 1982-1990</td>
<td>-0.03</td>
<td>-0.135</td>
<td></td>
<td></td>
</tr>
<tr>
<td></td>
<td>(-0.21)</td>
<td>(-0.45)</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 3. For the probit models we report the average partial effects. t-statistics are given in parenthesis. These are clustered at the person level and are bootstrapped for the probit models using 500 bootstrap samples. * p<0.10, ** p<0.05, *** p<0.01

Table 9. Wages for youth offenders aged 19-22 at the time of military enrollment

<table>
<thead>
<tr>
<th></th>
<th>2SLS estimates</th>
<th>5th year</th>
<th>6th year</th>
<th>7th year</th>
<th>8th year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Dependent variable: Log(hourly wage income)</td>
<td>2-years before</td>
<td>1-year before</td>
<td>1st year</td>
<td>2nd year</td>
<td>3rd year</td>
</tr>
<tr>
<td>Full-time workers</td>
<td>0.226</td>
<td>-0.048</td>
<td>-0.233**</td>
<td>-0.176**</td>
<td>-0.158**</td>
</tr>
<tr>
<td></td>
<td>(0.85)</td>
<td>(-0.25)</td>
<td>(-2.65)</td>
<td>(-2.04)</td>
<td>(-2.08)</td>
</tr>
<tr>
<td>No. of observations</td>
<td>538</td>
<td>755</td>
<td>1009</td>
<td>1133</td>
<td>1269</td>
</tr>
</tbody>
</table>

Dependent variable: Log(yearly wage income)

<table>
<thead>
<tr>
<th></th>
<th>2SLS estimates</th>
<th>5th year</th>
<th>6th year</th>
<th>7th year</th>
<th>8th year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full-time workers</td>
<td>0.212</td>
<td>-0.098</td>
<td>-0.334**</td>
<td>-0.077</td>
<td>-0.158</td>
</tr>
<tr>
<td></td>
<td>(0.62)</td>
<td>(-0.38)</td>
<td>(-2.24)</td>
<td>(-0.58)</td>
<td>(-1.11)</td>
</tr>
<tr>
<td>No. of observations</td>
<td>538</td>
<td>755</td>
<td>1009</td>
<td>1133</td>
<td>1269</td>
</tr>
</tbody>
</table>

All persons, but students

<table>
<thead>
<tr>
<th></th>
<th>2SLS estimates</th>
<th>5th year</th>
<th>6th year</th>
<th>7th year</th>
<th>8th year</th>
</tr>
</thead>
<tbody>
<tr>
<td>Full-time workers</td>
<td>-0.175</td>
<td>-0.104</td>
<td>0.029</td>
<td>-0.068</td>
<td>0.161</td>
</tr>
<tr>
<td></td>
<td>(-0.33)</td>
<td>(-0.28)</td>
<td>(0.11)</td>
<td>(-0.37)</td>
<td>(0.94)</td>
</tr>
<tr>
<td>No. of observations</td>
<td>1106</td>
<td>1464</td>
<td>1713</td>
<td>1891</td>
<td>1991</td>
</tr>
</tbody>
</table>

Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 3 and time dummies. t-statistics are given in parenthesis. These are clustered at the person level. * p<0.10, ** p<0.05, *** p<0.01

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### Appendix table 1. Individual characteristics and the lottery number

<table>
<thead>
<tr>
<th>Characteristics</th>
<th>Youth offenders</th>
<th>Non-offending youths</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Lottery number below median</td>
<td>Lottery number above median</td>
</tr>
<tr>
<td>High-school (0/1)</td>
<td>0.075</td>
<td>0.071</td>
</tr>
<tr>
<td>Vocational education (0/1)</td>
<td>0.190</td>
<td>0.163</td>
</tr>
<tr>
<td>Parent with vocational education (0/1)</td>
<td>0.431</td>
<td>0.370</td>
</tr>
<tr>
<td>Parent with short higher education (0/1)</td>
<td>0.046</td>
<td>0.027</td>
</tr>
<tr>
<td>Parent with long higher education (0/1)</td>
<td>0.098</td>
<td>0.115</td>
</tr>
<tr>
<td>Weight in kilograms/10</td>
<td>6.988</td>
<td>7.008</td>
</tr>
<tr>
<td>Height in centimeters/10</td>
<td>17.920</td>
<td>17.920</td>
</tr>
<tr>
<td>Copenhagen (0/1)</td>
<td>0.230</td>
<td>0.222</td>
</tr>
<tr>
<td>Large city (0/1)</td>
<td>0.008</td>
<td>0.004</td>
</tr>
<tr>
<td>Rural (0/1)</td>
<td>0.205</td>
<td>0.203</td>
</tr>
<tr>
<td>Received social assistance benefits (0/1)</td>
<td>0.017</td>
<td>0.011</td>
</tr>
<tr>
<td>Test score</td>
<td>-0.462</td>
<td>-0.478</td>
</tr>
<tr>
<td>Criminal brother aged 18-20 years (0/1)</td>
<td>0.094</td>
<td>0.107</td>
</tr>
<tr>
<td>Criminal brother aged 21-25 years (0/1)</td>
<td>0.050</td>
<td>0.048</td>
</tr>
<tr>
<td>Parents divorced (0/1)</td>
<td>0.205</td>
<td>0.180</td>
</tr>
</tbody>
</table>

Number of observations: 458 (Youth offenders), 5304 (Non-offending youths)

All variables besides crime are measured before enrollment. * p<0.10, ** p<0.05, *** p<0.01