The Opportunity Costs of Mandatory Military Service: Evidence from a Draft Lottery

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Conscription forces young men to serve in the military, irrespective of their opportunity costs. Using random assignment in the Danish draft lottery, we find a negative average effect of peacetime military service on earnings, an effect varying by ability, with high-ability men facing a $23,000 lifetime earnings penalty but with low-ability men facing none. This gradient is robust to different measures of labor market prospects. Educational disruption is an important mechanism, while service has little effect on health or criminal activity. The opportunity costs of conscription are borne by men with the best labor market prospects.

I. Introduction

Conscription exists in the majority of the world's countries, most of which are not involved in armed conflict. Military service is often extensive, ranging from a few months to several years, and is unique in that it

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constitutes the only remaining example in modern societies of forced labor that is legal outside the criminal justice system. It also takes place during an important phase of young men's lives,[1] when decisions are made about labor market entry and further education.[2]

The inequities of conscription have long been debated. In 1818, Benjamin Franklin wrote about "impressed" (conscripted) navy sailors' foregone earnings in the merchant fleet.[3] Friedman (1962) was an eloquent opponent of military conscription, viewing the draft as an in-kind tax paid only by those who serve, and economic analyses clarified the viability of an all-volunteer force in the Vietnam War era (Gates et al. 1970).[4] Although Poutvaara and Wagener (2007) argue that conscription is an inefficient allocation of resources, with forced labor leading to considerable loss of output, a military training also teaches discipline and social skills (Grönqvist and Lindqvist 2016) and helps integrate different groups in society.

Empirical evidence on the effects of mandatory peacetime military service remains limited, with even less known about how the burden of the draft is distributed across recruits. Our paper uses a draft lottery to estimate the effect of peacetime military service on a wide range of outcomes: earnings, schooling, health, and crime. Moreover, by estimating the effects across the ability distribution of fit-for-service men, we provide the first detailed evidence on the distribution of the opportunity costs of the draft. Our estimates help quantify conscription's inefficient allocation of resources.

To estimate the effect of peacetime military service, we use data from the Danish draft. On turning 18 years old, men in Denmark must participate in an "Armed Forces Day" (AFD) military recruitment event during which they undergo a variety of tests. Some men who are assessed fit for military service are randomly assigned to serve. We exploit this random assignment

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[1] Although most countries draft only men, as of 2015 nine countries also draft women. For an overview of conscription gender, ages, and length of military service worldwide, see the World Factbook 2017 (Washington, DC: Central Intelligence Agency, 2016).

[2] Albrecht (1999) pinpoint three reasons that such interruptions matter. First, labor market experience is lost, and wages tend to rise with experience. Second, anticipated interruptions may affect human capital investments and the choice of jobs. Third, time out of the civilian workforce may lead to human capital depreciation.

[3] Published posthumously in Benjamin Franklin and William Temple Franklin, *Memoirs of the Life and Writings of Benjamin Franklin*, vol. 3, p. 169.

[4] See Altman and Fechter (1967), Altman and Barro (1971), Hansen and Weisbrod (1967), and Oi (1967) for economic arguments for and against the draft.

to credibly estimate the causal effect of peacetime military service on labor earnings and other relevant outcomes.[5]

Our main contribution is to decompose service effects by labor market prospects. We exploit an attractive feature of the Danish system, whereby the random assignment to serve takes place *after* completion of the enlistment tests. We know Armed Forces Qualification Test (AFQT) scores and height measured during the AFD. This information—together with other preassignment characteristics, such as parental schooling, household income, and birth weight—provides us a unique opportunity to examine whether the effect of conscription varies across men with different labor market prospects.

Linkage to administrative data on educational qualifications, labor market outcomes, health care usage, and criminal convictions allows us to investigate to what extent any differential earnings effects run through these channels. In addition, with this data we can relate our findings to the broader literature that evaluates the average effect of military service on various outcomes.

We find that peacetime military service has a negative mean impact on earnings: men who serve because they are drafted but who otherwise would not have volunteered earn about 2.5% less at ages 25–35 than if they had not served. Allowing the service effect to vary across the AFQT score distribution, we find no effect among low-ability men but negative effects for those with high ability. At the top quartile of the AFQT score distribution, we find that serving causes a 7% earnings loss—a penalty that diminishes with age but adds up to $23,000 in foregone lifetime earnings. Our findings are robust to other measures of labor market prospects, with similar earnings losses for the top quartile of birth weight, height at AFD, parental schooling, and household income at age 15. For high-ability men, we find that part of the effect on earnings runs through delayed educational attainment, with service causing men to have completed fewer years of schooling and to more likely be enrolled in formal education at age 30.

Our findings have two important implications. First, the average costs of mandatory service, in terms of delayed human capital accumulation and reduced earnings, dominate any positive effects that we can measure. Second, the burden of the draft is unequally distributed, with men with good labor market prospects facing the greatest opportunity costs. To recruit a force of a given size, conscription imposes higher opportunity costs than an all-volunteer force: while some high-opportunity-cost men would not volunteer

[5] During the period of potential service we consider (1994–2007), the Danish military was involved in both peacekeeping and peace-enforcing operations in Afghanistan, the Balkans, and Iraq. While conscripts were not required to serve abroad, 4.4% of our sample volunteer to do so. Throughout the paper we refer to this period as "peacetime," to distinguish it from, say, conscription during the Vietnam War era and to place our study alongside other studies in which conscription does not entail a high probability of military combat.

at the all-volunteer wage rate, they would nonetheless be forced to serve under the draft.

To place our contributions within the literature on the effects of military service, we group previous studies into three sets focusing on earnings outcomes: wartime service, peacetime service, and heterogeneous effects. While our paper also relates to the many studies of military service effects on other outcomes (e.g., education,[6] health,[7] and crime[8]), our placement is best described alongside other earnings studies, as earnings are the most common outcome in the literature and our main outcome of interest.

A first group of studies estimates the effect of wartime service on earnings, capturing the effects of both loss of experience and career interruptions as well as the negative impact of participating in a war. Using the US Vietnam War draft lottery, Angrist (1990) finds 15% earnings penalties of serving in the medium run, but follow-up studies find insignificant long-run earnings effects (Angrist and Chen 2011; Angrist, Chen, and Song 2011), with similar zero long-run earnings effects for the Australian Vietnam draft (Siminski 2013).

A second group of studies have used various natural experiments to estimate causal effects of peacetime conscription. Using cohort differences, Grenet, Hart, and Roberts (2011) and Bauer et al. (2012) find no earnings effects of service for the UK and Germany, respectively, whereas Imbens and van der Klaauw (1995) and Hubers and Webbink (2015) find earnings losses of 3%–5% for the Netherlands. By exploiting a draft lottery, we provide new evidence on the effects of peacetime service.

The third and smallest group of studies examines heterogeneity in the effect of military service. In the context of wartime service, Angrist and Chen

[6] Cipollone and Rosolia (2007) show that regional exemptions from the Italian draft increased the high school graduation rates of men because of less disruption of studies. For France, Maurin and Xenogiani (2007) show that schooling levels of young men fell after the abolition of the draft because of weakened incentives for staying in school to avoid the draft. Bauer et al. (2014) show that the introduction of the draft in Germany increased higher education enrollment to avoid the draft. Card and Lemieux (2001) look at college attendance and draft avoidance in Vietnam War–era United States.

[7] For health outcomes, studies using the Vietnam War draft lotteries have found increased short-run mortality (Hearst, Newman, and Hulley 1986), which diminishes over time (Dobkin and Shabani 2009; Angrist, Chen, and Frandsen 2010; Siminski and Ville 2011; Conley and Heerwig 2012) but reappears in the very long run for the United States and Australia (Autor, Duggan, and Lyle 2011; Johnston, Shields, and Siminski 2016).

[8] Papers on military service effects on crime have mixed findings, with increases following the Vietnam War draft lottery in the United States (Rohlfs 2010; Lindo and Stoecker 2014), no effects of the Australian Vietnam lottery (Siminski, Ville, and Paull, 2016), increases in peacetime Argentina (Galiani, Rossi, and Schargrodsky 2011) and Sweden (Siminski, Ville, and Paull 2016; Hjalmarsson and Lindquist 2016), but no crime effects on average in Denmark (Albæk et al. 2017; Lyk-Jensen 2018).

(2011) find short-run earnings losses for whites but insignificant effects for nonwhites. Angrist, Chen, and Frandsen (2010) find effects on disability benefit receipt only for high school dropouts. For peacetime service, Card and Cardoso (2012) find a positive effect among low-educated men in Portugal. No previous study has exploited continuous proxies (e.g., AFQT scores) for labor market prospects—as we do here—to study heterogeneity in the effect of military service.

The paper proceeds as follows. Section II explains the organization of military conscription in Denmark and the details of the draft lottery. Section III describes the data we use, and section IV presents our empirical approach. Section V presents our results, and section VI concludes.

II. Military Conscription in Denmark

On their 18th birthday, all men[9] receive a letter requiring their attendance at an AFD on a specific date 3–9 months later.[10] About 200 AFDs take place over the year. At each of the six regional military recruitment centers, 40–50 men are assessed for 5–6 hours. Men found to be fit for service are randomly assigned to military service through a lottery held at the end of the AFD.

A. Conscription Procedure

Figure 1 illustrates the conscription procedure. Before participating in the AFD, all prospective draftees submit a health questionnaire to be used as background information for the fitness assessment. Based on responses to this questionnaire and supporting documentation, about 10% of a cohort is declared unfit for military service and therefore ineligible for the draft and not required to attend an AFD. Reasons for ineligibility are serious somatic or psychiatric disorders, which need to be certified by a consultant physician at a regional public hospital (Hageman, Pinborg, and Andersen 2008).

On the AFD, prospective draftees undergo a medical examination and a psychological evaluation and complete an AFQT. These test results and the health assessment form the basis for further selection. For low AFQT scores (10%), high body mass index (10%), low body mass index (5%), and certain medical conditions (the top three, at 2%, being attention-deficit/hyperactivity disorder, musculoskeletal disorders, and asthma), 27% of a cohort or 30% of men attending the AFD are declared ineligible. Thus, 70% of those participating in an AFD (63% of each cohort) are declared fit for military service and assigned a future date at which they may be required to

[9] Since 2004, women have been invited to participate in the AFD but not in the lottery. We include only men in our analysis, as no mandatory draft exists for women, and few women volunteered to serve during our sample period. We only refer to men in this paper.

[10] AFDs take place in birth-order sequence within recruitment districts.

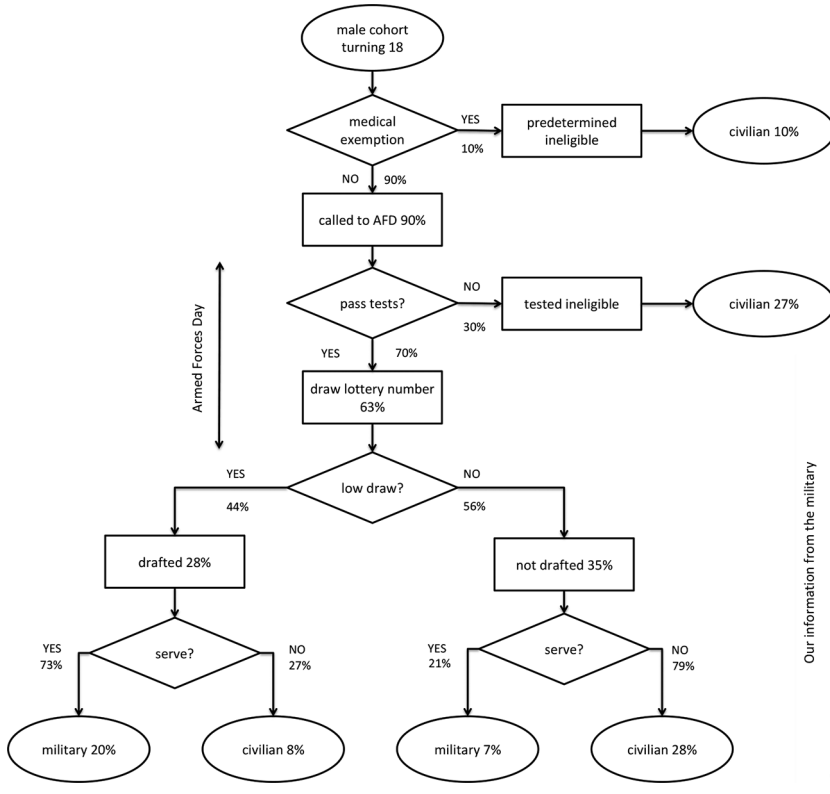

FIG. 1.—Flow chart of the conscription process. Numbers inside the shapes denote average percentages of our birth cohorts 1976–83. Numbers to the right or the left of the arrows denote average percentages for taking each route conditional on reaching the junction. The Armed Forces Day (AFD) includes test taking and drawing lottery numbers. Our estimation data set contains information on all those who drew a lottery number.

serve (service begins every 6 months). This date allows for completion of education before service if a deferment is requested, up to age 25.[11] Finally, all fit-for-service men must draw a lottery number from a drum.

B. Lottery Draws and Service Probability

Regardless of the lottery outcome, at the AFD any man declared fit for service can volunteer and will serve from his preassigned service date. Each February and August, the Ministry of Defense announces a lottery number

[11] In exceptional circumstances service start can be delayed until age 31. See article 25, paragraph 2, of the Law of Military Service.

threshold below which men assessed fit for service during the previous half year are assigned to serve. Importantly, as thresholds are set for each half year AFD, rather than potential service dates, deferment has no effect on the probability of being drafted.[12] The threshold depends on the needs of the military and the number of volunteers and is subject to the distribution of potential service dates. We refer to men drawing a lottery number below the threshold as "drafted" regardless of whether they served or whether they volunteered.

Lottery numbers are generated by a third party (TDC [Tele Danmark]) and range from 1 to 36,000. Both lottery thresholds for recent years and that the threshold is volatile are known to these men.[13] The relationship between service probability and proximity to the lottery threshold is presented in figure 2. Those below the threshold are drafted and are much more likely to serve than those above the threshold. Service probability increases somewhat approaching the threshold from above or below. Although threshold proximity appears to encourage volunteering, because the threshold is announced only after volunteering is complete, this pattern is likely due to a desire to reduce future uncertainty for men who have drawn a low number.[14]

C. Compliance with the Draft

Of a full male cohort, 37% are unfit for service, 36% are judged fit for service but do not serve, and 27% serve in the military. Twenty eight percent of a cohort are drafted: 20% who actually serve, and 8% who do not (3% are conscientious objectors,[15] 4% later have poor health, 0.5% have a criminal record,[16] and 0.5% are either fined or imprisoned under the draft

[12] In contrast to some other countries, educational deferment cannot be considered draft avoidance behavior in Denmark. In our sample, 99.6% had completed their military service before turning 25.

[13] The mean lottery number threshold during our enlistment period is 16,730, but it ranges from 3,000 for those with service dates in the second half of year 2000 to 36,000 for the first half of year 2003. Correlation in the threshold from one enlistment period to the next is −0.39.

[14] In specification checks we show that our results are robust to inclusion of lottery numbers as instruments and robust to the exclusion of men close to the lottery threshold. As lottery thresholds are assigned biannually according to AFD date, men who are fit for service cannot manipulate their draft probability. Nevertheless, fig. 2 clearly shows that service probability increases close to the threshold. Excluding lots near to the threshold produces estimates similar to the sample as a whole, suggesting that volunteering close to the draft threshold is not driving our findings (see table A7).

[15] Conscientious objectors are assigned to work for their local municipality in care facilities for children or the elderly, hospitals, libraries, etc. This civil service is of a similar 8-month duration, and the rate of pay is the same as for military service.

[16] For practical purposes, criminal background checks are not run before the AFD.

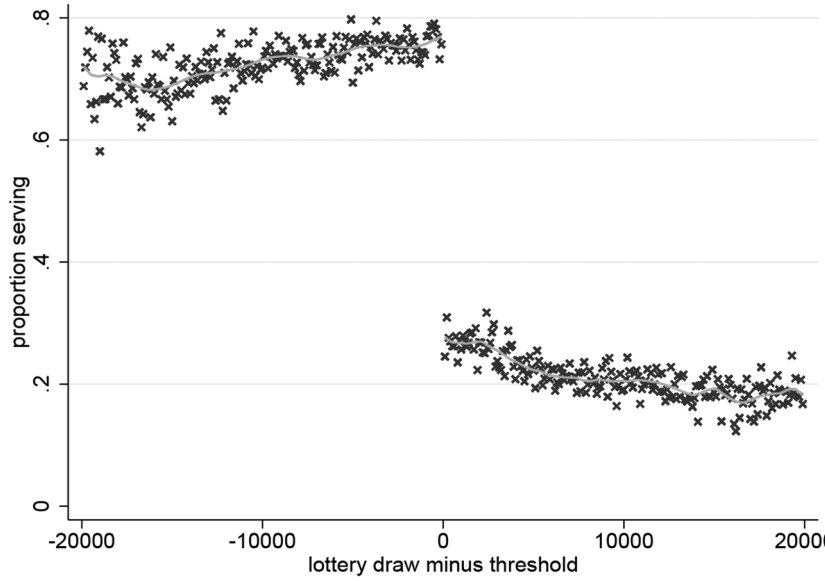

FIG. 2.—Service probability by lottery draw. *X*s indicate proportions serving among bins containing 100 consecutive lottery draws above the threshold. The lines are (Epanechikov) kernel-weighted (second-degree) local polynomials estimated on the bin proportions each side of the threshold. A color version of this figure is available online.

law).[17] We observe 7% of a cohort volunteering for the military because they serve but were not drafted, but we are unable to observe those who would have volunteered had they not been drafted. Assuming that no individuals will always do the opposite of what they are assigned to, we can infer that 14% of a cohort serve because they were drafted but otherwise would not have volunteered.[18] While the 27% rate of service in Denmark is higher than the 4%–26% in the US Vietnam War era (Angrist 1990), it is lower than the 40% in the Netherlands (Hubers and Webbink 2015).[19] Although

[17] We are unable to distinguish conscientious objectors from others who are drafted but do not serve. Thus, in our analysis conscientious objectors are classified as not serving.

[18] Of a cohort, 14% serve because they are drafted, as 20% are drafted and serve and 6% would serve even if they were not drafted (21% of fit-for-service are always-takers and 28% of a cohort are drafted as shown in fig. 1).

[19] While rates of compliance with the draft are much higher in Denmark (73%) than in the US Vietnam War era (23%), US rates are percentages of everyone with a selected date of birth and do not account for ineligibility (Angrist and Chen 2011). In Denmark, only those fit for service enter the draft lottery. For comparison with the United States, assuming that lots were drawn in Denmark regardless of fitness and that 44% of a cohort was drafted but none of the unfit men served, the 8% of a cohort not serving would increase to 24% once we include the additional 16%

we find some variation in compliance rates across the different subpopulations (not shown), the variation in Denmark is much less than that between whites and nonwhites in the United States.

D. Age of Service, Type of Service, and Remuneration

Although military service can begin from 6 months after announcement of the lottery thresholds, educational deferment may delay service start. To illustrate the effect of educational deferment on service age, figure 3 shows the distribution of age at start of military service by AFQT score quartile. As service might affect eventual educational attainment, we use AFQT scores here as a proxy for education for this illustration.

Among those serving at ages 19–20, lowest-quartile AFQT scores are more common than highest-quartile ones, whereas among those serving at ages 21–24, an AFQT score gradient with higher scores is more common. That age of service has a choice element and is correlated with observed characteristics is clear. Thus, service age will affect outcomes measured before age 25. While reduced-form estimates of draft effects would still make sense, instrumental variable (IV) estimates of service effects would be hard to interpret. As our aim is to estimate the causal effects of service, the validity of IV estimates is paramount. Therefore, we exploit random assignment to service some time during the age range 19–24 and measure the consequences on outcomes from age 25. In robustness checks, we consider younger outcomes.

The length of military service for most of our sample period is 8 months.[20] Service is performed in the army (82%), navy (7%), air force (5%), and civil defense and fire and rescue services (6% for the last three combined). Conscripts receive compensation for their military service, in the form of food and lodging, a monthly taxable salary of 7,421 Danish kroner (DKK) ($1,060) in 2013, and a tax-free monthly allowance of 6,230 DKK ($890). For comparison, the minimum gross monthly wage in construction in the same year was 16,000 DKK ($2,285).

III. Data

We use administrative records from the Ministry of Defense on 155,570 fit-for-service men for birth cohorts 1976–83. The data to which we have access is for AFQT scores, height, lottery number, the AFD half year,

(37% of a cohort unfit multiplied by 0.44 draft probability). Thus, we would obtain a hypothetical Danish rate of service for those drafted of 45% (20/(20 + 24)).

[20] A small minority of army placements can last longer than 8 months; e.g., service with the Mounted Royal Guards lasts 12 months. At the end of our sample period, 376 men (0.2%) were subject to the new 4-month service requirement after 2006. The change in service length was announced at the same time as the lottery draft threshold for service in the first half of 2007. Thus, deferring the date of service in anticipation of a shortened period of service was not possible.

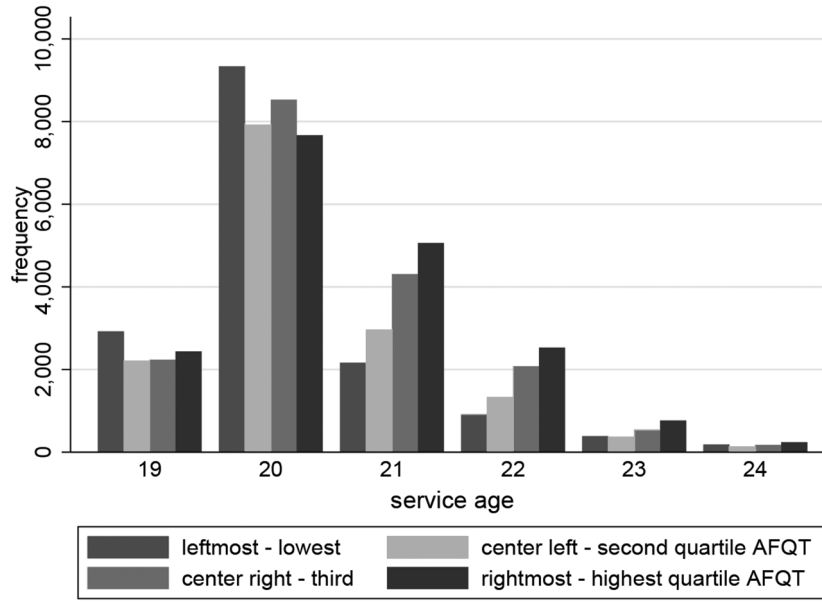

FIG. 3.—Service age distribution by Armed Forces Qualification Test (AFQT) score quartile. For those who served among our 1976–83 birth cohorts, 0.4% serving after age 24 are not shown.

and the potential starting half year for their military service.[21] The Danish AFQT, a cognitive test called the Børg Prien Prøve, was developed for Danish armed forces recruitment and has been used since 1957 on about 1.5 million men.[22] The test comprises 78 items with an even balance of logical (matrices), verbal (analogies), numerical (series), and spatial (geometry) reasoning. Tests are time limited, items are not multiple choice, and total test score is the sum of correct items, together measuring fluid intelligence rather than acquired knowledge.

Because AFQT scores are central to our analysis, we validate them with scores obtained in mandatory tests taken at the end of compulsory school

[21] These are the first years of Ministry of Defense records with computerized lottery numbers. Albæk et al. (2017) digitize records for the 1964 birth cohort from one recruitment district.

[22] See Teasdale (2009) for psychometric properties of the Danish AFQT and a review of its applications. Mortensen, Reinisch, and Teasdale (1989) show that the Børg Prien Prøve is correlated 0.82 with the Wechsler Adult Intelligence Scale. Teasdale et al. (2011) show that test scores are invariant to attitudes toward the military, suggesting that the test's reliability is not undermined by lack of motivation or underperformance among those taking the test.

(ninth grade). In appendix B (apps. A and B are available online), we show relationships with ninth-grade grade point averages, indicating that AFQT scores are a good measure of intelligence and are not undermined by strategic behavior on the test. Moreover, given the requirements of the military, our sample of men is somewhat positively selected in terms of academic performance.

Our main outcome measure is total annual labor earnings from employment and self-employment as reported to the tax authorities.[23] We observe 152,269 men aged 25–35 with nonzero earnings in at least 1 year from 2001 to 2011.

Table 1 presents descriptive statistics for our estimation sample, according to service status and a 5% representative sample of men. Within our estimation sample, differences by service status are small. In comparison to the general population, our fit-for-service sample differs most according to AFQT scores of 3 points higher—an unsurprising result, as AFQT score is an explicit selection criterion—but otherwise differences are also small. Earnings in the age range 25–35 are remarkably similar in our estimation sample, both by service status and in the general population.

As the tests on the AFD take place before the lottery, we can use the test results, together with other preassignment variables, to assess whether the lottery randomization is balanced. Table 2 shows coefficients from four separate ordinary least squares (OLS) regressions explaining draft assignment by the lottery and, as expected, no covariates predict assignment status, confirming that the lottery is a balanced random assignment.[24]

IV. Method

We want to know the effect of military service on subsequent earnings, which we model as follows:

$$y_{it} = \pi_0 + \pi_1 MILITARY_i + X_i\pi_2 + \upsilon_{it}, \tag{1}$$

where y_{it} refers to the earnings of individual i at time t, $MILITARY$ is an indicator of participating in military service, and X is a set of control variables. However, an OLS estimate of π_1 would be biased because of noncompliance with the draft, making military service status endogenous. To deal with this endogeneity problem, we exploit the draft lottery and instrument $MILITARY$ according to

$$MILITARY_i = \delta_0 + \delta_1 LOTTERY_i + X_i\delta_2 + \eta_i. \tag{2}$$

[23] The standard earnings measure that we use includes sickness benefits and paternity leave benefits. In sensitivity checks, we exclude these benefits.

[24] To complement the draft status balancing check of table 2, in table A2 we perform a balancing check for lottery numbers. It also shows that preassignment characteristics have no explanatory power.

Table 1
Summary Statistics

	Estimation Sample			
	All (1)	Served = 1 (2)	Served = 0 (3)	5% Population (4)
Height (cm)	180.38	180.39	180.37	179.93
	(6.59)	(6.52)	(6.64)	(6.77)
AFQT	44.61	44.51	44.69	41.28
	(8.32)	(8.09)	(8.50)	(10.30)
Individuals	152,269	66,813	85,456	7,486†
Raised in single-parent family	.18	.18	.17	.19
	(.38)	(.39)	(.38)	(.39)
Placed in out-of-home care	.04	.04	.03	.05
	(.19)	(.21)	(.18)	(.22)
Son of immigrant	.04	.04	.04	.04
	(.20)	(.19)	(.20)	(.20)
Birth weight (g)	3,371	3,359	3,381	3,342
	(653)	(647)	(657)	(599)
Household income at age 15 (DKK)	134,047	132,738	135,071	130,579
	(57,582)	(56,377)	(58,487)	(56,554)
Mother's years of schooling	11.67	11.58	11.74	11.69
	(2.88)	(2.84)	(2.91)	(2.80)
Father's years of schooling	12.04	11.95	12.11	12.19
	(3.17)	(3.13)	(3.20)	(3.04)
Individuals	152,269	66,813	85,456	14,390‡
Earnings age 25–35 (DKK)	303,169	302,377	303,796	302,576
	(168,233)	(164,196)	(171,358)	(164,690)
Observations	1,068,599	472,117	596,482	101,404‡

NOTE.—Means, standard deviations in parentheses. Statistics in cols. 1–3 relate to fit-for-service 1976–83 male birth cohorts; statistics in col. 4 indicated by a dagger are for 1988–90 male birth cohorts attending the Armed Forces Day (AFD); statistics in col. 4 indicated by a double dagger are for full 1976–83 male birth cohorts. The Armed Forces Qualification Test (AFQT) is taken on the AFD, and height is measured. Birth weight is measured by midwife at birth. Raised in single-parent family is an indicator variable for household status on 17th birthday. Placed in out-of-home care is an indicator variable taking the value of 1 for living in out-of-home care (institutions or foster home) before age 18. Household income at 15 is equivalized according to the following formula: (sum of income in the household plus transfers minus taxes)/(1 × first_adult + 0.7 × second_adult + 0.5 × number_of_children) and deflated to 2012 prices by the consumer price index. Mother's and father's schooling are measured when son is age 15. Annual labor earnings, conditional on being positive, are deflated to 2012 DKK and include sickness and leave benefits. See table A1 for descriptive statistics of additional outcomes and controls.

Here, $LOTTERY_i$ refers to an indicator variable for drawing a lottery number below the threshold and thus being assigned to military service. Our IV estimator provides a local average treatment effect (LATE), which reflects the effect of military service among the group of compliers—that is, in our case men who would serve if randomly assigned to do so but who would otherwise not have volunteered. The effect on draft compliers is of interest for measuring the cost of forced service.[25]

[25] The risk of being drafted may impose psychological costs associated with planning uncertainty. We do not consider these psychological costs.

Table 2
Randomization Balance Check for Draft Assignment (Dependent Variable: Draft Status = 1)

	Total Sample (Including Zero Earnings)		Estimation Sample (Excluding Zero Earnings)	
	(1)	(2)	(3)	(4)
Height (cm)	−.00598	−.00817	−.00577	−.00800
	(.00648)	(.00681)	(.00656)	(.00689)
Height^2	.00002	.00002	.00002	.00002
	(.00002)	(.00002)	(.00002)	(.00002)
AFQT	.00086	.00087	.00081	.00095
	(.00119)	(.00122)	(.00120)	(.00124)
AFQT^2	−.00001	−.00001	−.00001	−.00001
	(.00001)	(.00001)	(.00001)	(.00001)
Raised by single parent	−.00406	−.00354	−.00420	−.00349
	(.00299)	(.00308)	(.00304)	(.00312)
Placed in out-of-home care	−.00246	−.00188	−.00100	−.00066
	(.00586)	(.00613)	(.00606)	(.00632)
Nonimmigrant and nondescendant	.00559	−.00008	.00574	.00103
	(.00588)	(.00875)	(.00605)	(.00890)
Household income (TDKK)		.00205		.00273
		(.00333)		(.00340)
Mother schooling		−.00026		−.00031
		(.00047)		(.00047)
Father schooling		.00026		.00033
		(.00042)		(.00042)
Birth weight (kg)		.00142		.00186
		(.00531)		(.00537)
Birth weight^2		−.00003		−.00011
		(.00059)		(.00060)
F-statistic	1.03676	.64554	1.01620	.69897
F-statistic *p*-value	.40259	.80461	.41718	.75415
Partial R^2	.00005	.00005	.00005	.00006
Observations	155,750	148,723	152,269	145,663

NOTE.—The columns contain coefficients from different ordinary least squares regressions: cols. 1 and 2 for the sample including individuals with zero earnings, and cols. 3 and 4 for the estimation sample excluding individuals with zero earnings. The dependent variable is an indicator taking the value of 1 if the lottery draw was below the threshold and the individual was drafted. Unit of household income at age 15 is in thousand Danish kroner (TDKK). These regressions also control for birth year, birth month, half year of the AFD, and half year of potential service start. Standard errors in parentheses.

We are primarily interested in the effect of service across men with different labor market prospects. Neal and Johnson (1996) show that AFQT scores in the United States are a good proxy for labor market prospects because they constitute a basic skills measure predicting job performance. We perform separate regressions by AFQT score quartiles, and in several robustness checks we consider other proxies for labor market prospects (measured on the AFD or earlier) and slice the data accordingly.

V. Results

We begin our analyses by estimating the mean relationships among draft status, service status, and earnings, before presenting our main estimates of service effects on earnings by labor market prospects. Then we investigate explanatory mechanisms for earnings effects via schooling disruption and work experience. Finally, we consider the effects of military service on a range of outcomes.

A. Draft Status, Military Service Status, and Earnings

Table 3 presents regression coefficients for the relationships among draft status, service status, and earnings. In panel A, we show coefficients on an indicator for service in separate OLS regressions explaining log annual labor earnings. Estimates are stable across specifications, showing that those serving earn 1.5%–1.9% more than those who did not serve.

In panel B, we turn to our causal analysis and show reduced-form, or intention-to-treat (ITT), estimates of the effect of draft status on log earnings. Those drafted earn 1.3%–1.7% less than those not drafted, reflecting a negative effect of serving for those forced to serve through the lottery assignment.

To estimate the LATE of serving, we need to relate the ITT estimate to the fraction of compliers. First-stage estimates of the effect of draft status on actual military service are shown in panel C. Being drafted increases the probability of service by 52 percentage points, and *F*-statistics show that draft status is very relevant for explaining service status (Stock, Wright, and Yogo 2002).

Panel D shows IV estimates of the effect of service on log earnings. In all specifications, military service decreases earnings, with an effect ranging from 2.5% to 3.2%.[26] These estimates for 8 months of service compare with 15% losses for white US veterans 10 years after serving for 2 years in the Vietnam War (Angrist 1990) and with 5% losses for Dutch men 10 years after 14 months of peacetime service (Imbens and van der Klaauw 1995).

While the contrast between the negative causal effects of service on earnings in panel D and the positive associations in panel A is striking, a similar upward bias for OLS relative to IV has been found elsewhere (i.e., while OLS estimates showing the associations are positive, IV causal effects are negative). Angrist et al. (2011) also find negative short-run earnings effects of service for white US Vietnam War veterans but with positive associations, and Angrist and Krueger (1994) find positive earnings associations for US World War II veterans but with insignificant causal effects.

[26] In table A3, we include draft status and lottery number as instruments for military service, finding estimates of the effect of military service on earnings very similar to those obtained when we instrument only by draft status. In the remainder of the paper, we use a binary draft status indicator as our only excluded instrument.

Table 3
Draft Status, Military Service Status, and Earnings

	No Controls (1)	Basic (2)	Extended I (3)	Extended II (4)	Extended III (5)
	A. OLS Regressions: Outcome Log Earnings				
Service status = 1	.0170***	.0188***	.0149***	.0169***	.0147***
	(.00386)	(.00393)	(.00394)	(.00392)	(.00397)
Adjusted R^2	.0000520	.0456	.0470	.0513	.0534
	B. Reduced-Form Regressions: Outcome Log Earnings				
Draft status = 1	−.0169***	−.0129***	−.0128***	−.0135***	−.0150***
	(.00389)	(.00418)	(.00418)	(.00416)	(.00421)
Adjusted R^2	.0000509	.0456	.0470	.0513	.0534
	C. First-Stage Regressions: Outcome Service Status = 1				
Draft status = 1	.522***	.518***	.518***	.518***	.517***
	(.00234)	(.00254)	(.00253)	(.00253)	(.00258)
Adjusted R^2	.272	.294	.296	.297	.296
F-statistic, excluded instrument	49,569	41,693	41,894	41,926	40,105
Mean of dependent variable	.442	.442	.442	.442	.442
	D. Second-Stage IV Regressions: Outcome Log Earnings				
Service status = 1	−.0323***	−.0249***	−.0247***	−.0261***	−.0289***
	(.00746)	(.00808)	(.00806)	(.00803)	(.00813)
Mean of dependent variable	12.30	12.30	12.30	12.30	12.30
Individuals	152,269	152,269	152,269	152,269	145,663
Observations	1,068,599	1,068,599	1,068,599	1,068,599	1,026,441

NOTE.—Each cell contains the coefficient of interest from different regressions. The dependent variable in panels A, B, and D is log annual labor earnings during 2001–11 in age range 25–35, deflated to 2012 DKK and including sickness and leave benefits. Columns differ according to the set of other explanatory variables included. Column 1 has no additional covariates. Column 2 includes controls for birth year, birth month, half year of the Armed Forces Day, and half year of potential service. While half year of potential service start has an element of choice because of educational deferment, this date is set before lots are drawn, so it is predetermined. Column 3 also includes Armed Forces Qualification Test score, height, and their square. Column 4 also includes controls for being raised in a single-parent family, placed in out-of-home care, and having nonimmigrant parents. The fullest specification, col. 5, also includes birth weight, household income at age 15, and mother's and father's years of schooling. Standard errors clustered at the individual level in parentheses. IV = instrumental variable; OLS = ordinary least squares.

*** $p < .01$.

Positive selection into the military on health grounds can better explain bias in the US context, where health checks take place after the draft rather than before it, as in Denmark. Nevertheless, compared with men who are drafted but do not serve, Danish volunteers must have characteristics sufficiently more favorable in the civilian labor market to outweigh the negative causal effects of those who comply with the draft. Surveys of individual motivations for serving in the military in the United States (Eighmey 2006) and Denmark (Lyk-Jensen and Glad 2018) highlight the importance of

challenge, responsibility, fidelity, and dignity—motivations that the civilian labor market is likely to value.

Our results are robust to earnings definitions (table A4; tables A1–A20 are available online), using discounted earnings (table A5), restrictions on service length (table A12), and proximity of lottery draw to the draft cutoff (table A7).

B. Heterogeneous Effects by Labor Market Prospects

We investigate heterogeneous service effects across the AFQT score distribution and across the distribution of other measures of labor market prospects.

Table 4 presents estimates of the relationship among draft status, service status, and earnings, by quartile of the AFQT score distribution. Panel A clearly shows that the positive mean OLS coefficient from table 3 is driven by those with AFQT scores in the second and third quartiles. Panel B shows reduced-form effects of draft status across the AFQT score distribution,

Table 4
Draft Status, Military Service Status, and Earnings by Armed Forces Qualification Test (AFQT) Score Quartile

	First Quartile (1)	Second Quartile (2)	Third Quartile (3)	Top Quartile (4)
	A. OLS Regressions: Outcome Log Earnings			
Service status = 1	.00372	.0257***	.0245***	.00491
	(.00774)	(.00773)	(.00810)	(.00791)
Adjusted R^2	.016	.036	.065	.106
	B. Reduced-Form Regressions: Outcome Log Earnings			
Draft status = 1	.00654	−.00716	−.0153*	−.0370***
	(.00822)	(.00820)	(.00862)	(.00835)
Adjusted R^2	.016	.036	.065	.107
	C. First-Stage Regressions: Outcome Service Status = 1			
Draft status = 1	.497***	.524***	.531***	.524***
	(.00493)	(.00496)	(.00531)	(.00502)
F-statistic, excluded instrument	10,156	11,139	10,018	10,870
	D. Second-Stage IV Regressions: Outcome Log Earnings			
Service status = 1	.0132	−.0137	−.0288*	−.0706***
	(.0165)	(.0157)	(.0162)	(.0160)
Observations	291,453	277,032	234,842	265,272
Individuals	41,983	39,371	33,156	37,759

NOTE.—Each of the four columns contains the coefficient of interest from different regressions, according to quartiles of the AFQT score distribution for those who drew a lottery number on the Armed Forces Day. The dependent variable is log annual labor earnings during 2001–11 in age range 25–35, deflated to 2012 DKK and including sickness and leave benefits. The specification is the same as for col. 3 in table 3. Standard errors clustered at the individual level in parentheses. IV = instrumental variable; OLS = ordinary least squares.

* $p < .10$.

*** $p < .01$.

with the draft decreasing earnings by 3.7% for top-quartile AFQT scores. This result provides the first evidence that the opportunity cost of being drafted is greater for high-ability men. Panel C shows that first-stage estimates of the effect of draft on service are similar across the AFQT score distribution, and *F*-test statistics show that draft status is a very relevant instrument for service status across the entire AFQT score distribution.

The IV estimates in panel D show a clear gradient in the effect of serving by AFQT score quartile. The effects are positive and insignificant at the bottom and negative and significant at the top. We thus obtain no evidence that low-ability men, with poor labor market prospects, would increase their earnings by being assigned to military service. However, men at the higher end of the ability distribution face an earnings penalty from being forced to serve, with men in the third quartile of the AFQT score distribution losing 3% of earnings and men in the top quartile losing 7%.

IV estimates compare compliers who serve with compliers who do not serve, whereas OLS estimates also include always-takers and never-takers in the comparison (Angrist, Imbens, and Rubin 1996). Across the AFQT score distribution, behavioral response to the lottery affects sample selection for OLS but not for IV; there is a modest AFQT score gradient for selection but not for compliance with draft assignment (see fig. A1; figs. A1–A3, B1–B3 are available online).[27] The divergence between IV and OLS estimates increases with AFQT scores and is driven by those who serve: compliers who serve suffer a growing earnings penalty relative to always-takers (see fig. A2).

We now consider whether other correlates of labor market prospects also reflect differences in the opportunity costs of serving across individuals. In table 5 we examine the effect of serving across quartiles of parental education and household income at 15, birth weight, and height measured at the AFD. Although estimates of earnings effects are not monotonic across quartiles of the distribution of other correlates of labor market prospects, for all of these measures the earnings penalties are significant for men in the top quartile, and earnings effects are insignificant for those in the bottom quartile. Thus, our results appear quite robust to different ways of distinguishing between individuals with high and low opportunity costs.[28]

[27] These relatively small differences in complier types across the AFQT score distribution are supportive of the stable unit treatment value assumption with respect to noninterference, in that we would expect only modest changes in the AFQT score distribution of those serving when changing military recruitment from conscription to an all-volunteer force. With this change in the recruitment system, neither the civilian labor supply nor the level of civilian earnings for high-ability men is likely to change very much.

[28] When we look for heterogeneity between distinct, potentially vulnerable groups rather than across continuous proxies for labor market prospects—sons of immigrants, those raised in out-of-home care, or those raised in single-parent families—we find insignificant effects. However, sample sizes are rather small, resulting in less precision (see table A8).

Table 5
Effects of Military Service on Earnings across Quartiles of the Distributions of Background Characteristics

Outcome log earnings	First Quartile (1)	Second Quartile (2)	Third Quartile (3)	Top Quartile (4)
	A. Across Household Income Quartiles			
Service status = 1	.0172	−.0379**	−.0105	−.0696***
	(.0167)	(.0159)	(.0154)	(.0164)
Observations	272,079	278,709	266,739	249,045
Individuals	37,579	38,088	38,188	38,101
F-statistic, excluded instrument	10,147	10,373	10,628	10,829
	B. Across Father's Education Quartiles			
Service status = 1	.00341	−.0222	−.0160	−.0617***
	(.0164)	(.0148)	(.0165)	(.0166)
Observations	279,451	301,072	226,083	259,966
Individuals	39,558	43,223	31,555	37,620
F-statistic, excluded instrument	10,337	12,150	8,545	10,981
	C. Across Mother's Education Quartiles			
Service status = 1	−.000529	−.00544	−.0180	−.0692***
	(.0164)	(.0159)	(.0151)	(.0170)
Observations	273,937	262,599	275,408	254,628
Individuals	37,847	38,456	38,444	37,209
F-statistic, excluded instrument	9,807	10,112	11,201	10,840
	D. Across Birth Weight Quartiles			
Service status = 1	−.0141	−.00447	−.0505***	−.0346**
	(.0133)	(.0219)	(.0159)	(.0176)
Observations	378,960	143,105	276,368	230,500
Individuals	47,644	25,483	36,923	35,983
F-statistic, excluded instrument	14,537	6,090	10,703	9,044
	E. Across Height Quartiles			
Service status = 1	−.0213	−.00919	−.0212	−.0528***
	(.0151)	(.0163)	(.0154)	(.0180)
Observations	295,206	253,177	292,512	227,704
Individuals	42,096	35,966	41,699	32,508
F-statistic, excluded instrument	12,570	10,289	11,294	7,880

NOTE.—The sample is split according to quartiles of various predetermined background characteristics. Different background characteristics are considered in each panel. Each cell contains the coefficient of interest from separate second-stage instrumental variable regressions. The explanatory variable of interest is military service. Specification is the same as for col. 3 in table 3. Standard errors in parentheses.
** $p < .05$.
*** $p < .01$.

Our findings of an opportunity cost gradient correspond to the findings of Card and Cardoso (2012), who found that low-educated Portuguese men have increased earnings due to service compared with higher-educated men, and to findings for the United States that white men suffer short-run earnings losses compared with nonwhite men (Angrist and Chen 2011) and

that high school dropouts have more disability insurance receipt compared with high school graduates because of Vietnam War–era military service (Angrist, Chen, and Frandsen 2010).

Our results thus far concern earnings at ages 25–35. We also estimate earnings effects for younger ages (see table A13) and age by age (see fig. A3). We find a positive spike at age 21 in the earnings effect, a spike driven by men in the top quartile of AFQT scores, who earn more from the military than they otherwise would have made from part-time work while studying. The mean effect of service on earnings becomes insignificant after age 28, and the earnings effect for top-quartile AFQT scores becomes insignificant after age 30.[29] This result is consistent with delayed educational enrollment and educational attainment followed by eventual educational catch-up.[30]

Even though there is no significant causal effect of military service on earnings across the AFQT score distribution after the age of 30, the lifetime earnings of men who served has been reduced. For men in the top quartile of AFQT scores, earnings lost in the age range 25–35 due to military service are estimated to total $23,000 (the annual mean earnings for men with top-quartile AFQT scores is $43,500).[31] Annualized earnings while conscripted are $30,000, and for men with top-quartile AFQT scores, conscripted earnings would need to double to compensate for lifetime earnings losses.[32]

[29] Earnings at age 35 are observed only for the 1976 birth cohort; earnings at 34 are also observed for 1977 births.

[30] Splitting the sample by years of earnings observations (table A9) and by birth cohorts (table A10), we find that earnings penalties are lower for more recent years but do not differ significantly across cohorts. That the reduction in penalty size is greater across years of observation rather than across cohorts is consistent with a higher proportion of early calendar year observations of earnings being for younger men.

[31] Annual earnings for men in the top quartile of AFQT scores aged 25–35 are DKK 304,413, which converts to $43,500 with an exchange rate of 7 DKK per $1. Discounting earnings losses of 7% at an interest rate of 5% back to age 25 sums to $26,500. In table A13, we show that earnings in the age range 19–24 for men in the top quartile of AFQT scores are an insignificant 2% lower because of serving. Annual earnings for men in the top quartile of AFQT scores aged 19–24 are DKK 126,920 (about $18,000). Compounding earnings losses of 2% at an interest rate of 5% forward to age 25 sums to $2,600. Food and lodging is provided during service, expenses that would otherwise probably constitute a budget share of 0.7; service lasts two-thirds of a year; and income tax is about 40%. Thus, we can infer that food and lodging is worth 0.28 of annual earnings, or $6,400. Most men serve at age 20, and compounding the food and lodging subsidy to age 25 yields $5,750. In sum, the earnings penalties of $26,500 and $2,600, less the food and lodging subsidy of $6,400, gives a total loss of $22,700 at age 25. Similar calculations for the third quartile of AFQT scores give a total loss of $5,400.

[32] Angrist (1990) found an annual $7,000 earnings penalty 10 years after 2 years of Vietnam War service. Hubers and Webbink (2015) find a persistent annual $2,000 earnings penalty for 14–16 months of peacetime military service in the Netherlands.

Our results by AFQT score quartile are robust to earnings definitions (table A4), using discounted earnings (table A11), restrictions on service length (table A12), and proximity of lottery draw to the draft cutoff (not shown).

C. Explanatory Mechanisms

Several mechanisms may explain the estimated effects across the ability distribution. An explanatory mechanism that we cannot explore is that military service may build human capital by teaching certain vocational skills (see Hanes, Norlin, and Sjöström 2010). Heterogeneous results for men of different ability groups could then result if these groups acquire different skills while serving in the military or if these groups later choose civilian careers that differ in their use of military-induced skills. We now consider several other mechanisms in turn: educational enrollment and attainment as well as work experience.

We first consider the effect of military service on years of schooling attained at different ages.[33] Serving in the military could mean that studies are not started, are interrupted, or are abandoned. Educational deferment is requested, and the potential service date is agreed on before lots are drawn. Therefore, deferments and draft status are independent. As almost everyone has served before age 25, we can estimate the causal effect of service on schooling from age 25.

Table 6 shows that serving significantly reduces schooling attainment at age 25, especially for men in the top quartile of AFQT scores (panel A), but that by age 30 effects are significant only for the top quartile (panel B). However, these effects are small and can only partially account for lower earnings.[34]

Another potential explanation for the earnings penalty is that some men are still studying when we measure their earnings—that is, they would have zero or very low earnings, possibly explaining part of the earnings penalty. While military service is negatively associated with missing earnings, serving does not cause earnings to be missing at the mean (table A15) or across the AFQT score distribution (table A16). Neither is the earnings penalty driven by very low earnings: estimates are robust to excluding the lowest 5% of earnings (table A17).

Panels C–E of table 6 show small and significant effects of military service on the probability of being enrolled in education, effects that fall with age and are concentrated in the top quartile of AFQT scores. These enrollment results

[33] We have also checked whether heterogeneity by AFQT scores is affected by excluding men who were not enrolled in education at age 17 or 18. The concern is that the level of schooling at age 18 might partly determine AFQT scores and could then explain some of the heterogeneity. However, when we exclude those not enrolled, the results are almost unchanged (not shown).

[34] Estimates of education effects are robust to the exclusion of half years of AFD and half years of potential service from the set of controls (table A14).

Table 6
Effect of Military Service on Schooling Attainment and Enrollment, by Armed Forces Qualification Test (AFQT) Score Quartile

	Full Sample (1)	First Quartile (2)	Second Quartile (3)	Third Quartile (4)	Top Quartile (5)
	A. Years of Schooling at Age 25				
Service status = 1	−.168***	−.127***	−.163***	−.198***	−.201***
	(.0165)	(.0352)	(.0316)	(.0326)	(.0310)
	B. Years of Schooling at Age 30				
Service status = 1	−.0332	−.0127	−.0406	−.0231	−.0782*
	(.0219)	(.0407)	(.0419)	(.0468)	(.0452)
	C. Studying at Age 25				
Service status = 1	.0247***	.0120*	.0281***	.0154	.0394***
	(.00455)	(.00636)	(.00857)	(.0105)	(.0106)
	D. Studying at Age 28				
Service status = 1	.0207***	.00578	.0164**	.0292***	.0315***
	(.00396)	(.00557)	(.00729)	(.00904)	(.00951)
	E. Studying at Age 30				
Service status = 1	.0108***	.00633	.00699	.00592	.0232***
	(.00324)	(.00474)	(.00599)	(.00719)	(.00781)
Observations	152,269	41,983	39,371	33,156	37,759
F-statistic, excluded instrument	46,019	11,453	12,054	10,758	11,925

NOTE.—Each cell contains a coefficient from a different instrumental variable regression. The specification is the same as for col. 3 in table 3. The explanatory variable of interest is an indicator taking the value of 1 if the individual served in the military. The dependent variable is years of educational attainment by age 25 in panel A and by age 30 in panel B. For panels C–E, the outcome is an indicator variable taking the value of 1 for enrollment in education at the age shown in the column header and 0 otherwise. The samples are split into four columns according to quartile of the AFQT score distribution. In our sample, we observe 13% studying at age 25, 8% at 28, 4% at 30, and 1% at 35. Standard errors in parentheses.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

suggest that part of the earnings penalty among the high-ability group results from schooling being delayed, resulting in a number of years with lower pay.

Because in Denmark military service can be deferred but not avoided, our results contrast to findings from France (Maurin and Xenogiani 2007), Germany (Bauer et al. 2014), and the United States (Card and Lemieux 2001), where conscription increases enrollment in education in order to avoid military service. However, our results are in line with Italian findings (Cipollone and Rosolia 2007), where men from regions exempted from the draft have longer education because of less disruption in their studies.

As for the relationship between service and work experience, serving in the military for 8 months is associated with 2 months more work experience at the mean (table A18) and 3 months more work experience for men with

above-median AFQT scores (table A19).[35] However, IV estimates show no causal effect of military service on total work experience; military experience completely crowds out civilian labor market experience, resulting in a zero net total experience effect. That there is no AFQT score gradient in the total experience effect suggests that lost civilian labor market experience is not driving the earnings penalty. In contrast, Angrist and Chen (2011) find that lost experience following US Vietnam War–era service offset the educational gains made through the Servicemen's Readjustment Act (commonly known as the GI Bill), yielding no earnings effects 25 years after service.

D. Other Outcomes

To shed further light on possible mechanisms underlying our main results, we extend our analysis in this section by looking at the effect of military service on a range of alternative outcomes, including unemployment, crime, bank balance, and health care use. Table 7 summarizes the findings of the effect of service on these alternative outcomes.[36]

In panel A, the outcome measures the proportion of the year during which unemployment insurance benefits were received. Service causes a modest reduction in unemployment, a reduction that is most pronounced at the lower and upper AFQT score quartiles. Thus, unemployment cannot explain the negative effect of service on earnings for the top AFQT score quartile. Panel B shows a positive average effect on bank balances, an effect strongest for the top quartile of AFQT scores, where serving leads to an increase in bank balances by 8,880 DKK (about $1,250). Either men may be able to save more while serving than otherwise or serving might delay house purchase and associated loans.[37]

Panel C shows that effects on crime are small and insignificant: this finding is in contrast to the negative effects found for Argentina (Galiani, Rossi, and Schargrodsky 2011) and Sweden (Hjalmarsson and Lindquist 2016) but is in line with other Danish studies finding no mean effect (Albæk et al. 2017; Lyk-Jensen 2018).

In panels D–G, we consider military service effects on a number of health outcomes by AFQT score quartile, finding only occasional significance for

[35] When we observe all cohorts, work experience is calculated as the sum of mandatory pension contributions made per day in the military and the civilian labor market in the age range 18–30. Experience effects in col. 1 of table A18 are significant because these estimates do not control for birth cohort in that specification.

[36] We have also included the preassignment values of bank balance, crime, and health outcomes as additional controls in our balancing checks. None of these predict assignment to military service.

[37] A bank balance increase of $1,250 corresponds to our calculated compounded gain by age 25 of $3,800 ($6,400 imputed value of food and lodging minus $2,600 earnings losses 19–24). as described in n. 31.

Table 7
Effect of Military Service on Various Outcomes, by Armed Forces Qualification Test (AFQT) Score Quartile

	Full Sample (1)	First Quartile (2)	Second Quartile (3)	Third Quartile (4)	Top Quartile (5)
	A. Unemployment = 1 at Age 26				
Service status = 1	−.00422***	−.00688**	−.00247	−.00285	−.00430*
	(.00136)	(.00316)	(.00265)	(.00267)	(.00227)
	B. Bank Balance in DKK at Age 26				
Service status = 1	4,213.1*	7,077.6*	−1,577.2	1,827.0	8,870.2**
	(2,169.6)	(4,293.4)	(4,362.5)	(4,526.3)	(4,168.0)
	C. Criminal Convictions = 1 for Age 26–35				
Service status = 1	.000373	.000422	−.00414	.00416	.00320
	(.00214)	(.00538)	(.00426)	(.00388)	(.00302)
	D. Hospitalization = 1 for Age 26–35				
Service status = 1	.00451	.00203	.0194**	−.00762	.00243
	(.00465)	(.00852)	(.00893)	(.00993)	(.00984)
	E. Prescription Medicine = 1 for Age 26–35				
Service status = 1	−.00129	.00723	−.00853	−.000965	−.00225
	(.00363)	(.00818)	(.00714)	(.00713)	(.00629)
	F. Psychiatric Diagnosis = 1 for Age 26–35				
Service status = 1	−.00238	−.00128	−.00348	−.00341	−.000551
	(.00211)	(.00502)	(.00406)	(.00396)	(.00353)
	G. Addiction Treatment = 1 for Age 26–35				
Service status = 1	−.00192*	−.00173	−.00363*	−.000856	−.000840
	(.00104)	(.00285)	(.00203)	(.00177)	(.00118)
Observations	152,269	41,983	39,371	33,156	37,759

NOTE.—Each cell contains the coefficient of interest from separate instrumental variable regressions. The explanatory variable of interest is always military service. Specification is the same as for col. 3 in table 3. The first column is for the entire sample; cols. 2–5 are for quartiles of the AFQT score distribution. Each panel corresponds to a different dependent variable. Standard errors in parentheses.

* $p < .10$.
** $p < .05$.
*** $p < .01$.

hospitalization and (treatment for) addiction problems and no significance for the probability of purchasing mental health medication or for being diagnosed with a psychiatric disorder.[38] As these health effects are insignificant

[38] Angrist, Chen, and Frandsen (2010) find that US Vietnam War–era service caused increased disability benefit receipt for low-skilled men, a finding they attribute to features of the veterans disability compensation program.

for the top quartile, they cannot explain the earnings penalties at the higher end of the ability distribution.[39]

VI. Conclusions

Many countries have compulsory military service, and recent papers use quasi-experimental variation to estimate the mean effect of peacetime military service on earnings and other outcomes. Our paper uses a draft lottery to estimate the effect of peacetime military service on a wide range of outcomes: earnings, schooling, health, and crime. We provide novel evidence on the opportunity costs of conscription by using random assignment in the Danish draft lottery to estimate the distribution of the effects of service by labor market prospects. If the army wants to recruit a force of equal size through a draft lottery, then the average opportunity costs for those serving would be higher than with in an all-volunteer system. The reason is that some high-opportunity-cost men would not volunteer at the all-volunteer wage rate but would nonetheless be forced to serve under the draft.

We identify a LATE for men who would not have served in the absence of the draft but are forced to do so, finding a mean earnings loss of 2.5% for ages 25–35 compared with if they had not served. Of a male cohort, 27% serve in the military, and our LATE is relevant for half of the men who serve.

The mean impact hides important heterogeneity. For low-ability men who are drafted and serve there is no earnings effect, but high-ability men face significant earnings penalties. Men in the top AFQT score quartile suffer a 7% earnings penalty if they are forced to serve. This penalty for high-ability men lasts until age 30, amounts to $23,000 in foregone lifetime earnings, and would require that earnings while conscripted double if they were to offset subsequent earnings losses. Moreover, earnings effects are similar across other preassignment measures of labor market prospects, from birth weight and height to parental schooling and household income, with significant earnings penalties for men with the best prospects and insignificant earnings effects for men with the worst prospects.

The main channel through which serving causes earnings losses appears to be educational career disruption for men in the top quartile of AFQT scores, with later enrollment and completion of studies. We find no effects on criminal convictions or measures of health care usage, either at the mean or across ability.

Our findings relate only to part of the opportunity cost of the draft lottery, as measured by subsequent civilian outcomes in the age range 25–35. Nevertheless, we find significant mean earnings losses driven by substantial losses for high-ability men with good labor market prospects who would

[39] Table A20 shows no important health effects across quartiles of the baseline health distribution, proxied by birth weight and height at the AFD.

not have volunteered. If technical change in the military requires fewer but higher-ability personnel and if such people were encouraged to volunteer through more competitive remuneration, then high-ability men and women facing the draft everywhere would benefit significantly.

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