Does Peacetime Military Service Affect Crime?

Karsten Albæk
SFI – The Danish National Centre for Social Research, DK-1052 Copenhagen, Denmark,
kal@sfi.dk

Søren Leth-Petersen
University of Copenhagen, DK-1353 Copenhagen, Denmark, soren.leth-petersen@econ.ku.dk

Daniel le Maire
University of Copenhagen, DK-1353 Copenhagen, daniel.le.maire@econ.ku.dk

Torben Tranæs
SFI – The Danish National Centre for Social Research, DK-1052 Copenhagen, Denmark,
ttr@sfi.dk

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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, and our results indicate that the effect is unlikely to be the result of incapacitation only. We find no effect of military service on violent crime, on educational attainment, or on employment and earnings, either in the short or the long run. 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 attitudes to criminal activity.

Keywords: draft lottery, empirical analysis, youth offenders

JEL: J, K, I

I. 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. Peacetime conscription is widespread. For example, many NATO countries have peacetime 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 peacetime, 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 wartime. A related strand of literature examines 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; 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 peacetime conscription increases the earnings of low-skilled Portuguese men.

A range of studies has 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. There appears to be evidence that human capital upgrading has lasting effects. 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 that activate 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 peacetime 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 the estimates are not driven by self-selection into military service. Our data cover an extract of the 1964 birth cohort, and are longitudinal so that we are able to follow individuals for 20 years from the year they turn 16. 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.

Our results show that peacetime military service lowers the propensity to commit property crime among youth offenders. We find indications that the effect lasts beyond the service period, and the effect is therefore unlikely to be the result of incapacitation while in service only. 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. For the group of youth offenders we do not find any effects of military service on post-service educational attainment, employment or 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 attitudes to criminal activity for this group.

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[1] This is documented by Statistics Denmark, http://www.statbank.dk/statbank5a/default.asp?w=1301. Similar patterns are well-known for other countries (e.g. Imai and Krishna, 2004; Grogger, 1998).

Our study provides new evidence about the impact of peacetime military conscription on criminal activity. The only previous study of peacetime 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. 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.

II. Military Service and data

Military service

All Danish men upon reaching the age of 18 become liable for conscription and must participate in a draft examination for national service. The draft lottery takes place in connection with a draft examination where participants are also subjected to a health examination and an IQ test. The IQ test and the health test is used for identifying individuals who are not fit 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

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[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.

of crises or catastrophes. The vast majority of inductees participate in military service, and we will simply refer to civil defense and military service 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 or the career for workers who interrupt a job spell to enter service.

The lottery has numbers corresponding to the number of persons included in the draft lottery in the entire country in a given year, i.e. lottery numbers are thus assigned by draft cohort rather than by birth cohort. 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.

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 fit for service. 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 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 draft examination can be postponed if participation interrupts on-going full time education, if a subject has a permanent address outside the country, if he is on parental leave or has close relatives with severe health or social problems. Deferment is only granted after a formal application process, it is typically granted one year at the time, and can at most be postponed until age 25.[3] The lottery thus randomizes entry into military service but does not randomize the timing of the draft examination or the timing of service.

Data

We use merged administrative data for this study. The core data set consists of draft examination records for all men born in 1964 and residing in the Eastern part of Denmark.[4] According to the population registers 17,393 men, or 42% of the 1964 cohort, is from the Eastern part of Denmark. The draft examination data includes records for 16,839 individuals. About 30 percent of these are not fit for service according the draft examination records. 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 IQ test scores. When we compare

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[3] See circular no. 156 of the Defence Minister. (https://www.retsinformation.dk/Forms/R0710.aspx?id=8879).

[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. From population registers we know the identity of all Danish men born in 1964. In the Online Appendix, Table A1 we have calculated means of the covariates that we have available from administrative registers for Danes living in the Eastern and Western part of Denmark. The most striking difference is that people living in Eastern part of Denmark tend to have a slightly higher crime rate than people living in Western part of Denmark. Furthermore, among non-offending youths, individuals from Eastern part of Denmark have more education and a higher fraction of parents are non-cohabiting. The comparison of average characteristics between individuals from the Eastern and Western part of the country is based on all the individuals recorded in the gross population registers.

observed characteristics of the group of men who participate in the lottery but are not fit for service with those who are, the IQ test score is the only factor for which we can trace significant differences between these two groups.[5] We focus on the sample of men who draw lottery numbers and are fit for service. We discard 10 individuals who have incomplete information from the draft examination. Our final sample consists of 11,544 men who are fit for service and who have drawn a lottery number.[6]

The background for considering only the 1964 cohort of men from the Eastern part of Denmark is related to 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 draft examination records including 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 the draft examination records is made by the local subsidiaries, and at the time of the data collection only the archives covering the Eastern part of Denmark had decided to archive the lottery numbers. Therefore, the final sample covers eligible men who took part in the draft lottery in the Eastern part of Denmark. For this group, we have identified all individual files and typed into electronic format the information available in the draft examination records including the draft lottery number, information about body weight and height, the results of the IQ test and the draft 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

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[5] We do not have information about health in our data apart from the body height and body weight data that are collected during the conscription examination.

[6] In Table A1, presented in the Online Appendix, we also provide the average values of characteristics of the people that enter our final sample. Compared with the total 1964 cohort of men from the Eastern part of Denmark the final sample has very similar characteristics, except that the men included in our sample commit slightly less crimes in the period 1982-1990.

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.[7] 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 consider charges leading to acquittal.[8] 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 a 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 records 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 being a wage earner at a military facility during a given year. Draft examination records contain information about the year in which a person attended the conscription examination, and we identify military service from employment at a military facility in the same or subsequent year. This procedure potentially involves some error in the

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[7] Participation in the conscription examination is distributed as follows: 1982: 14%, 1983: 57%, 1984: 18% 1985:7%, later: 4%.

[8] 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.

measurement of military service. As a consequence the relationship between military service and the lottery number will not be deterministic. Furthermore, military service lasts for 3-12 months for regular service.[9] 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 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.

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 had a conviction 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 a 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.

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[9] 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.

Table 1. Sample means

	Youth offenders	Non-offending youths
Accumulated property crime, 1982-1990	1.222	0.200
	(1.621)	(0.635)
Accumulated violent crime, 1982-1990	0.217	0.031
	(0.594)	(0.201)
Enrolled in the military at some point, dummy	0.429	0.390
	(0.495)	(0.488)
Education, 1981: High-school, dummy	0.074	0.226
	(0.261)	(0.418)
Education, 1981: Vocational education, dummy	0.178	0.118
	(0.382)	(0.322)
Parent has vocational education, 1981, dummy	0.401	0.431
	(0.49)	(0.495)
Parent has short higher education, 1981, dummy	0.037	0.040
	(0.188)	(0.196)
Parent has long higher education, 1981, dummy	0.108	0.200
	(0.311)	(0.400)
Weight in kilograms/10, at examination	6.992	7.093
	(0.877)	(0.901)
Height in centimeters/10, at examination	17.92	18.00
	(0.669)	(0.642)
Living in Copenhagen, 1981, dummy	0.226	0.139
	(0.419)	(0.346)
Living in large city other than Copenhagen, 1981, dummy	0.006	0.004
	(0.079)	(0.062)
Living in rural area, 1981, dummy	0.204	0.259
	(0.403)	(0.438)
Received social assistance benefits, 1980, dummy	0.014	0.007
	(0.116)	(0.081)
Test score from examination, standardized	-0.504	0.052
	(0.944)	(0.993)
Has criminal brother aged 18-20 years in 1980-1981, dummy	0.102	0.032
	(0.329)	(0.182)
Has criminal brother aged 21-25 years in 1980-1981, dummy	0.049	0.014
	(0.222)	(0.122)
Non-cohabiting parents, 1981, dummy	0.190	0.124
	(0.393)	(0.330)
Draft lottery number	17,557	18,002
	(10,457)	(10,342)
Number of observations	951	10,593

Notes: Accumulated crime measures the number of crimes that have been accumulated over the period 1982-1990. Education variables for both children and parents measure the highest level of completed education in 1981. Test scores have been standardized to have mean zero and unit variance in the full sample. Standard deviations are given in parentheses.

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. Distribution of age at enrollment
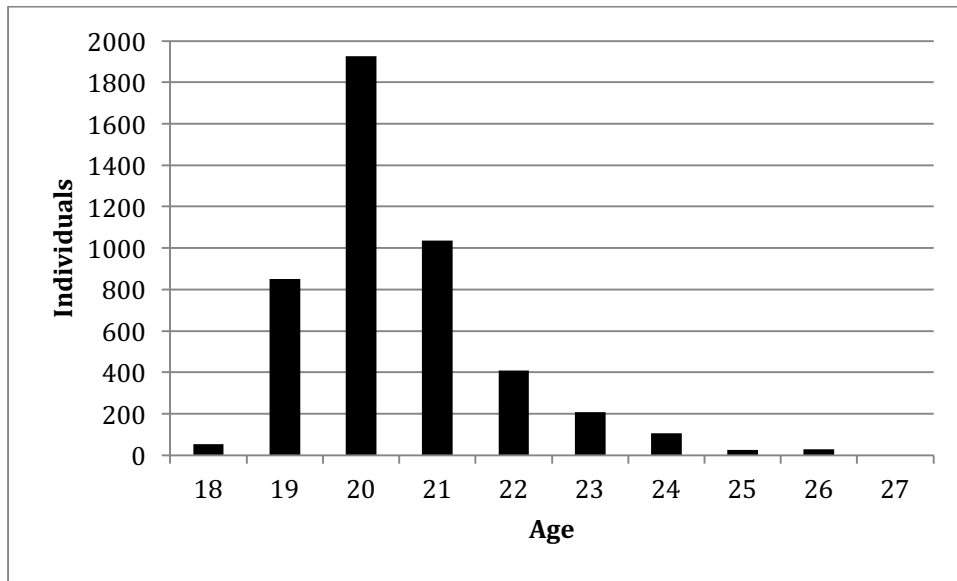
Notes: The figure displays the number of individuals in the data set who are enrolled in military service by age at enrollment.

III. 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 of military service. Alluding to the randomization by the draft lottery can plausibly defend this assumption.

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[10] and we do not expect this to be quantitatively important for our analysis. Another requirement for the instrument to be valid is, therefore, that the lottery number is uncorrelated with this type of measurement error. To estimate the causal effect of military service we are confined to the subsample consisting of eligible men who 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 measured before 1982. That is

$$crime_{1982-1990,i} = \beta_0 + \beta_1 Military_{1982-1990,i} + X_{1980-1981,i}\beta_2 + u_i \qquad (1)$$

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

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[10] 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 100-200 resisters in our sample. We assign military service from employment at a military facility. When resisters are recorded at a military facility then they will be classified as being in service. If the resisters are not recorded at a military facility then they will be recorded as not being in service. We do not know how draft resisters are recorded.

estimation will only reveal whether military service has had an effect on criminal activity, but it does not reveal, for example, whether the military service only has an incapacitation effect or whether military service has longer-lived effects.

We also attempt to unfold the time pattern of the effect of military enrollment relative to the point in time where military service is initiated. We do this by separately estimating a series of equations that 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 relative 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 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 that military service is initiated.

$$crime_{it} = \beta_0 + \beta_1 Military_{it-k} + X_{1980-1981,i}\beta_2 + D_t + u_{it} \qquad (2)$$

where $k = (-2, -1, 1, 2, ..., 7)$, 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. In practice we estimate equation (2) as a series of independent linear regressions for each year relative to the year that service is initiated. Because we do not know the exact calendar month in which service is initiated and we do not know the exact duration, except that it is maximally 12 months, the effects estimated for the year that service is initiated and the first year after are potentially a mix of pre-service, service and post-service time. However, we can unequivocally identify up to two pre-service years for the subset that start service when aged 20, and a number of post-service years. Finding significant post-service effects beyond two

years after initiation of service is evidence of effects of military service that cannot simply be attributed to incapacitation due to military service. The effect of military service is captured by the vector of parameters β_1. Unbiased estimates of these parameters can only be obtained if the error term does not covary with $Military_{it-k}$, i.e. if the timing of military service is uncorrelated with the propensity to commit crime. It is important to note that the draft lottery does not randomize the timing of attending the draft examination nor the timing of service, i.e. the draft lottery does not ensure that the error term is uncorrelated with $Military_{it-k}$.

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 out 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 reflects the rate of volunteering.

Figure 2. Enrollment probability and lottery number
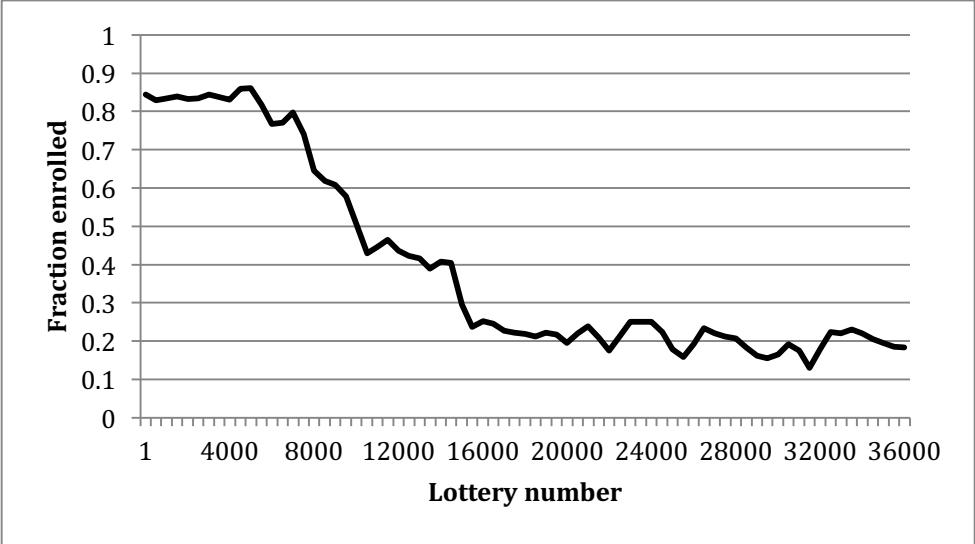
Notes: The graph plots the fraction of people who enter military service by size of the lottery number, where the average propensity of military service is calculated within buckets of 500 lottery numbers.

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).

For estimating the connection between the draft lottery number and the propensity to enter military service, i.e. the first stage equation, we rank lottery numbers and divide them into deciles. We then construct dummies for the first four deciles separately. We collapse the upper six deciles into one category because lottery participants drawing numbers in this segment of the distribution are effectively not at risk of being selected for service.[11] In an attempt to address the consequences of potential non-random allocation to draft cohorts we include draft cohort dummies in all regressions.

If the lottery truly allocates people randomly to service, then we would expect that lottery numbers are unrelated to baseline characteristics. To check for this we ran regressions of each lottery number decile indicator on the total set of baseline covariates and tested for the joint significance of covariates. The results are reported in Table 2. In these tests the null hypothesis of no explanatory power of the baseline characteristics is accepted for all lottery number decile indicators. We also test whether the baseline characteristics are balanced across the deciles that we use as the basis for constructing instruments. Specifically, we regress each of the baseline characteristics reported in table 1 on draft cohort dummies and dummies for the lottery number deciles, and conduct F-tests for the joint insignificance of decile dummies. These tests are conducted separately for youth offenders and non-offending youths.

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[11] The draft lottery is based on a new pool of numbers every year. To be consistent with this, separate first stage profiles should be estimated for each draft cohort. We have tried to do this (not reported), and it did not change the results much, except that standard errors become larger because more parameters are estimated. This is consistent with the fact that the draft lottery cohorts considered here are relatively homogenous in terms of size and eligibility threshold.

Table 2. Regressing lottery decile indicators on baseline characteristics

	Youth offenders				Non-offending youths			
	Lottery decile				Lottery decile			
	1	2	3	4	1	2	3	4
Draft age 19	0.005	0.060***	-0.014	0.003	-0.021**	-0.009	-0.017*	0.015*
	(0.17)	(2.74)	(-0.48)	(0.12)	(-2.26)	(-1.03)	(-1.88)	(1.84)
Draft age 20	-0.018	0.067**	-0.03	0.023	-0.023**	-0.009	-0.019*	-0.002
	(-0.48)	(2.16)	(-0.80)	(0.66)	(-2.16)	(-0.83)	(-1.75)	(-0.17)
Draft age 21	0.004	0.090*	-0.071	-0.012	-0.035***	-0.003	-0.01	0.033**
	(0.08)	(1.69)	(-1.53)	(-0.25)	(-2.65)	(-0.19)	(-0.68)	(2.25)
Draft age 22 or older	0.019	0.042	-0.036	0.03	-0.030**	-0.004	-0.018	0.014
	(0.31)	(0.84)	(-0.67)	(0.52)	(-2.05)	(-0.24)	(-1.24)	(0.95)
Education, 1981: High-school, dummy	-0.015	0.064	-0.043	-0.048	0.002	-0.003	0.009	-0.015*
	(-0.31)	(1.54)	(-1.20)	(-1.19)	(0.22)	(-0.34)	(1.00)	(-1.76)
Education, 1981: Vocational education, dummy	0.013	0.009	0.019	0.005	0.009	-0.013	0.011	-0.01
	(0.42)	(0.34)	(0.64)	(0.22)	(0.97)	(-1.46)	(1.18)	(-1.10)
Parents, vocational education dummy	0.044*	-0.02	0.008	0.02	0.000	0.000	-0.009	0.005
	(1.84)	(-0.94)	(0.35)	(1.00)	(-0.05)	(0.00)	(-1.28)	(0.79)
Parent has short higher education, 1981, dummy	0.016	-0.019	0.045	0.157**	0.002	-0.003	-0.006	-0.006
	(0.28)	(-0.39)	(0.72)	(2.17)	(0.11)	(-0.19)	(-0.40)	(-0.40)
Parent has long higher education, 1981, dummy	0.017	-0.016	-0.022	0.05	0.016*	-0.003	-0.011	0.010
	(0.45)	(-0.47)	(-0.69)	(1.41)	(1.70)	(-0.33)	(-1.26)	(1.14)
Weight in kilograms/10, at examination	-0.003	-0.002	0.016	-0.004	0.001	0.005	-0.004	-0.003
	(-0.23)	(-0.12)	(1.23)	(-0.32)	(0.27)	(1.25)	(-0.92)	(-0.83)
Height in centimeters/10, at examination	0.004	-0.005	-0.016	-0.005	-0.003	0.001	0.008	0.000
	(0.19)	(-0.25)	(-0.88)	(-0.26)	(-0.55)	(0.25)	(1.41)	(-0.06)
Living in Copenhagen, 1981, dummy	-0.023	0.003	0.039	-0.006	0.010	-0.022***	0.006	-0.006
	(-0.91)	(0.13)	(1.46)	(-0.25)	(1.18)	(-2.69)	(0.64)	(-0.66)
Living in large city other than Copenhagen, 1981, dummy	0.178	0.08	0.092	-0.124***	0.023	0.003	-0.026	0.071
	(0.87)	(0.55)	(0.60)	(-3.88)	(0.45)	(0.06)	(-0.63)	(1.21)
Living in rural area, 1981, dummy	-0.005	0.024	0.015	-0.044**	0.001	0.002	-0.016**	0.009
	(-0.18)	(0.88)	(0.56)	(-1.99)	(0.09)	(0.28)	(-2.36)	(1.33)
Received social assistance benefits, 1980, dummy	0.174	0.018	0.041	-0.044***	0.023	-0.037	0.037	-0.008
	(1.28)	(0.18)	(0.39)	(-2.68)	(0.61)	(-1.19)	(0.91)	(-0.21)
Test score from examination, standardized	0.004	-0.016	-0.002	0.033*	0.000	-0.004	-0.001	0.001
	(0.23)	(-1.39)	(-0.19)	(1.91)	(-0.14)	(-1.05)	(-0.31)	(0.19)
Test score from examination, standardized, squared	0.008	-0.006	0.003	0.017*	0.000	-0.002	-0.002	0.001
	(0.68)	(-0.63)	(0.32)	(1.73)	(-0.19)	(-0.98)	(-0.67)	(0.49)
Has criminal brother aged 18-20 years in 1980-1981, dummy	-0.026	0.01	-0.061***	0.009	-0.004	0.051***	0.012	0.012
	(-0.98)	(0.29)	(-2.79)	(0.35)	(-0.25)	(2.66)	(0.73)	(0.69)
Has criminal brother aged 21-25 years in 1980-1981, dummy	0.051	0.116*	-0.023	-0.059**	0.023	-0.002	0.032	-0.025
	(0.87)	(1.92)	(-0.56)	(-2.57)	(0.89)	(-0.09)	(1.16)	(-1.26)
Non-cohabiting parents, 1981, dummy	-0.007	0.002	0.057**	0.003	0.000	-0.006	-0.002	0.000
	(-0.27)	(0.09)	(1.99)	(0.14)	(-0.04)	(-0.65)	(-0.26)	(0.04)
R-squared	0.015	0.020	0.020	0.030	0.002	0.003	0.002	0.002
F-test of joint significance, p-value	0.118	0.291	0.419	0.765	0.197	0.662	0.613	0.645
No. of observations	951	951	951	951	10,593	10,593	10,593	10,593

Notes: Robust t-statistics in parenthesis. * p<0.10. ** p<=0.05. *** p<0.01. The F-test tests the hypothesis that all of the explanatory variables are jointly insignificant. For F-tests p-values are reported.

We find that the parameters of the decile indicators are jointly insignificant across all covariate regressions except one. This finding is consistent with the notion that the lottery

truly randomizes people into service. These results are relegated to the Online Appendix, Tables A2 and A3. Overall, both sets of tests of the balancing property of the lottery confirm that the lottery balances observable characteristics.

Table 3. First stage regressions

	Youth offenders	Non-offending youths
Draft age 19	-0.031	-0.043***
	(-0.76)	(-3.40)
Draft age 20	-0.158***	-0.127***
	(-3.04)	(-8.59)
Draft age 21	-0.224***	-0.170***
	(-3.09)	(-8.91)
Draft age 22 or older	-0.055	-0.100***
	(-0.66)	(-4.80)
Lottery no. in 1st decile	0.566***	0.628***
	(13.76)	(50.26)
Lottery no. in 2nd decile	0.479***	0.609***
	(9.79)	(47.68)
Lottery no. in 3rd decile	0.420***	0.394***
	(8.35)	(24.80)
Lottery no. in 4th decile	0.231***	0.217***
	(3.96)	(13.80)
Education, 1981: High-school, dummy	-0.014	0.023*
	(-0.24)	(1.88)
Education, 1981: Vocational education, dummy	-0.018	0.031**
	(-0.46)	(2.39)
Parent has vocational education, 1981, dummy	-0.002	-0.01
	(-0.07)	(-1.03)
Parent has short higher education, 1981, dummy	-0.115	-0.009
	(-1.41)	(-0.42)
Parent has long higher education, 1981, dummy	-0.038	-0.037***
	(-0.78)	(-2.92)
Weight in kilograms/10, at examination	-0.002	0.012**
	(-0.08)	(2.28)
Height in centimeters/10, at examination	0.014	0.009
	(0.51)	(1.21)
Living in Copenhagen, 1981, dummy	-0.022	-0.003
	(-0.61)	(-0.22)
Living in large city other than Copenhagen, 1981, dummy	-0.204	0.023
	(-1.31)	(0.36)
Living in rural area, 1981, dummy	0.080**	0.040***
	(2.02)	(4.07)
Received social assistance benefits, 1980, dummy	-0.042	-0.061
	(-0.37)	(-1.22)
Test score from examination, standardized	-0.007	-0.012**
	(-0.36)	(-2.48)
Test score from examination, standardized, squared	0.005	-0.003
	(0.36)	(-0.74)
Has criminal brother aged 18-20 years in 1980-1981, dummy	0.049	-0.008
	(1.10)	(-0.38)
Has criminal brother aged 21-25 years in 1980-1981, dummy	0.054	0.04
	(0.68)	(1.25)
Non-cohabiting parents, 1981, dummy	-0.013	-0.012
	(-0.33)	(-0.95)
R-squared	0.239	0.278
F-statistic for joint significance of lottery decile indicators	69.62	1082.64
No. of observations	951	10,593

Notes: Robust t-statistics in parenthesis. * p<0.10. ** p<=0.05. *** p<0.01. For explanations relating to the explanatory variables, see notes to table 1.

Table 3 presents the first stage estimates. In the analysis we consider youth offenders and non-offending youths separately, and the results from estimating the first stage regression using the lottery decile dummies described above are presented for these two groups separately in Table 3. The parameter estimates for the lottery number deciles are clearly significant in both regressions showing that the draft lottery number predicts enrollment for both groups. Drawing a number in the first decile increases the chance of ending up in service by about 60 percent relative to drawing a number in the fifth-tenth decile, where the lottery number does not force anyone to enter. In the second to fourth deciles the relative chances of ending up in service are, respectively, 48, 42, and 23 percent for youth offenders and the corresponding numbers for non-offending youths are 61, 39, and 22 percent.

IV. Results

Results from estimating equation (1) are presented in Table 4. 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 between 1982 and 1990.

For youth offenders, results show that military service reduces the number of property crimes both according to OLS and the 2SLS estimates. We find no significant effects on property crime for non-offending youths and we find no effects for violent crime for any of the groups. Interestingly, the relative size of the estimated parameters for offending and non-offending youths is of similar magnitude across property crime and violent crime. This might be indicative that military service has the potential to reduce violent crime as well but that violent crime is too infrequent for us to detect it statistically.

Table 4. Accumulated crime 1982-1990

	Property crime				Violent crime			
	Youth offenders		Non-offending youths		Youth offenders		Non-offending youths	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
Military enrollment	-0.285***	-0.438**	0.014	0.016	-0.073*	-0.089	0.000	-0.004
	(-2.78)	(-2.05)	(1.08)	(0.64)	(-1.88)	(-1.11)	(-0.11)	(-0.55)
Draft age 19	-0.233	-0.229	-0.014	-0.014	-0.079	-0.076	-0.005	-0.005
	(-1.58)	(-1.58)	(-0.77)	(-0.74)	(-1.30)	(-1.32)	(-0.92)	(-0.95)
Draft age 20	-0.597***	-0.605***	-0.014	-0.014	-0.197***	-0.192***	0.001	-0.001
	(-3.26)	(-3.35)	(-0.62)	(-0.60)	(-2.92)	(-2.87)	(0.07)	(-0.12)
Draft age 21	-0.458**	-0.486**	-0.066***	-0.065***	-0.238***	-0.235***	0.005	0.006
	(-2.00)	(-2.11)	(-2.70)	(-2.64)	(-3.35)	(-3.29)	(0.52)	(0.55)
Draft age 22 or older	0.123	0.131	-0.046	-0.045	0.038	0.011	-0.015*	-0.015*
	(0.36)	(0.39)	(-1.56)	(-1.52)	(0.25)	(0.08)	(-1.85)	(-1.89)
Education, 1981: High-school, dummy	-0.412**	-0.405**	-0.062***	-0.061***	-0.117***	-0.116***	-0.007*	-0.007*
	(-2.46)	(-2.45)	(-4.84)	(-4.78)	(-2.71)	(-2.72)	(-1.94)	(-1.87)
Education, 1981: Vocational education, dummy	-0.355***	-0.351***	0.029	0.028	-0.048	-0.055	0.017**	0.018**
	(-2.73)	(-2.75)	(1.25)	(1.22)	(-0.95)	(-1.16)	(2.18)	(2.23)
Parent has vocational education, 1981, dummy	-0.153	-0.145	-0.037**	-0.036**	-0.073*	-0.083*	-0.007	-0.007
	(-1.32)	(-1.27)	(-2.34)	(-2.29)	(-1.65)	(-1.91)	(-1.30)	(-1.40)
Parents, short-term higher education	-0.122	-0.121	-0.053**	-0.051**	0.024	0.023	-0.017**	-0.017**
	(-0.59)	(-0.60)	(-2.05)	(-2.01)	(0.29)	(0.30)	(-2.28)	(-2.35)
Parent has long higher education, 1981, dummy	-0.235	-0.239	-0.031*	-0.029*	-0.115**	-0.116***	-0.011**	-0.011**
	(-1.38)	(-1.42)	(-1.74)	(-1.65)	(-2.53)	(-2.58)	(-2.08)	(-2.17)
Weight in kilograms/10, at examination	-0.057	-0.058	-0.030***	-0.030***	0.107***	0.103***	0.005*	0.005*
	(-0.75)	(-0.77)	(-3.57)	(-3.62)	(3.36)	(3.34)	(1.75)	(1.76)
Height in centimeters/10, at examination	-0.071	-0.075	-0.008	-0.008	-0.040	-0.043	-0.004	-0.004
	(-0.68)	(-0.73)	(-0.63)	(-0.62)	(-0.96)	(-1.09)	(-0.87)	(-1.02)
Living in Copenhagen, 1981, dummy	-0.034	-0.032	0.112***	0.114***	-0.169***	-0.169***	0.01	0.01
	(-0.26)	(-0.24)	(4.89)	(5.02)	(-4.02)	(-4.09)	(1.40)	(1.46)
Living in large city other than Copenhagen, 1981, dummy	1.536	1.545	0.021	0.013	-0.061	-0.065	0.003	0.003
	(1.37)	(1.41)	(0.20)	(0.13)	(-0.34)	(-0.36)	(0.14)	(0.13)
Living in rural area, 1981, dummy	-0.396***	-0.379***	-0.057***	-0.055***	-0.102*	-0.100**	-0.008*	-0.008*
	(-3.19)	(-3.13)	(-4.21)	(-4.15)	(-1.96)	(-2.02)	(-1.74)	(-1.75)
Received social assistance benefits, 1980, dummy	0.852**	0.879**	0.044	0.049	0.408*	0.398*	-0.015	-0.014
	(1.99)	(2.05)	(0.58)	(0.66)	(1.87)	(1.84)	(-0.99)	(-0.95)
Test score from examination, standardized	-0.284***	-0.287***	-0.066***	-0.067***	-0.057***	-0.051***	-0.014***	-0.014***
	(-4.24)	(-4.33)	(-9.19)	(-9.29)	(-2.84)	(-2.64)	(-6.24)	(-6.28)
Test score from examination, Standardized, squared	-0.078*	-0.081*	0.016***	0.016***	0.002	0.001	0.002	0.002
	(-1.71)	(-1.82)	(2.93)	(2.97)	(0.14)	(0.07)	(1.15)	(1.18)
Has criminal brother aged 18-20 years in 1980-1981, dummy	0.296*	0.301*	0.227***	0.229***	0.081	0.07	0.023	0.019
	(1.86)	(1.92)	(3.90)	(3.96)	(1.03)	(0.97)	(1.31)	(1.16)
Has criminal brother aged 21-25 years in 1980-1981, dummy	0.487*	0.503*	0.082	0.083	0.198	0.183	0.014	0.014
	(1.73)	(1.81)	(1.40)	(1.41)	(1.49)	(1.47)	(0.54)	(0.55)
Non-cohabiting parents, 1981, dummy	0.099	0.093	0.122***	0.120***	-0.054	-0.028	0.021***	0.020***
	(0.73)	(0.70)	(5.35)	(5.31)	(-1.28)	(-0.71)	(2.88)	(2.84)
R-squared	0.094	0.092	0.043	0.043	0.087	0.086	0.013	0.013
No. of observations	951	951	10,593	10,593	951	951	10,593	10,593

Notes: Robust t-statistics in parenthesis. * p<0.10. ** p<=0.05. *** p<0.01. For explanations relating to the explanatory variables, see notes to table 1.

In general, the parameter estimates for the control variables have the expected signs. 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 be associated with a higher propensity to commit property 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 already committed crime as youths. We find that living in rural areas is associated with a smaller propensity to commit crime. Non-offending youths based in Copenhagen have a higher propensity for committing property crime, but youth offenders have a lower probability of committing violent crimes if they are based in Copenhagen. There is some evidence that body weight is associated with a lower propensity to commit property crime but a higher propensity to commit violent crimes.

The lottery randomized entry into service, and consistent with this the balance tests reported above confirmed that covariates are balanced across lottery number deciles. As a consequence it is not obvious why covariates should be included when estimating the effect of military service on crime, as we have done in Table 4. However, we are interested in estimating the effect of military service for a sub-group consisting of youth offenders, which includes relatively few observations. Including covariates in the regressions may help to gain more precise estimates of the military service effect. To investigate the importance of the covariates we have re-estimated equation (1) in a sequence of versions where we include progressively more covariates as regressors. That exercise confirms the conjecture that including covariates is useful for gaining precision of the estimate of military service on crime. Results are shown in the Online Appendix, Table A4 and Table A5.

The magnitude of the parameter estimates for military service presented in Table 4 suggests that, over the course of 1982-1990, military service reduced criminal activity among youth

offenders in the order of 0.4 crimes.[12] Comparing this with the level of accumulated property crimes among youth offenders before entry, c.f. Table 1, this amounts to a reduction in criminal activity of about 35%. Taking into account that youth-crimes are counted over a two-year period and the accumulated effect is measured over a nine-year period, the effect reduces to about 10%. 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 4 do not reveal whether, for example, the effect of military service on property crime is solely an incapacitation effect.

We now turn to exploring the dynamic pattern of effects of military enrollment. To do this we run a sequence of regressions of the form of equation (2) where the dependent variable is measured at different distances from the time of military service. Estimating equation (2) we push the research design to the limit. The draft lottery randomizes entry into the military, but it does not randomize the timing of draft lottery attendance nor entry into service. When measuring effects of service relative to the timing of entry we therefore risk comparing individuals who have selected into military service at different times depending on their propensity to commit crime outcome. 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. Furthermore, we generally ask more from our data when estimating equation (2) compared to equation (1). By estimating dynamic effects we effectively estimate more parameters and, therefore, get less precise estimates than when

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[12] 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 in the sample get older.

estimating equation (1), cf. Table 4. This is a real issue in this data set as we are trying to estimate effects based on a relatively small subset consisting of youth offenders. To estimate equation (2) we compare people who have entered military service with people who have not entered military service.[13] For example, for estimating the first year effect we pick all observations for individuals aged 19-22, assign observations for individuals who enter service in a given year to the treatment group and assign observations for people who are not serving and have not previously served to the control group. We control for year fixed effects as well as the baseline characteristics that were also used in the estimation of equation (1), the results of which are reported in Table 4.

Table 5 reports the parameter estimates for youth offenders. The top panel shows estimates for property crime and the bottom panel for violent crime. For property crime the OLS estimates are generally small, a few parameter estimates are significant, but the pattern of significance is not systematic. 2SLS estimates are negative and larger in magnitude than OLS, and they are significant only in year one to four relative to the time of enrollment. According to the estimates the crime rate drops by some 18 percent in the year of enrollment and then tapers off to about 10 percent in year four after entry. The effect estimated in year five points to a reduction of about eight percent, but the estimate is not significant at any conventional level. From year six after enrollment effects are negligible. 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, last 12 months. Therefore, significant

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[13] Using this approach, we effectively include persons who have not yet entered military service, but eventually will, as control group observations. We have also tried to estimate the effects by only comparing people who enter service at some point with people who never enter service. Doing that we obtain parameter estimates displaying the same pattern, but the parameters are estimated with less precision. These results are available upon request.

effects, albeit only at the 10 percent level, in years 3 and 4 after enrollment cannot be ascribed to incapacitation associated with serving. Marginal effects are also economically significant, as military service appears to lower property crime rates by 18 percent in the first year and about 10 percent in the fourth year.

Table 5. Crime relative to time of military service for youth offenders aged 19-22

	2-years before	1-year before	1st year	2nd year	3rd year	4th year	5th year	6th year	7th year	8th year
Property Crime:										
OLS	-0.050**	-0.001	-0.03	-0.034*	-0.023	-0.021	-0.039**	-0.021	-0.024	-0.023
	(-2.25)	(-0.06)	(-1.48)	(-1.72)	(-1.21)	(-1.07)	(-2.17)	(-1.11)	(-1.35)	(-1.36)
2SLS	-0.142	-0.093	-0.179**	-0.139**	-0.101*	-0.106*	-0.079	-0.021	0.008	0.044
	(-1.34)	(-1.04)	(-1.97)	(-2.03)	(-1.76)	(-1.88)	(-1.37)	(-0.37)	(0.14)	(0.81)
Violent Crime:										
OLS	0.014	-0.015*	-0.014	-0.007	-0.008	-0.007	-0.009	-0.001	-0.014**	-0.013*
	(1.08)	(-1.71)	(-1.58)	(-0.74)	(-1.02)	(-0.91)	(-1.34)	(-0.13)	(-2.12)	(-1.80)
2SLS	0.082	0.058	-0.054	-0.006	-0.024	-0.016	-0.008	-0.009	-0.022	-0.035*
	(1.47)	(1.25)	(-1.37)	(-0.19)	(-1.21)	(-0.97)	(-0.47)	(-0.47)	(-1.15)	(-1.87)
No. of observations	2,535	3,142	3,142	2,825	2,688	2,626	2,587	2,563	2,550	2,532
No. of treated	298	388	388	387	387	384	382	382	381	376

Notes: Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 4 and year dummies. t-statistics are given in parenthesis. These are clustered at the person level. * $p<0.10$. ** $p<0.05$. *** $p<0.01$

In Table 5, we also report pre-service estimates of the effect of military service on crime one and two years before military service actually takes place. The draft lottery randomizes entry into service but not the timing, and the experiment hence does not guarantee that we find no pre-service effects. Furthermore, there is a time gap between the draft lottery and the service in which the anticipation of future service could potentially influence the decision to commit crime. [14] We find parameter estimates that are of a magnitude comparable to the effects estimated after entry into service, but, reassuringly, the pre-service effects are not significant at any conventional level. [15] Taken together the results provided in Table 5 give some

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[14] We have also addressed the anticipation effect directly by conducting an event analysis similar to the analysis presented in Table 5 but where we measure the effect relative to the time of the draft lottery. In this analysis we do not find any significant effects. The results are relegated to the Online Appendix, Table A6.

[15] The identification of the effect of the timing of entry on crime relies on the assumption that the timing of entry into military service is unrelated to the propensity to commit crime. To investigate the plausibility of this assumption we have re-estimated table 5 separately for entry ages 19-20 and 21-22. These results are presented

indication that military service reduces crime for youth offenders not only while in service but also for some time after the service has been completed.

In Table 4 we did not find evidence that military service reduced the propensity to ever commit violent crimes. This result could, however, conceal significant short-term effects. The second panel in Table 5 shows results for violent crime for youth offenders where the effect is quantified relative to the timing of entry into military service. Here we find no systematic effects of service on violent crime either before or after the point of entering military service, and we conclude that military service 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 is more often 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 in the fact that the frequency of violent offenses in the sample is low.

We have also performed a similar analysis for the subsample of individuals who are not youth offenders. Generally, both OLS and 2SLS estimates are small in size and insignificant, and we conclude that military service does not have 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, but the effect is very small and only significant at the ten percent level in the 2SLS estimation. These results are relegated to the Online Appendix, Table A9.

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in the Online Appendix, Table A7. Across the different entry ages we find the same overall pattern as in Table 5. Splitting the sample generally leaves the parameters less precisely estimated. However, the results do not indicate that the parameter estimates presented in Table 5 are driven by any particular age of entry. As another check we have performed regressions of entry age on the baseline characteristics. The age of entry is naturally related to the age at which people have attended the draft lottery. However, for youth-offenders there appears to be no strong association between entry age and baseline characteristics. For non-offending youths the entry age is linked to education and educational background, i.e. the education of the parents, test score, and living in Copenhagen. These results are reported in the Online Appendix, Table A8.

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 younger 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 comparable 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 makes conscription less costly in terms of foregone income than in many other countries.

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

The results presented so far suggest that military service reduces the property crime rate among youth offenders. In the dynamic analysis we found some indication that criminal activity is not only reduced while in service but also for up to four years from the beginning of service. 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 lowers 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 change in post-service employment 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.

Table 6. The effect of military enrollment on educational attainment, employment and earnings at age 35 for youth offenders

	OLS	2SLS
Vocational and higher education, age 35	-0.030	0.004
	(-0.93)	(0.06)
Higher education, age 35	-0.015	-0.022
	(-0.82)	(-0.61)
Annual earnings, age 35	-0.009	-0.043
	(-0.17)	(-0.39)
Full-time employment, age 35	-0.002	-0.059
	(-0.07)	(-0.90)
No. of observations	927	927

Notes: Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 4. Earnings are measured relative to the average earnings in any given year. Robust t-statistics are given in parenthesis.

In Table 6 we consider the long-term effect of military service on completion of education, employment, and earnings among all youth offenders in our sample. We do this by estimating a series of equations similar to equation (1), but where the dependent variable is the outcome measured at age 35. In the first two rows of Table 6 we have estimated the effect of military service on completing vocational or higher education and the effect of completing higher education. In neither case do we find any evidence that military service has moved long-term educational outcomes, and the result is the same whether considering OLS or 2SLS. In rows three and four we consider earnings and full-time employment. In both cases parameter estimates are insignificant. Overall, there is thus no indication, that military service among youth offenders has had any lasting effect on education, employment, or income.

The previous results suggest that the long run impact of conscripted military service among youth offenders has no effects on long-term earnings outcomes. To investigate whether employment and earnings might have been affected in the short-run we estimate the effect of military service relative to the timing of service using a specification similar to equation (2) and focusing again on the group who entered service at age 19-22. The first panel in Table 7 focuses on earnings, and the outcome is annual earnings relative to average earnings in the

sample in the same year. The OLS results suggest that there is no effect on earnings except in the year where service begins. However, that effect vanishes when estimating by 2SLS and taking into account the potential endogeneity of the military service indicator. The pattern is confirmed when moving to the lower panel of Table 7 and considering full-time employment. The OLS estimates suggest an increase in full-time employment in the year where service begins, but that effect is not present when taking into account the endogeneity of military service. In summary, we find no effects of military service on earnings and full-time employment, either in the short or the long run.

Table 7. Earnings and full-time employment for youth offenders aged 19-22 at the time of military enrollment

	2-years before	1-year before	1st year	2nd year	3rd year	4th year	5th year	6th year	7th year	8th year
Annual earnings:										
OLS	-0.006	-0.052	0.171***	0.038	0.001	0.025	0.053	0.029	-0.016	-0.009
	(-0.15)	(-1.38)	(5.61)	(1.02)	(0.03)	(0.60)	(1.20)	(0.63)	(-0.33)	(-0.20)
2SLS	0.132	-0.036	-0.102	-0.21	-0.216	-0.148	-0.049	-0.078	-0.129	-0.09
	(0.56)	(-0.18)	(-0.58)	(-1.37)	(-1.44)	(-0.99)	(-0.32)	(-0.50)	(-0.78)	(-0.54)
Employment:										
OLS	-0.003	-0.044*	0.157***	-0.028	-0.008	0.033	0.065**	0.029	-0.022	0.026
	(-0.09)	(-1.65)	(6.29)	(-0.98)	(-0.27)	(1.15)	(2.23)	(0.94)	(-0.70)	(0.86)
2SLS	0.072	0.026	0.15	-0.037	0.046	0.062	0.108	0.036	-0.084	-0.008
	(0.47)	(0.20)	(1.22)	(-0.36)	(0.48)	(0.65)	(1.13)	(0.38)	(-0.87)	(-0.08)
No. of observations	2,535	3,142	3,142	2,825	2,688	2,626	2,587	2,563	2,550	2,532
No. of treated	298	388	388	387	387	384	382	382	381	376

Notes: Each parameter estimate is an estimate from a separate regression using the same control variables as in Table 4 and year dummies. Earnings are measured relative to the average earnings in any given year. t-statistics are given in parenthesis. These are clustered at the person level. * $p<0.10$. ** $p<0.05$. *** $p<0.01$.

According to our results there appears to be no positive effect of service on earnings. This is in line with findings of Angrist (1990), Imbens and Van der Klaauw (1995), and Galiani *et al.* (2011). Our estimates of military service for youth offenders are insignificant across the board, and if anything, they point towards a temporary drop in earnings.

Summarizing the results, focusing on youth offenders, for whom we find 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 or on earnings and full-time employment. Combining these findings suggests that military service does not equip youth offenders with productive human capital.

Simple cost-benefit analysis

Our results show that crime is reduced for youth offenders without affecting their earnings and employment prospects when leaving the military. This suggests that there may be beneficial effects of peacetime military service on youth offenders through socialization. Our main estimates presented in Table 4, showed that military service reduced the number of crimes by 0.44. This roughly implies that for every two youth offenders entering service, one property crime less was committed in the period 1982-1990. Ejsing and Olsen (2015), using contingent valuation methods, estimate that the subjective valuation of property crimes in the population is 40,000 DKK per crime. This means that the benefits of the reduction in crime is about 20,000 DKK per youth offender drafted into military service. Our results showed that there did not appear to be any adverse effects of military service on employment or educational attainment for youth offenders. However, military service may entail costs for the group of non-offending youths in terms of reduced earnings. To gauge the potential effects of military service on earnings we have estimated the effect of service on earnings cumulated over two horizons, 18-26 years, and 18-35 years, using the full sample. Results are presented in Table 8. When looking at earnings cumulated over the period where the individual under consideration was aged 18-26, the 2SLS estimate for youth offenders shows that the average youth offender in the sample earned 24,695 DKK less than corresponding individuals who did not serve. Similarly, over the age interval 18-35 the same group earned 18,816 DKK less. Consistent with the previous findings, the estimates reported in Table 8 remain insignificant for youth offenders. For non-offending youths we also fail to find significant effects of

military service on cumulated earnings. This suggests that there is, in fact, a small net social benefit associated with reducing crime for youth offenders that is in the order of magnitude of 20,000 DKK per youth offender who is drafted into service. This calculation does not take into account the direct cost of military service, which will exceed the benefits by far. Our results thus point towards benefits of military service that cannot justify military service in its own right, but which nevertheless represent a benefit of peacetime military service that has been largely overlooked.

Table 8. Accumulated earnings

	Youth offenders	Non-offending youths	All
Age 18-26:			
OLS	2,748	17,872*	15,205**
	(0.11)	(2.40)	(2.11)
2SLS	-24,695	-17,381	-19,708
	(-0.46)	(-1.19)	(-0.52)
Age 18-35:			
OLS	-40,553	14,433	5,895
	(-0.52)	(0.62)	(0.26)
2SLS	-18,816	-19,718	-39,427
	(-1.38)	(-1.17)	(-0.43)
No. of observations (age 26)	951	10,593	11,544
No. of observations (age 35)	927	10,299	11,226

Notes: Robust t-statistics in parenthesis. * p<0.10. ** p<=0.05. *** p<0.01. All regressions include the same control variables as in Table 4. The dependent variable measures accumulated earnings in DKK.

V. 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 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. For this group property crime is reduced for up to four years from the beginning of service, and the effect is, therefore, not only the result of incapacitation while enrolled. We find no effect of military service on violent crimes.

Furthermore, we find no effect of military service on employment and earnings, either in the short or the long run, and we find no effect of service on educational attainment. Finally, 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 appears to exist 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. We estimate the monetary value of these benefits to be positive, albeit relatively small, and not large enough to justify the direct costs of military service. Nevertheless, the positive external effect of military service has been largely overlooked up to this point. The results from this study also have broader relevance in terms of fighting crime. 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 peacetime military service.

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