Opportunity Cost and the Incidence of a Draft Lottery

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Abstract

Military conscription implicitly taxes draftees. Those who would have volunteered at the market wage may be forced to serve for lower wages, and those with higher opportunity costs will be forced to serve regardless. Yet little is known about the distribution of this burden. We exploit the Danish draft lottery to estimate the causal effect of peacetime military service on labor earnings of young men across the cognitive ability distribution. We find that high-ability men who are inducted face a USD 50,000 lifetime earnings penalty, whereas low-ability men face none. Educational career disruption is the main channel.

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> When the author speaks of impressing... by presenting to the mind one sailor only suffering a hardship... he places against this private mischief the inconvenience to the trade of the kingdom. But if, as I suppose is often the case, the sailor who [is] pressed and obliged to serve for the defense of this trade at the rate of 25s. a month could [earn] 3l. 15s. in the merchant's service, you take from him 50s. a month. (Benjamin Franklin 1818)[1]

> I got a letter from the government the other day. / I opened and read it, it said they were suckers. / They wanted me for their army or whatever. / Picture me giving a damn—I said never. /.../ I wasn't with it but just that very minute it occurred to me: The suckers had authority. (Public Enemy 1989)[2]

I. Introduction

The inequity of the military conscription tax has long been recognized. Franklin emphasized the income loss of impressed (i.e., conscripted) sailors by comparing earnings in the navy and merchant fleets. Those who would have volunteered at the market wage are paid lower conscription wages. In the Vietnam era, economists made important contributions to the debate about the merits of military conscription versus an all-volunteer force.[3] Friedman (1962) was an eloquent opponent of military conscription and economic analyses clarified the viability of an all-volunteer force.[4]

A salient feature of a military draft is that those who serve but would not have volunteered are paid a military wage below civilian opportunity cost. In this paper we measure an important part of the opportunity cost for draftees forced to serve—the distribution of subsequent earnings differences as civilians—and identify significant sources of heterogeneity. Of the two quotations with which we began this paper, the Franklin statement indicates that sailors would be willing to serve if the Navy paid them a fair wage—but it did not. In contrast, the Public Enemy statement makes clear that they would not want to serve for any wage. We focus on the Public Enemy statement: not only conscript versus volunteer earnings, but earnings differences for all reluctant draftees who serve.

Many men and women share the experience of military service. Mandatory military service exists in the majority of the world's countries, most of which are not involved in any armed conflict. The training is often extensive, ranging from a few months to several years.[5]

Compulsory military service entails an interruption in an important phase of young men's and women's career paths[6], a time when decisions about human capital investments and labor market entry are made. Albrecht et al. (1999) pinpoint three reasons why 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.

In this paper, we contribute to the literature on the labor market effects of military service in two ways. First, we provide new causal estimates of the effect of peacetime conscription on adult earnings. The Danish draft offers an excellent opportunity for providing such estimates. Upon turning 18 years, Danish men must participate in the "Armed Forces Day" (AFD) in which they are subject to a variety of tests and examinations. In the cohorts we consider, 63 percent are judged fit for military service. Part of each cohort is then randomly assigned to serve. We exploit this random assignment to estimate the causal effect of peacetime conscription[7] on labor earnings. Draft enforcement is such that only 8 percent of our cohorts do not comply with assignment to serve, and our Instrumental Variables (IV) estimation strategy deals with this non-compliance.

Second, we exploit an attractive feature of the Danish system, whereby the random assignment to serve takes place after the enlistment tests have been conducted. We have information on two pre-conscription factors ascertained during the AFD: Armed Forces Qualification Test (AFQT) scores and height. We can use this information and other pre-conscription characteristics from population registers to examine whether the effect of conscription on earnings varies across the population. We expect that the opportunity cost of military service is greater for men with better labor market prospects. Using the data on AFQT scores, we can test this hypothesis by estimating the effect of conscription across the ability distribution.[8] As robustness checks we also consider other correlates of labor market prospects: birth weight, height at the AFD, parental schooling, and household income at age 15.

In addition, we can investigate whether conscription has beneficial effects for particular groups of men, as some have argued (Berger and Hirsch 1983; Mangum and Ball 1989). Serving in the army may improve certain types of skills, such as discipline and teamwork, that may especially benefit disadvantaged youth. Angrist (1991) finds that Whites were more likely than non-Whites to enlist when at risk of being drafted. This differential response rate is explained by non-Whites being more likely than Whites to consider enlistment preferable to a civilian career. This finding is consistent with Angrist's (1990) finding that White veterans earn 15 percent less than White non-veterans, whereas non-White veterans have no earnings loss. We analyze whether conscription has a beneficial effect for sons of immigrants or for men who grew up in out-of-home care or in single-parent families.

We use data from the Danish military that includes draft lottery outcome and military service status for all Danish men born between 1976 and 1983, judged fit for service at the AFDs—155,570 in total. To this information, we have linked longitudinal administrative register data on educational qualifications, labor market outcomes, health care usage, and criminal convictions. We observe 152,269 men with non-zero earnings in at least one year over the age range 25-35. This data also allows us to investigate to what extent any earnings differences run through educational attainment, employment, health or criminal activity. Our paper relates to the broader literature that evaluates the effect of military service on various outcomes.

Men who self-select into the military may be different from other men. In order to estimate the effects of military service the empirical approach must deal with this selection. A series of papers have exploited draft lotteries in which individuals were randomized into service. Hearst et al. (1986) use the Vietnam-era U.S. draft to look at mortality; Galiani et al. (2011) use the Argentinian war and peacetime draft to look at crime; Siminski and Ville (2011) use the Vietnam-era Australian draft to look at mortality. Draft lotteries have been used to look at earnings effects of military service in the U.S. by Angrist (1990) and Angrist, Chen and Song (2011), and in Australia by Siminski (2013). The estimates from these studies reflect both the effect of career interruptions and the negative impact of going to war.

A number of studies have evaluated the effect of peacetime conscription. In this setting, lotteries have typically been unavailable, with researchers instead exploiting alternative research designs to estimate causal effects. Grenet et al. (2011) and Bauer et al. (2012) use regression discontinuity designs based on birth date cutoffs and find no effect of military service on earnings among British and German men. A positive effect on earnings among low-educated Portugese men was obtained by Card and Cardoso (2012), who use information on pre-conscription earnings to control for non-random selection into the military. In contrast, Imbens and van der Klauuw (1995) found a negative effect on earnings, exploiting policy-induced variation in enrollment rates year to year in the Netherlands. Given that the findings in this literature are as varied as the research designs, more evidence on the topic is clearly needed for understanding the costs and benefits of conscription.[9]

Our results suggest a negative mean impact of military service on earnings. On average, drafted men who were serving in the military but who otherwise would not have volunteered earn about 2.5 percent less at ages 25-35 than they would have had they not served. This mean impact hides important heterogeneity. Allowing the effect to vary across the AFQT score distribution, we find no effect among low-ability men but large effects for those with high ability. At the top quartile of the AFQT score distribution, we find that men who are drafted and served but otherwise would not have volunteered earn 7 percent less than they would have had they not served. This penalty for high ability men lasts until age 32 and amounts to USD 50,000 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 15.

We also show that part of the effect among high-ability men is likely to run through reduced educational attainment. We find that even at age 30 men assigned to serve have completed fewer years of schooling and are more likely to be enrolled in an ongoing education. In addition, we show that conscription reduces the risk of subsequent unemployment. We find no effect of conscription among potentially vulnerable subgroups: sons of immigrants or those growing up in out-of-home care or a single-parent family. Our findings contribute to the discussion about the merits of military conscription versus an all-volunteer force. The results suggest that conscription can lead to high costs, especially for high-ability men. Such costs are not clearly revealed by studies focusing only on average effects.

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

II. Military Conscription in Denmark

Denmark reintroduced conscription after World War II. The induction of young men for military service is randomly determined through a lottery held on the AFD. On their 18th birthday all men and women are invited to attend an AFD on a date 3-9 months later. There are 200 AFDs over the year and at each of the 6 regional military recruitment centers, 40-50 men and women are assessed during 5-6 hours.[10]

The length of military service for most of our sample period is eight months.[11] Service is performed in the army (82 percent), navy (7 percent), air force (5 percent), civil defense, fire & rescue services (altogether 6 percent for the last three). In 2012 conscripts received accommodation and food, a monthly taxable salary of 7,421 DKK (1,337 USD) and a tax-free monthly allowance of 6,230 DKK (1,150 USD). [12]

A. Conscription Procedure

The flowchart in Figure 1 illustrates the military conscription procedure. Before participating in the AFD, all prospective draftees submit a health questionnaire that forms the basis of a health assessment. About 10 percent of a cohort is declared unfit for military service and therefore ineligible for the draft, in advance, by documentation of serious somatic or psychiatric disorders (Hageman, Pinborg, & Andersen, 2008). They are not called to the AFD. On the AFD, prospective draftees undergo a medical examination, a psychological evaluation, and complete an AFQT.

From these assessments, about 70 percent (63 percent of each cohort) are declared fit for military service and must participate in the draft lottery. They draw from a drum filled with lottery numbers ranging from 1 to 36,000.[13] The data we have access to is for those judged fit for service, and in our analysis we consider only those who were judged fit for service (eligible) and who drew a lottery number at the end of the AFD.[14] We refer to men who drew a number below the assignment threshold as *drafted*, regardless of whether they served.

[Figure 1 about here]

After the AFD, those who are drafted will be called for military service beginning 7-18 months later. The service date may be delayed because of employment abroad or continuing an education, but service must commence before turning 32.[15] In our sample, 99.6 percent had completed their military service by age 25. We refer to men age 18-24 and who start military service as having *served*, regardless of whether they were drafted.

B. Enforcing the Draft

Figure 1 shows the conscription process, with four possible exits to civilian life. The fit-for-service criteria obviously mean that men entering the draft lottery are not representative of the general population. Our inference is on the basis of random assignment conditional on being fit for service. Describing the selection process helps understanding this conditioning and interpreting our findings.

As previously mentioned, on grounds of disability 10 percent of a cohort is not required to attend the AFD. Disability needs to be certified by a consultant physician at a regional public hospital. For recent cohorts, we can see AFD attendance across the distribution of Grade Point Average (GPA) scores in mandatory 9th grade tests taken at the end of compulsory schooling. Average AFD attendance is 96 percent for those with a 9th grade test score, rising from 93 percent in the bottom GPA quartile to 98 percent in quartiles 2-4 (see appendix figure A1). This gradient in AFD attendance by GPA suggests that learning difficulties, rather than outsmarting the draft board, are the main cognitive grounds for not attending the AFD.

At the AFD, 27 percent of a cohort are found unfit for service on the grounds of low AFQT (10 percent), high BMI (10 percent) or low BMI (5 percent). The remaining 2 percent are unfit because of medical conditions, the top three being: ADHD, musculoskeletal disorders and asthma. For recent cohorts we can compare AFQT *pass rates* with mandatory 9th grade test scores. The pass rate increases rapidly from 60 percent in the bottom quartile of 9th grade test scores to 90 percent in the top quartile (see appendix figure A2). This strong association between academic achievement and passing the cognitive ability test is supportive of the validity of AFQT-taking.

Among those who are fit for service, 8 percent are drafted but do not serve: 3 percent are conscientious objectors who are required to work for the local municipality in care facilities for children and the elderly, hospitals, libraries, etc. This civil service is of similar 8 months duration, and the rate of pay is the same. Of the remaining non-compliers, 4 percent do not serve on the grounds of subsequent poor health, 0.5 percent is excluded because of a criminal record[16], and 0.5 percent is sanctioned under the draft law, get a criminal record, and are fined or spend time in a correctional facility.

Selecting men who are fit for service and encouraging these men who are drafted to serve in the military is an exercise in enforcing the law. For the 1976-83 cohorts in our analysis the needs of the military were such that 63 percent were judged fit for service, 28 percent were drafted, and 20 percent served (plus 7 percent who were not drafted volunteered to serve). Selection out of the military on the grounds of prior medical conditions, failing AFD tests, and draft assignment non-compliance is controlled according to well documented criteria. The selection process into being fit for service determines the conditioning of our analysis—the data we have access to, is only for those who are fit for service. Enforcement of the draft determines compliance, which is dealt with in our IV estimation strategy.

C. Compliance

In our sample of men declared fit for military service, and who participated in the draft lottery, 44 percent are drafted and 43 percent serve. However, of those drafted, 27 percent do not serve; and of those not drafted, 21 percent serve. Angrist, Imbens and Rubin (1996) distinguish four response types relative to assignment to a treatment. First, when told what to do, some individuals will always comply ("compliers"). Second, in contrast, some will always do the opposite ("defiers"). Third, regardless of the instructions, some will always receive the treatment ("always-takers"). Fourth, regardless of instructions some will never receive the treatment ("never-takers").

While we can identify those who serve but were not drafted as volunteers, we cannot know among those drafted who would have volunteered. Randomization guarantees that draft assignment is independent of response type. Assuming there are no defiers, this independence enables us to calculate the sample proportion of never-takers as P(served=0|drafted=1) = 0.27, the sample proportion of always-takers as P(served=1|drafted=0) = 0.21, and together the sample proportion of always takers and compliers as P(served=1|drafted=1) = 0.73. Hence the difference between the latter two is the sample proportion of compliers, 0.52 (Imbens and Rubin, 1997). Of a full male cohort, 27 percent serve in the military, 36 percent are judged fit for service but do not serve, and 37 percent are unfit for service. We can infer that 16 percent of a cohort serve because they were drafted but otherwise would not have volunteered. Similarly, 11 percent of a cohort volunteer or would have volunteered if they had not been drafted.

Figure 2 shows the frequency distribution of service age by draft and response status. Most of those who serve were drafted, as shown in yellow. The red bars show the frequency of always takers, and the green bars show the frequency of compliers, by service age. Always-takers and compliers are equally frequent among those serving at age 19, but at older ages compliers are more common. Mean service age for the drafted, 20.5, is greater than for the not drafted, 20.3. Implied mean service age for compliers is 20.5 and for always-takers is 20.4.

[Figure 2 about here]

III. Data

The dataset comprises administrative records from the Danish Ministry of Defense. The military register dataset contains information on 155,570 fit-for-service conscripts for birth cohorts 1976-1983. The data to which we have access is for AFQT scores, height, lottery number, the AFD year, and the starting-year for their military service. The Danish AFQT is a cognitive test called the Børg Prien's Prøve (BPP). It was developed for Danish Armed Forces recruitment and has been used since 1957 to test about 1.5 million men (see Teasdale, 2009, for its psychometic properties and a review of its applications). 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 which together measures fluid intelligence rather than acquired knowledge. Mortensen et al. (1989) show the BPP is correlated 0.82 with Wechsler Adult Intelligence Scale. Teasdale et al. (2011) show that test scores are invariant to positive, neutral, or negative attitudes to the military, suggesting that the test's reliability is not undermined by lack of motivation or underperformance amongst those taking the test.

In our estimation sample, we have access to raw AFQT scores for men assessed as fit for service at the end of the AFD. For more recent cohorts, we have access to AFQT scores for all men attending the AFD. In the Appendix we compare AFQT with GPA from tests taken in grade 9, the final year of compulsory schooling. Associations with 9th grade GPAs in mandatory subjects show sensible relations with

ADF attendance, AFQT pass rates and AFQT scores. Higher academic achievement at the end of compulsory schooling has a strong almost linear relationship with higher AFQT scores (see Appendix figure A3). In summary, the AFQT scores appear to be a good measure of fluid intelligence, which is not undermined by strategic behavior on the test.

Thanks to the Danish civil registration number, military records are linked to other administrative registers at Statistics Denmark containing information on demographic characteristics, education, health care usage, employment, earnings, and criminal records. Our main outcome measure is annual labor income from employment and self-employment[17]. The source of the earnings measure is employer reports of annual labor income for employees and the self-employed. Reports are sent to the tax authorities and employees each January for earnings paid in the previous calendar year. We observe the sum of labor earnings during the year from all employments that the individual may have had.[18] Due to the age distribution in our sample, we focus on labor earnings for ages 25 to 35. In total, we observe 152,269 men with non-zero earnings. Another important outcome variable in our analysis is schooling. Data on years of schooling is based on register information on education enrollment and qualifications obtained. This information is reported by educational institutions to the Ministry of Education. Statistics Denmark calculates highest completed education from the qualifications awarded and we use this information to impute number of years of schooling.[19]

[Table 1 about here]

Table 1 presents descriptive statistics for our estimation sample, according to service status alongside a representative 5 percent sample of men. The first two rows are for height and AFQT scores for our sample, with a 5 percent comparison for cohorts 1988-90 (7486 observations), which provide the first available measures of height and AFQT scores for all AFD participants. The 5 percent comparison shown in the remaining rows are male cohorts 1976-1983 as in our estimation sample. Within our estimation sample there are only small differences by service status. Parental schooling is 1 percent lower and household income at age 15 is 1.7 percent lower among those serving than those not serving. In comparison to the general population, our fit-for-service sample differs most according to AFQT, scoring 3 points higher, which is unsurprising because this is an explicit selection criterion. Household income at age 15 is 3 percent higher among those fit for service compared to the general population, but otherwise, differences are small, with fathers' schooling 1 percent lower and birth weight 1 percent greater among those fit for service.

As the tests on the AFD are performed before the lottery, we can use this to assess whether the lottery randomization is balanced. Predicting assignment based on the test results should not be possible if it is truly random. In addition, we can exploit other pre-assignment variables, such as family background, in our checks. Table 2 shows coefficients from four separate OLS regressions explaining draft assignment by the lottery as a function of test results and other background variables. The first column uses our full sample of all men in birth cohorts 1976-83 judged fit for service at the AFD and drawling a lottery number. As expected, AFQT scores, height, parental immigrant status, or being raised in out-of-home care or a single-parent family do not predict assignment. These regressions also control for birth year, birth month, and timing of the AFD. In all cases, the coefficients are small and insignificant, confirming that the lottery works as expected.

In the second column of Table 2, we add an extended set of controls, including birth weight from the medical birth register, equivalized disposable household income when aged 15 from the tax records, and parental years of schooling. As this information is missing for some individuals, the sample size is reduced. Again, we find no evidence that such characteristics affect the probability of being assigned to military service. The third and fourth columns of Table 2 perform the same exercise as in columns one and two, but restrict the sample to men for whom we observe non-zero earnings in at least one year between ages 25 and 35. Pre-assignment characteristics have no explanatory power, regardless of whether we observe earnings. Thus the lottery appears to be a balanced random assignment.

[Table 2 about here]

As well as being balanced for fit-for-service men as a whole, random assignment to the military is also balanced on pre-determined covariates by AFQT score quartile[20]. However, assignment to the different armed forces changes across the AFQT score distribution. Moving from bottom to top AFQT score quartile, assignment to the Air Force increases from 3.2 to 6.7 percent, assignment to the Navy falls from 7.4 to 6.3 percent, and assignment to the Army falls from 83.6 to 81.1 percent. Civil Defense assignment changes only from 5.8 to 5.9 percent. The assignment pattern suggests that the Air Force is the most technically demanding, requiring more than twice as many personnel from the top AFQT score quartile as from the bottom. Although assignment to military service *per se* is random, conditional on this, assignment to the different armed forces is not random, and we cannot make causal inferences about service in any one of the armed forces compared to another. In the remainder of the paper we use the balanced random assignment to the military and are silent about assignment to particular armed forces within the military.

IV. Method

We are interested in the effect of military service on subsequent earnings, which we model as follows:

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

where y_{it} refers to the subsequent earnings of individual i at time t, $MILITARY$ is an indicator of participating in military service, and X is a set of control variables. As we have repeated observations on earnings for ages 25-35 for each individual, we pool the observations and cluster standard errors at the individual level. However, an OLS estimate of π_1 would be biased because the presence of volunteers and resisters makes the decision to join the military endogenous. To deal with this endogeneity problem, we exploit the lottery and instrument $MILITARY$ according to:

$$MILITARY_i = \delta_0 + \delta_1 LOTTERY_i + X_i\delta_2 + \eta_i.$$

Here, $LOTTERY_i$ refers to an indicator variable for drawing a lottery number below the threshold and thus being assigned to military service. As the lottery randomly assigns individuals to military service, in principle, there is no need for including control variables, other than to increase precision. However, as an additional check for whether the randomization works out properly, we compare estimates obtained with and without controls.

Our IV estimator provides a Local Average Treatment Effect (LATE), which reflects the effect of military service among the group of compliers, i.e., in our case men who would serve if randomly assigned to do so but that would otherwise not have volunteered. The LATE is the parameter of interest in our study, because it is relevant for the group of men who would not have self-selected into service and who are forced to serve. The effect on draft compliers is of interest for measuring the cost of forced conscription.[21]

We are primarily interested in the effect of conscription across men with different labor market prospects. Neal and Johnson (1996) show that AFQT scores in the U.S. are a good proxy for labor market prospects because they are a basic skills measure that predicts job performance. We perform separate regressions by pre-service AFQT scores by slicing the sample into different ability groups and performing the IV regressions separately for these different slices. In a series of robustness checks we consider other proxies for labor market prospects, either measured at AFD or earlier, and slice the data accordingly.

V. Results

In this section we first present the effect of being drafted on the probability of military service and show the effect of service on subsequent earnings. Second, we allow service and earnings effects to vary over the AFQT score distribution, and across the distribution of other proxies for labor market prospects. Third, we perform sensitivity analyses for sample inclusion and earnings definitions. Fourth, we investigate explanatory mechanisms for earnings effects, via schooling disruption and other outcomes. Finally, we describe the nature of the Local Average Treatment Effects we identify by way of a compliers analysis.

A. Draft status, military service status and earnings

Table 3 presents regression coefficients for the relationships between draft status, service status and earnings. Panel A shows coefficients on an indicator for service in seperate OLS regressions explaining log earnings for ages 25-35. In the first column we control for only a basic set of covariates: birth year, birth month, and other variables capturing the timing of the AFD. In the second column we also control for AFQT scores and height at the AFD (and the square of AFQT scores and height). The third column adds an extended set of controls for family background: growing up in a single-parent family, placement in out-of-home care, and being the son of an immigrant[22]. Finally, in the fourth column we use the restricted sample, where we also have information on birth weight, parental education and household income at age 15.

The service coefficient in Panel A is remarkably stable across OLS specifications, showing that those serving earn 1.5 to 1.9 percent more than those who did not serve. Panel B shows reduced form coefficients on an indicator for draft status explaining log earnings in regressions with different sets of controls as described for Panel A. The draft coefficient shows that those drafted earn 1.3 to 1.5 percent less than those not drafted. In Panel C we present first stage coefficients on draft status explaining service status. Being drafted increases probability of service by 52 percentage points. The F-statistic on draft status shows that the excluded instrument is very relevant for explaining service status.

Panel D shows second stage IV estimates for the effect of service on log earnings. In all specifications, military service is found to decrease earnings. In columns 1-3, the effect ranges from 2.5 to 2.6 percent. In column 4, the effect is somewhat larger at 2.9 percent, but the sample composition also changes, because of missing information for some of the control variables included in this specification.

The estimates suggest that the impact of peacetime military service on earnings is negative and sizable among Danish men. We can relate this finding to other recent estimates of the effect of peacetime military service in the literature. In a qualitative sense, our estimate is closest to Imbens and van der Klaauw (1995), who found a 5 percent earnings penalty among Dutch men.

[Table 3 about here]

B. Heterogeneous effects by labor market prospects

Next we investigate heterogeneous effects across the distribution of labor market prospects and present results in three ways. First we show estimates across the AFQT score distribution, by quartile in Table 4 and by scrolling over the score distribution in Figure 3. Second we present estimates across quartiles of the distribution of other measures of labor market prospects in Table 5. Third we estimate on sub-samples of potentially vulnerable groups in Table 6. We find remarkably similar gradients in the earnings effect of service across the distributions of all measures of labor market prospects we consider, but we find no effect for potentially vulnerable groups.

[Table 4 about here]

Table 4 presents estimates of the relationship between draft status, service status and earnings, by quartile of the AFQT score distribution. Specification is the same as for column 2 in Table 3, with controls for birth year, month, timing of the ADF, and linear and squared terms for AFQT score and height. It is evident that the positive mean OLS coefficient from Table 3 is driven by those with AFQT scores in the second and third quartiles. Reduced forms in Panel B show draft status only affecting earnings for the top quartile of AFQT scores. First stage regressions in Panel C show that draft status affects service status in a similar way by AFQT scores, having only slightly less effect for AFQT scores in the lower quartile. F-statistics on the excluded instrument show draft status to be a very relevant instrument for service status across the AFQT score distribution.

Instrumental variables regressions in Panel D show a clear gradient in the effect of serving by AFQT score quartile, with positive and insignificant effects at the bottom and negative and significant effects as the top. We thus obtain no evidence that low-ability men, with poor labor market prospects, would have higher earnings by being assigned to military service. However, men at the higher end of the ability distribution face a penalty from being assigned to serve. Moreover, the penalty is large, where men in the top quartile of the distribution face a 7 percent earnings penalty from serving, and those in the third quartile face a penalty of about 3 percent.

[Figure 3 about here]

Figure 3 presents service status coefficients from OLS and IV earnings regressions across the distribution of AFQT scores. Regressions are run separately for a moving 20 percent window of AFQT scores, using the same specification as for Table 4. OLS coefficients are positive, but only significant towards the middle of the AFQT score distribution. IV coefficients show a gradual decline moving up the AFQT score distribution: Initially positive but insignificant, becoming negative and significant for the upper third of the distribution. It is also the upper third of the AFQT score distribution where IV coefficients fall significantly below OLS, illustrating that this is where selection bias into service is greatest.

AFQT measures basic skills that predict job performance, and scores are thought to be a good proxy for labor market prospects. In Section 2 we have shown that the AFQT does not appear to be undermined by strategic test-taking behavior for avoiding the draft, but nevertheless we cannot rule out the possibility. We now consider other pre-assignment background characteristics likely correlated with labor market prospects and reflecting differences in the opportunity costs of serving across individuals.

In Table 5 we examine the effect of serving across quartiles of household income at 15, parental education, birth weight, and height. As shown in panel A, we obtain similar results when looking across quartiles of household income. Again, we find the greatest penalty, 7 percent, for men from households in the top quartile of the income distribution. We obtain similar results for mother's and father's education, as shown in panels B and C. In Panels D and E, we focus on health measures; birth weight, and height. Greater values signal better health, and we again find that the opportunity costs are highest for the most healthy individuals.

Our similar findings across the distribution of other background characteristics that are correlated with labor market prospects is supportive of our main result across the AFQT score distribution. Results are robust to different ways of distinguishing between individuals with high and low opportunity costs.

[Table 5 about here]

Next, we look for heterogeneity between distinct groups rather than across continuous proxies for labor market prospects. Angrist (1990) compares Whites and non-Whites in the U.S., finding that Whites face earnings penalties and Card and Cardoso (2013) consider men with only primary schooling in Portugal and find that those who serve have higher earnings. Our results for three distinct groups appear in Table 6. An interesting question is whether the sons of immigrants gain from military service, e.g., by forming networks with the sons of non-immigrants. Indeed, we find an earnings premium for the sons of immigrants of almost 3 percent, but the estimate is not significant. For those raised in out-of-home care, and thus coming from a disadvantaged background, we find a positive and large 7.6 percent earnings premium, but the estimate is again insignificant.[23]

[Table 6 about here]

When we consider those raised in single-parent families, we find negative and insignificant effects.[24] However, the magnitude of the coefficent is similar to that in our main specification. We have also checked whether the effect is different across the ability distribution but found no such evidence[25]. Sample sizes for all of these potentially disadvantaged groups are much smaller than for the main analysis, resulting in less precision.

We have found that the effects of conscription are heterogeneous. This heterogeneity is also as expected, where men with favorable civilian labor market prospects have a high opportunity cost of serving in terms of earnings forgone, whereas those with less favorable prospects are less hurt. The cost of forcing high-ability men to serve in the military is high. The magnitude of the effect is similar to the return to one additional year of schooling in Denmark, (see, e.g., Pedersen et al., 1990 and Asplund et al., 1996).

C. Sensitivity analysis

We now examine the sensitivity of our results with respect to certain choices we made about measurement and sample inclusion. First, as our main earnings measure includes sickness and leave benefits, we test an alternative measure excluding these benefits. As Table A.1 in the Appendix shows, excluding these benefits has little consequence for our estimation results. Given that sickness absence is rather uncommon among the age groups we consider, this result is not surprising.

In our main specification, we impose no restriction on the age at which the person actually served. This assumption is of little consequence, as only 663 persons, or 0.4 percent of the sample, had not yet served when we started to measure earnings at age 25. We kept these observations in the analysis so as not to undo the lottery randomization by selecting on subsequent behavior. However, when we restrict the sample to only those having served by age 25, our results remain unaffected (not shown).

As previously mentioned in the description of the Danish conscription process, a change in the length of the service period occured after 2006. We expect the effect of service to be less after 2006, as the period was halved for most men. Only 127 men in our sample (0.08 percent) served after 2006. When interacting the service indicator with an indicator of serving post 2006, we find a small and negative insignificant effect.

Our results thus far concern earnings at ages 25-35. We can also look at younger ages and study the short-term costs of serving. At these ages, we expect high-ability men to be in school if not serving, whereas low-ability men are more likely to be working in the civilian labor market instead. Thus during these ages one might expect that low-ability men pay the highest short term cost. As Table A.2 in the Appendix shows, this is indeed the case, with a 6 percent penalty in the two lowest AFQT score quartiles but no penalty in the two highest quartiles. The pattern of opportunity costs is thus reversed at these young ages.

In our main analyses we pool earnings for ages 25-35 and estimate mean effects across these ages. When we run the earnings regressions separately for each age we can see that the mean earnings effect of service becomes insignificant after age 30 and the earnings effect for top quartile AFQT scores becomes insignificant after age 32.[26] This is consistent with delayed educational enrolment and educational attainment, as shown in Table 7, followed by eventual educational catch-up.

Estimated earnings losses are concentrated between ages 25-32 and are due to lower wages during ages of lower educational attainment and fewer hours worked in the labour market while still enrolled in education. It is not clear whether the civilian labour market values military versus civilian work experience differently, but in any case, they are equally rewarded after age 32.

After age 32 there is no causal effect of military service on earnings across the AFQT score distribution. Nevertheless, the lifetime earnings of those serving has been reduced. For men in the top quartile of AFQT scores, earnings lost aged 25-35 due to military service are estimated to total USD 50,000 (the annual mean earnings is USD 65,000). Annualised earnings while conscripted are USD 30,000, and for top quartile AFQT score men, conscripted earnings would need to increase by 150 percent to compensate for lifetime earnings losses. For a cohort of men who serve, aggregate earnings losses sum to USD 330 million.

Angrist (1990) finds significant earnings losses 15 years after U.S. military service in Vietnam, whereas Angrist and Chen (2011) find no earnings effects after 25 years, attributing the catch-up to educational opportunities available to veterans through the G.I. Bill.[27] While the contexts are very different, the importance of educational catch-up following military service which eventually equates earnings differences for veterans and non-veterans is striking.

D. Explanatory Mechanisms

Several mechanisms may explain the estimated effects across the ability distribution.[28] We first consider the effect of military service on years of schooling attained at different ages. Serving in the army could mean that studies are not started, are interrupted, or are given up. 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 education to avoid the draft.

[Table 7 about here]

Panel A of Table 7 shows the effect of serving on attained years of schooling at age 25. In Panel B, we repeat this exercise at age 30. Serving has a negative and significant impact on years of schooling at all quartiles but the effect is strongest for those in the top quartile of the AFQT score distribution. The smallest effect at age 25 is a reduction of schooling by 0.13 years for low-ability men, and is insignificant at age 30. Only for high-ability men does a significant and negative effect remain at age 30, but it is now much smaller, amounting to a 0.08 years decrease.

These results suggest that among high-ability men, the large earnings penalty for serving cannot be fully explained by reduced educational attainment, because by age 30 most have largely caught up on their interupted studies. One tenth of a year of lost schooling explains very little of the earnings penalty among high-ability men. Another potential explanation for the earnings penalty is that some men are still studying when we measure their earnings, i.e., they would have zero or low earnings, possibly explaining part of the earnings penalty.[29] However, as we use log earnings in our main specification, those with zero earnings drop out of the analysis.[30]

As many Danish students have part-time jobs, another explanation for the earnings penalty may be that serving increases the likelihood of their studying, rather than embarking on a career job, during their late 20s, and thus having low, but non-zero, recorded earnings. In our sample, we observe 13 percent studying at age 25, 8 percent at 28, 4 percent at 30, and 1 percent at 35.

Panels C-E in Table 7 show the effect of military service on the probability of being enrolled in education at different ages. The average effect is small but significant, with a 2.5 percent greater likelihood of studying at age 25, falling to a 1.1 percent greater likelihood of studying at age 30. As Table 7 shows, these average estimates hide some heterogeneity across the ability distribution. With age, educational enrollment effects become concentrated among those with high ability. For the top quartile-AFQT score men, service makes them 3.9 percent more likely to be enrolled in education at age 25, and they are still 2.3 percent more likely to be enrolled at age 30. These results suggest that part of the earnings penalty among the high-ability group is that schooling is delayed, resulting in lost labor market experience in a career civilian job and lower earnings.

E. 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 8 summarizes the findings of the effect of conscription on these alternative outcomes. In panel A, the outcome measures the proportion of the year during which unemployment insurance benefits were received (at age 26). The estimates suggest that conscription indeed reduces unemployment risk and that the effects are most pronounced at the lower and upper AFQT score quartiles. The effect, however, is rather modest. Moreover, these estimates show that the large negative effect of conscription on earnings for the top AFQT score quartile cannot be explained by unemployment.

Panel B shows results for bank balances at age 26, calculated as bank assets minus bank debt.[31] This information is obtained from financial institution reports to the tax authorities. We find a positive average effect that is significant at the 10 percent level. The clearest effect is seen for the top quartile of AFQT scores, where serving leads to an increase in bank balances by 8,880 DK (about 1,640 USD). One explanation may be that some men manage to save money during their conscription because of an after tax wage including allowances above the minimum wage with food and housing costs covered.[32]

In Panel C, we show results for crime. The outcome variable is an indicator for conviction for any type of crime, excluding traffic offenses, from age 26. The data on criminal convictions comes from the Central Crime Register, which collects criminal court proceedings and reports them to the Ministry of Justice. The effects are small and insignificant across the distribution. Our findings thus differ from those of Galiani et al. (2011), who find that conscription increases crime in Argentina.[33]

[Table 8 about here]

In panels D to G, we focus on a number of health outcomes. First, we consider the probability of experiencing hospitalization from age 26. We obtain the data on hospitalizations from the national hospital discharge register, which collects reports that local health authorities send to the national board of health. We consider all admissions to hospitals, including outpatients. A significant effect is obtained at the second quartile of the AFQT score distribution, where conscription increases the risk of hospitalization (panel D). We find no effects on the probability of purchasing prescription medicine for psychiatric problems or for being diagnosed with a psychiatric disorder from the age of 26 (panels E and F).[34] However, we find a significant effect (10 percent level) of military service reducing (treatment for) addiction problems, although this effect is restricted to the second quartile of the AFQT score distribution. Nevertheless, as these health effects are insignificant for the top quartile, they cannot explain the large earnings penalties at the higher end of the ability distribution.

F. Compliers analysis

In the LATE framework, understanding the nature of compliers is a useful approach to establishing external validity. A compliers analysis provides the context for interpreting behavioral responses contributing to the treatment effect that is identified by the instrument. While it is not always possible to observe individual response type, we can identify treatment groups as mixtures of compliers and non-compliers. Different groups may comply with treatment assignment to a varying degree. We can estimate the outcome distributions for compliers from outcome mixtures of different observed response types weighted by response probabilities. In this section we describe response types for different groups and summarize different outcome distributions by response type.

[Figure 4 about here]

Figure 4 plots sample proportions of the different response types over the AFQT score distribution. The proportions of compliers who serve and compliers who do not serve are quite stable, respectively declining by 1 percent and increasing by 1 percent from lowest quintile AFQT score to highest quintile. Sample proportion of never-takers (always-takers) is more variable by AFQT score, exhibiting a U-shape (inverted U-shape), falling (increasing) by 1 percent from the lowest quintile to the middle quintile and then increasing (decreasing) by 4 percent to the highest quintile. These relatively small differences in complier types across the AFQT score distribution are supportive of the stable unit treatment value assumption with respect to non-interference, 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. Since the civilian labour supply of high-ability men is unlikely to change very much when changing recruitment system, neither will the level of civilian earnings for high ability men.

In Table 9, columns 2-5 we break down the information shown in Figure 4 by labor market prospects and for vulnerable groups. We characterize response types according to pre-assignment characteristics. Columns show percentages of each response type for groups defined in the row header. The top row shows response type percentages for the whole sample, for reference. Panel A refers to the top quartile of different proxies for labor market prospects. Compliance rates for top quartile AFQT scores are similar to the population as a whole, but they are 2 percent less likely to be always-takers and 2 percent more likely to be never-takers. The response type breakdown is very similar for those in the top quartile of parental schooling and household income at age 15. However, response types for the top quartile of the height distribution differ from the general population in that they are 2.5 percent less likely to comply, especially less likely to comply with being assigned not to serve. Individuals in the birth weight top quartile are almost 2 percent more likely to be never-takers than the general population.

Panel B of Table 9 shows response type probabilities for three potentially vulnerable groups. Men raised in a single parent-family are 1.5 percent less likely to be never-takers. Sons of immigrants are rather different from the general population in response type, being 5 percent less likely to be always-takers and 5 percent more likely to comply—4 percent more likely to comply with the assignment not to serve. Men who grew up in out-of-home care have response types which are most distinctive to the general population. They are 7 percent more likely to be always-takers, 3 percent less likely to comply with being assigned not to serve, and 4 percent less likely to be never-takers than the general population.

Always-takers are the response type with greatest variation between groups, ranging from 15 percent for sons of immigrants to 28 percent for men who grew up in out of home care. Total compliance rates vary from 50 percent for the tallest quartile to 58 percent for sons of immigrants. Never-takers have the least variation, ranging from 22 percent for men growing up in out of home care to 29 percent for those with fathers in the top quartile of schooling. Men growing up in out of home care are 7 percent more likely than the general population to serve, but these are all always-takers. Sons of immigrants have the lowest rate of service at 42 percent, which is due to the lowest proportion of always-takers, and the highest compliance rate, but this is mostly compliance with assignment not to serve.

Angrist (1990) showed U.S. Vietnam-era service rates of 11-26 percent for Whites and 4-17 percent for non-Whites. In our Danish data we have a service rate which is just above at 27 percent.[35] Although we find some variation in response across the different sub-populations we have considered, the variation in Denmark is much less than between Whites and non-Whites in the U.S.

[Table 10 about here]

Table 10 presents summary statistics for the implied distribution of the different outcomes we consider by response types. Following Imbens and Rubin (1997) we compute these statistics without conditioning on covariates, hence implied OLS and IV estimates differ from those presented elsewhere in the paper. Across all outcomes, the largest difference between OLS and IV estimates are for earnings, especially for the top quartile of AFQT scores, with changes in sign from significantly positive for OLS to significantly negative for IV. Distinguishing between compliers who serve and compliers who do not serve shows that the IV-OLS difference is largely due to compliers who serve compared to always-takers, rather than compliers who do not serve compared to never-takers. Indeed, the increasing OLS-IV earnings differential when moving from the full sample to top quartile AFQT scores, is mostly due to compliers who serve.

Unemployment and bank balances are the other outcomes shown in Table 10 with significant estimates of IV coefficients, and the differences from OLS are driven in a similar way by contrasts between compliers who serve and always-takers, rather than between compliers who do not serve and never-takers.

[Figure 5 about here]

In Figure 5 we decompose the difference in service status coefficients estimated by OLS and IV earnings regressions, as in the first row of Table 10, but do so in a 20 percent window moving across the AFQT score distribution. The difference that opens up at the upper end of the AFQT score distribution is due to the earnings penalty for compliers who serve compared to always-takers. The difference between compliers who do not serve compared to never-takers is borderline significant for AFQT scores above the median.

In summary, selection into service effects, as apparent from OLS-IV differentials, appear to be much stronger for earnings than for other outcomes. Decomposing complier types contributing to IV differences, it is clear that selection is mostly among compliers who serve versus always-takers, rather than compliers who do not serve versus never-takers. Our compliers analysis shows some differences in response types for vulnerable groups, but only very modest differences across the distribution AFQT scores and other proxies for labour market prospects. Compliers are a large proportion of those fit for service and have on average quite similar observed characteristics to those of other response types.

VI. Conclusions

In the U.S. during the Vietnam era, economists made important contributions to the debate that led to the shift from military conscription to an all-volunteer force. For recruiting a force of equal size through a draft lottery, the average opportunity costs for those serving would be higher than with all volunteers. While some high-opportunity cost men would not volunteer at the all-volunteer wage rate, they would nonetheless be forced to serve under the draft. Angrist (1990) estimated the mean effect for Whites on civilian earnings of veteran status inducted by the draft lottery to be a loss, 10 years after service, of about 15 percent. Most countries still have some form of military conscription, and recent papers use quasi-experimental variation to estimate the effect of peacetime military service on earnings and other outcomes. Our paper revisits the original question about the distribution of opportunity costs under a draft by using the original causal research design to estimate heterogeneous effects from the Danish draft lottery.

For men who are drafted and who serve in the military but who otherwise would not have volunteered, we find a mean earnings loss of 2.5 percent for ages 25-35 compared to if they had not served. We identify the LATE for precisely the population that would not have served in the absence of the draft but are forced to do so. 27 percent of a male cohort serves in the military, and these are split into 16 percent compliers and 11 percent always-takers. Our LATE is relevant for most of those who serve.

The mean impact hides important heterogeneity. For low-ability men who are drafted and served, there is no earnings effect, but for high-ability men, the costs are high. Men in the top AFQT score quartile suffer a 7 percent earnings penalty if they are drafted and forced to serve. This penalty for high ability men lasts until age 32, amounts to USD 50,000 in foregone lifetime earnings, and would require that earnings while conscripted were increased by 150 percent to offset subsequent earnings losses. Moreover, the gradient of earnings effects is remarkably similar across other pre-assignment measures of labor market prospects, from birth weight and height to parental schooling and income.

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. There are no effects on criminal convictions or measures of health care usage, either at the mean or across ability.

Our findings relate to only 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 that are driven by large losses for high-ability men with good labor market prospects and who would not have volunteered. If technical change in the military requires fewer but higher-ability personnel, and if such people were encouraged to volunteer, then high-ability men and women facing the draft everywhere would benefit significantly.

VII. Tables and Figures

TABLE 1.—SUMMARY STATISTICS PRE-ASSIGNMENT FOR ESTIMATION SAMPLE, BY SERVICE STATUS AND FOR THE GENERAL POPULATION

	Estimation sample			5 percent population
	All	served=1	served=0	
	(1)	(2)	(3)	(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)
Birth year	1979.28	1979.27	1979.29	1979.43
	(2.26)	(2.33)	(2.19)	(2.27)
Birth month	6.40	6.40	6.40	6.30
	(3.34)	(3.29)	(3.38)	(3.12)
Raised in single-parent family	0.18	0.18	0.17	0.19
	(0.38)	(0.39)	(0.38)	(0.39)
Placed in out-of-home care	0.04	0.04	0.03	0.05
	(0.19)	(0.21)	(0.18)	(0.22)
Son of immigrant	0.04	0.04	0.04	0.04
	(0.20)	(0.19)	(0.20)	(0.20)
Birth weight (gr)	3371	3359	3381	3342
	(653)	(647)	(657)	(599)
Household income at age 15 (DKK)	134047	132738	135071	130579
	(57582)	(56377)	(58487)	(56554)
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)
Observations	152269	66813	85456	14390

Notes: Means, standard deviations in parentheses. In column (4) headed 5 percent population, Height & AFQT score refer to the 1988-90 birth cohorts attending the AFD (7486 observations), while the other variables refer to the 1976-1983 male birth cohorts (14390 observations). AFQT is taken on the AFD. Height is measured on AFD. 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 one if has lived in out-of-home care (institutions or foster home) before age 18. Household income at 15 is equivalized according to the formula (sum of income in the household plus transfers minus taxes)/(1*first_adult+0.7*second_adult+0.5*number_of_children) and reflated to 2012 prices by the CPI. Mother's and father's schooling are measured when son is age 15.

TABLE 2.—DRAFT STATUS RANDOMIZATION BALANCE CHECK. OLS COEFFICIENTS ON PRE-ASSIGNMENT CHARACTERISTICS

	Total sample incl. zero earnings		Estimation sample excl. zero earnings	
	(1)	(2)	(3)	(4)
Height (cm)	-0.00002	-0.00005	-0.00000	-0.00001
	(0.00018)	(0.00019)	(0.00018)	(0.00019)
AFQT	0.00022	0.00019	0.00023	0.00021
	(0.00014)	(0.00015)	(0.00014)	(0.00015)
Son of immigrant	-0.00527	0.00110	-0.00545	-0.00016
	(0.00586)	(0.00870)	(0.00603)	(0.00886)
Raised in single-parent family	-0.00402	-0.00355	-0.00414	-0.00352
	(0.00299)	(0.00307)	(0.00304)	(0.00312)
Placed in out-of-home care	-0.00263	-0.00195	-0.00116	-0.00094
	(0.00586)	(0.00608)	(0.00605)	(0.00627)
Birth weight (gr)		0.00000		0.00000
		(0.00000)		(0.00000)
Household income at age 15 (DKK)		0.00000		0.00000
		(0.00000)		(0.00000)
Mother's years of schooling		-0.00031		-0.00037
		(0.00047)		(0.00047)
Father's years of schooling		0.00022		0.00028
		(0.00042)		(0.00042)
Observations	155750	149124	152269	146033
F-test of covariates	0.98	0.92	1.13	1.00

Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes:The columns contain coefficients from different OLS regressions. Columns 1-2 for the sample including individuals with zero earnings and columns 3-4 for the estimation sample excluding individuals with zero earnings. The dependent variable is an indicator taking the value one if the lottery draw was below the threshold and the individual was drafted.

TABLE 3.—DRAFT STATUS, MILITARY SERVICE STATUS AND EARNINGS

	(1)	(2)	(3)	(4)
Panel A. OLS regressions: outcome log earnings				
Service status=1	0.0188***	0.0149***	0.0169***	0.0147***
	(0.00393)	(0.00394)	(0.00392)	(0.00397)
Adjusted R^2	0.046	0.047	0.051	0.053
Panel B. Reduced form regressions: outcome log earnings				
Draft status=1	-0.0129***	-0.0128***	-0.0135***	-0.0150***
	(0.00418)	(0.00418)	(0.00416)	(0.00421)
Adjusted R^2	0.046	0.047	0.051	0.053
Panel C. First-stage regressions: outcome service status=1				
Draft status=1	0.518***	0.518***	0.518***	0.517***
	(0.00254)	(0.00253)	(0.00253)	(0.00258)
Angrist Pischke F-stat	41693	41895	41927	40105
Panel D. Second stage IV regressions: outcome log earnings				
Service status=1	-0.0249***	-0.0247***	-0.0261***	-0.0289***
	(0.00808)	(0.00806)	(0.00803)	(0.00813)
Basic controls	Yes	Yes	Yes	Yes
Extended controls I	No	Yes	Yes	Yes
Extended controls II	No	No	Yes	Yes
Extended control III	No	No	No	Yes
Observations	1068599	1068599	1068599	1026441
Individuals	152269	152269	152269	145663

Standard errors clustered at the individual level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: Each column contains the coefficient of interest from different regressions. The dependent variable is log annual labor earnings during the period 2001-2011 in age range 25-35, reflated to 2012 Danish Kroner and including sickness and leave benefits. Columns differ according to the set of other explanatory variables included. Column 1 includes in the regression controls for birth year, birth month, and the timing of the draft lottery. Column 2 also includes AFQT and height and their square. Column 3 also includes controls for raised in single-parent family, placed out-of-home care and having non-immigrants parents. The fullest specification, column 4, also includes birth weight, household income at age 15, mother's years of schooling and father's years of schooling.

TABLE 4.—DRAFT STATUS, MILITARY SERVICE STATUS AND EARNINGS BY AFQT SCORE QUARTILE

	1st quartile (1)	2nd quartile (2)	3rd quartile (3)	Top quartile (4)
Panel A. OLS regressions: outcome log earnings				
Service status=1	0.00372	0.0257***	0.0245***	0.00491
	(0.00774)	(0.00773)	(0.00810)	(0.00791)
Adjusted R^2	0.016	0.036	0.065	0.106
Panel B. Reduced form regressions: outcome log earnings				
Draft status=1	0.00654	-0.00716	-0.0153*	-0.0370***
	(0.00822)	(0.00820)	(0.00862)	(0.00835)
Adjusted R^2	0.016	0.036	0.065	0.107
Panel C. First-stage regressions: outcome service status=1				
Draft status=1	0.497***	0.524***	0.531***	0.524***
	(0.00493)	(0.00496)	(0.00531)	(0.00502)
Angrist Pischke F-stat	10156	11139	10018	10870
Panel D. Second-stage IV regressions: outcome log earnings				
Service status=1	0.0132	-0.0137	-0.0288*	-0.0706***
	(0.0165)	(0.0157)	(0.0162)	(0.0160)
Observations	291453	277032	234842	265272
Individuals	41983	39371	33156	37759

Standard errors clustered at the individual level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.
Notes: Each of the four columns contains coefficient of interest from different regressions, according to quartiles of the AFQT distribution for those who drew a lottery number on AFD. The dependent variable is log annual labor earnings during the period 2001-2011 in age range 25-35, reflated to 2012 Danish Kroner and including sickness and leave benefits. The specification is as in column 2 of table 3.

TABLE 5.—EFFECTS OF MILITARY SERVICE ON EARNINGS ACROSS QUARTILES OF THE DISTRIBUTIONS OF BACKGROUND CHARACTERISTICS

Outcome log earnings	1st quartile (1)	2nd quartile (2)	3rd quartile (3)	Top quartile (4)
Panel A. Across household income quartiles				
Service status=1	0.0172	-0.0379**	-0.0105	-0.0696***
	(0.0167)	(0.0159)	(0.0154)	(0.0164)
Observations	272079	278709	266739	249045
Individuals	37579	38088	38188	38101
Angrist Pischke F-stat	10147	10373	10628	10829
Panel B. Across father's education quartiles				
Service status=1	0.00341	-0.0222	-0.0160	-0.0617***
	(0.0164)	(0.0148)	(0.0165)	(0.0166)
Observations	279451	301072	226083	259966
Individuals	39558	43223	31555	37620
Angrist Pischke F-stat	10337	12150	8545	10981
Panel C. Across mother's education quartiles				
Service status=1	-0.000529	-0.00544	-0.0180	-0.0692***
	(0.0164)	(0.0159)	(0.0151)	(0.0170)
Observations	273937	262599	275408	254628
Individuals	37847	38456	38444	37209
Angrist Pischke F-stat	9807	10112	11201	10840
Panel D. Across birth weight quartiles				
Service status=1	-0.0141	-0.00447	-0.0505***	-0.0346**
	(0.0133)	(0.0219)	(0.0159)	(0.0176)
Observations	378960	143105	276368	230500
Individuals	47644	25483	36923	35983
Angrist Pischke F-stat	14537	6090	10703	9044
Panel E. Across height quartiles				
Service status=1	-0.0213	-0.00919	-0.0212	-0.0528***
	(0.0151)	(0.0163)	(0.0154)	(0.0180)
Observations	295206	253177	292512	227704
Individuals	42096	35966	41699	32508
Angrist Pischke F-stat	12570	10289	11294	7880

Standard errors clustered at individual level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Notes: Each cell contains the coefficient of interest from separate second stage regressions. The explanatory variable of interest is always military service. Specification is always as in column 2 of Table 3.

TABLE 6.—EFFECT OF MILITARY SERVICE ON EARNINGS FOR VARIOUS SUBGROUPS

	Son of immigrant (1)	Out-of-home care (2)	Single-parent family (3)
Panel A. OLS regressions: outcome log earnings			
Service status=1	0.0205	-0.00409	0.0242**
	(0.0240)	(0.0263)	(0.0103)
Adjusted R^2	0.042	0.020	0.037
Panel B. Reduced form regressions: outcome log earnings			
Draft status=1	0.0163	0.0355	-0.0118
	(0.0253)	(0.0273)	(0.0110)
Adjusted R^2	0.042	0.020	0.037
Panel C. First-stage IV regressions: service status=1			
Draft status=1	0.575***	0.466***	0.518***
	(0.0126)	(0.0136)	(0.00607)
Angrist Pischke F-stat	2092	1174	7284
Panel D. Second-stage IV regressions: outcome log earnings			
Service status=1	0.0284	0.0763	-0.0228
	(0.0439)	(0.0587)	(0.0212)
Observations	37577	38427	184685
Individuals	6047	5880	27077

Standard errors clustered at the individual level in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: Each column contains the coefficient of interest from different regressions on three potentially vulnerable samples. The specification is as in column 2 of Table 3.

TABLE 7.—EFFECT OF MILITARY SERVICE ON SCHOOLING ATTAINMENT AND ENROLLMENT, AT VARIOUS AGES BY AFQT SCORE QUARTILE

	Full sample (1)	1st quartile (2)	2nd quartile (3)	3rd quartile (4)	Top quartile (5)
Panel A. Years of schooling at age 25					
Service status=1	-0.168***	-0.127***	-0.163***	-0.198***	-0.201***
	(0.0165)	(0.0352)	(0.0316)	(0.0326)	(0.0310)
Panel B. Years of schooling at age 30					
Service status=1	-0.0332	-0.0127	-0.0406	-0.0231	-0.0782*
	(0.0219)	(0.0407)	(0.0419)	(0.0468)	(0.0452)
Panel C. Studying at age 25					
Service status=1	0.0247***	0.0120*	0.0281***	0.0154	0.0394***
	(0.00455)	(0.00636)	(0.00857)	(0.0105)	(0.0106)
Panel D. Studying at age 28					
Service status=1	0.0207***	0.00578	0.0164**	0.0292***	0.0315***
	(0.00396)	(0.00557)	(0.00729)	(0.00904)	(0.00951)
Panel E. Studying at age 30					
Service status=1	0.0108***	0.00633	0.00699	0.00592	0.0232***
	(0.00324)	(0.00474)	(0.00599)	(0.00719)	(0.00781)
Observations	152269	41983	39371	33156	37759
Angrist Pischke F-stat	46019	11453	12054	10758	11925

Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Notes: Each coefficient comes from a different instrumental variables regression.The specification is as in column 2 of table 3. The dependent variable is years of educational attainment by a certain age—25 in Panel A and 30 in Panel B. For Panels C-E, the outcome is an indicator variable taking the value one for enrolment in education at the age shown in the column header, and zero otherwise. The samples are split into four columns according to quartile of the AFQT distribution. The explanatory of interest is an indicator taking the value one if the individual is served in the military.

TABLE 8.—EFFECT OF MILITARY SERVICE ON VARIOUS OUTCOMES BY AFQT SCORE QUARTILE

	Full sample (1)	1st quartile (2)	2nd quartile (3)	3rd quartile (4)	Top quartile (5)
Panel A. Unemployment=1 at age 26					
Service status=1	-0.00422***	-0.00688**	-0.00247	-0.00285	-0.00430*
	(0.00136)	(0.00316)	(0.00265)	(0.00267)	(0.00227)
Panel B. Bank balance in DKK at age 26					
Service status=1	4213.1*	7077.6*	-1577.2	1827.0	8870.2**
	(2169.6)	(4293.4)	(4362.5)	(4526.3)	(4168.0)
Panel C. Criminal convictions=1 for age 26-35					
Service status=1	0.000373	0.000422	-0.00414	0.00416	0.00320
	(0.00214)	(0.00538)	(0.00426)	(0.00388)	(0.00302)
Panel D. Hospitalization=1 for age 26-35					
Service status=1	0.00451	0.00203	0.0194**	-0.00762	0.00243
	(0.00465)	(0.00852)	(0.00893)	(0.00993)	(0.00984)
Panel D. Prescription medicine=1 for age 26-35					
Service status=1	-0.00129	0.00723	-0.00853	-0.000965	-0.00225
	(0.00363)	(0.00818)	(0.00714)	(0.00713)	(0.00629)
Panel F. Psychiatric diagnosis=1 for age 26-35					
Service status=1	-0.00238	-0.00128	-0.00348	-0.00341	-0.000551
	(0.00211)	(0.00502)	(0.00406)	(0.00396)	(0.00353)
Panel G. Addiction treatment=1 for age 26-35					
Service status=1	-0.00192*	-0.00173	-0.00363*	-0.000856	-0.000840
	(0.00104)	(0.00285)	(0.00203)	(0.00177)	(0.00118)
Observations	152269	41983	39371	33156	37759

Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Notes: Each cell contains coefficient of interest from separate instrumental variables regressions. The explanatory variable of interest is always military service. Specification is always as in Table 4, namely including controls for birth year, birth month, timing of the lottery, AFQT, height, and their square. The first column is for the whole sample, columns 2 through 5 are for quartiles of the ability distribution. Each panel corresponds to a different dependent variable.

TABLE 9.—CHARACTERIZING RESPONSE TYPES BY GROUPS, COUNTS AND PROPORTIONS

	Individuals	Always Taker	Complier		Never Taker
			Served=1	Served=0	
	(1)	(2)	(3)	(4)	(5)
Full sample	152269	0.2095	0.2557	0.2703	0.2644
Panel A. Labor market prospect groups top quartile					
AFQT	37759	0.1867	0.2515	0.2757	0.2861
Father's education	37620	0.1839	0.2523	0.2729	0.2910
Mother's education	37209	0.1854	0.2535	0.2747	0.2863
Household income at age 15	38101	0.1866	0.2574	0.2729	0.2832
Height at age 18	32508	0.2201	0.2461	0.2535	0.2804
Birth weight	35593	0.2039	0.2475	0.2666	0.2820
Panel B. Vulnerable groups					
Single parent family	27077	0.2156	0.2620	0.2746	0.2479
Son of immigrant	6047	0.1503	0.2707	0.3093	0.2696
Out-of-home care	5880	0.2779	0.2616	0.2409	0.2196

Notes: The first column shows number of observations for the group defined in the row header. Columns 2-5 are sample proportions of response types. Panel A shows response types for the top quartile of different measures of labor market prospects. Panel B shows response types by vulnerable groups.

TABLE 10.—OUTCOME SUMMARY BY RESPONSE TYPE, MEANS AND STANDARD ERRORS

	OLS	IV	Always Taker	Complier		Never Taker
				Served=1	Served=0	
	(1)	(2)	(3)	(4)	(5)	(6)
Log earnings - all	0.0178***	-0.0316***	12.3495	12.2808	12.3124	12.2783
	(0.0036)	(0.0075)	(0.0049)	(0.0059)	(0.0054)	(0.0065)
Log earnings - AFQT Q4	0.0169**	-0.0528***	12.3216	12.2093	12.2621	12.2256
	(0.0080)	(0.0153)	(0.0111)	(0.0105)	(0.0111)	(0.0122)
Unemployment	-0.0010	-0.0035***	0.0732	0.0692	0.0727	0.0713
	(0.0006)	(0.0012)	(0.0009)	(0.0008)	(0.0009)	(0.0009)
Bank balance	5271***	3899**	-89303	-94204	-98103	-96094
	(874)	(1831)	(1173)	(1406)	(1387)	(1505)
Criminal conviction	0.0028***	0.0001	0.0852	0.0840	0.0840	0.0793
	(0.0011)	(0.0021)	(0.0014)	(0.0016)	(0.0014)	(0.0012)
Hospitalization	0.0257***	0.0037	0.9442	0.9005	0.8968	0.8944
	(0.0022)	(0.0040)	(0.0029)	(0.0028)	(0.0030)	(0.0031)
Prescription medicine	-0.0027	-0.0014	0.2340	0.2252	0.2266	0.2381
	(0.0017)	(0.0028)	(0.0026)	(0.0022)	(0.0022)	(0.0027)
Psychiatric diagnosis	0.0017*	-0.0023	0.1117	0.1002	0.1025	0.1055
	(0.0009)	(0.0019)	(0.0016)	(0.0013)	(0.0014)	(0.0015)
Addiction treatment	0.0005	-0.0016*	0.0635	0.0607	0.0622	0.0607
	(0.0004)	(0.0009)	(0.0007)	(0.0006)	(0.0007)	(0.0007)

Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Notes: OLS is the mean difference in outcomes by service status. IV is the ratio of differences in mean outcome by draft status to the difference in mean service probability by draft status. OLS and IV estimates in this differ from those presented elsewhere in the paper because this is estimated without covariates. Mean outcomes by response type are computed following Imbens and Rubin (1997). Standard errors are computed from 1000 bootstrap replications, clustering at the individual level for earnings, and are shown in parentheses.

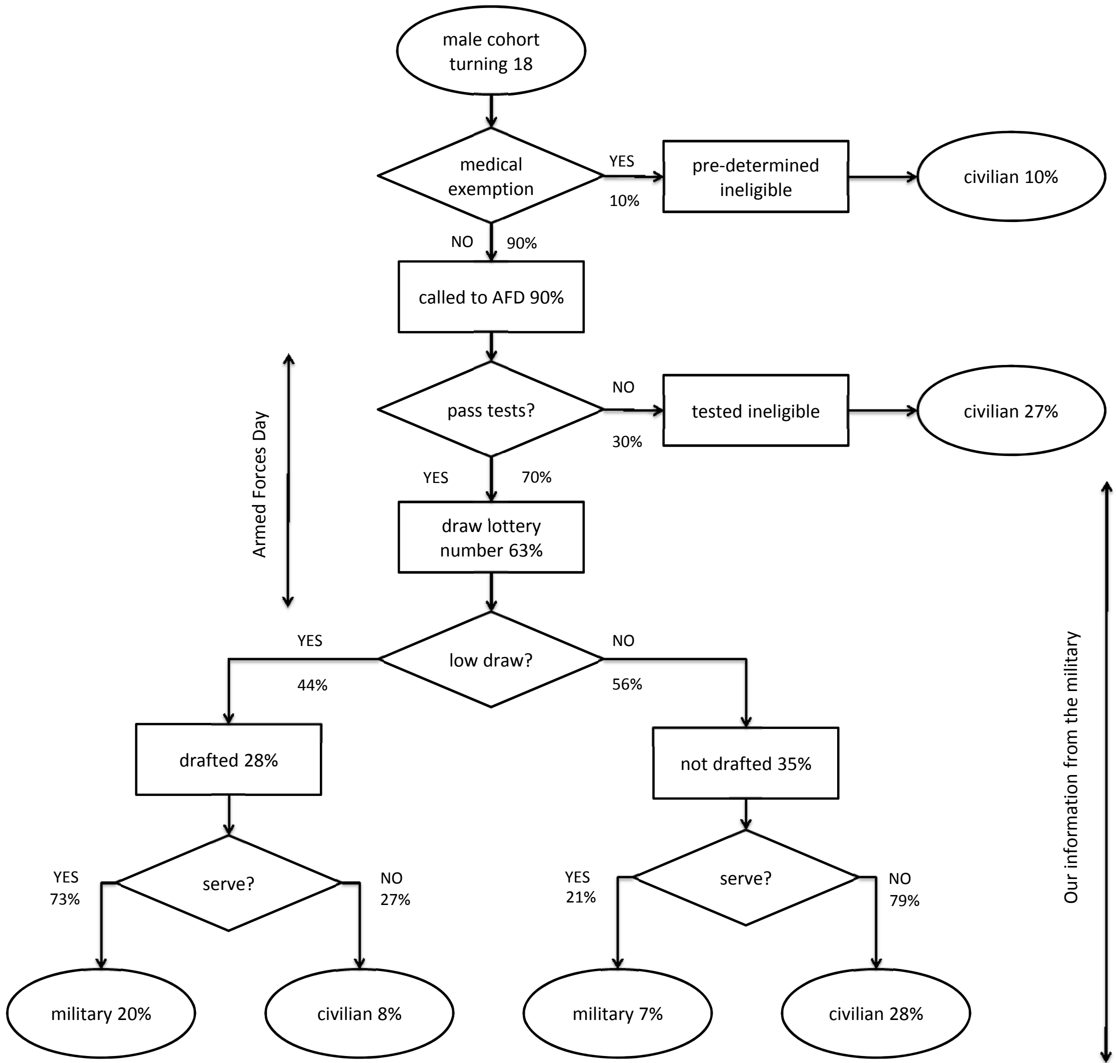

FIGURE 1.—FLOW CHART OF THE CONSCRIPTION PROCESS

Notes: Numbers inside the shapes denote average percentages of our birth cohorts 1976-83. Numbers beside the arrows denote average percentages taking each route conditional on reaching the junction. The AFD includes test taking and drawing lottery numbers. Our estimation dataset contains information on all those who drew a lottery number. See Section 2.2 for a more detailed description of conscription enforcement.

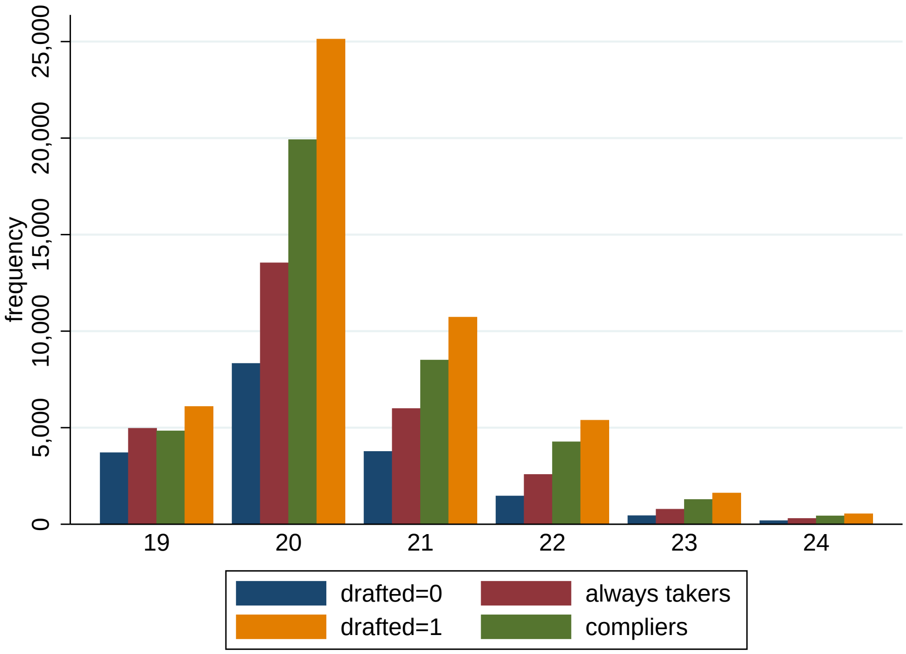

FIGURE 2.—SERVICE AGE DISTRIBUTION BY DRAFT AND COMPLIANCE STATUS

Notes: For those who serve among our 1976-83 birth cohorts, frequencies of those drafted and not drafted are shown in blue and yellow respectively. Assuming draft assignment is independent of response type, we infer frequencies of always takers from sample proportions of volunteers who are not drafted. Further assuming no defiers, we infer frequencies of compliers.

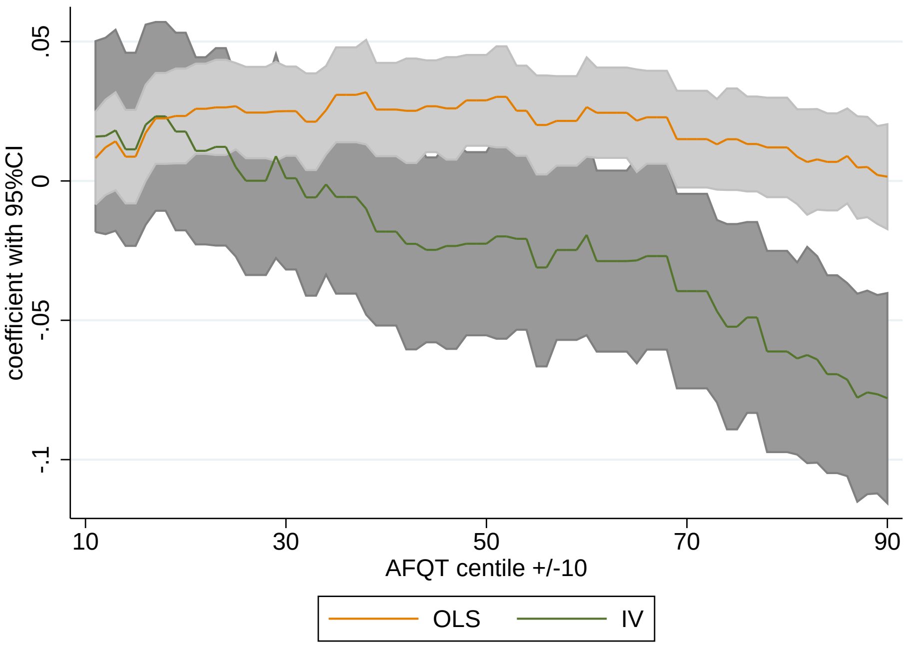

FIGURE 3.—OLS AND IV ESTIMATES OF SERVICE STATUS COEFFICIENTS IN EARNINGS REGRESSIONS BY AFQT SCORE

Notes: Each point on each line represents a coefficient from a separate regression for a moving 20 percent window of AFQT scores, centered on the AFQT centile denoted on the horizontal axis. The dependent variable is log annual labor earnings during the period 2001-2011 in age range 25-35. The regressions also control for birth year, birth month, timing of the draft lottery, AFQT and height and their square. This is the same specification as for Table 4. Shaded areas represent 95 percent confidence bands.

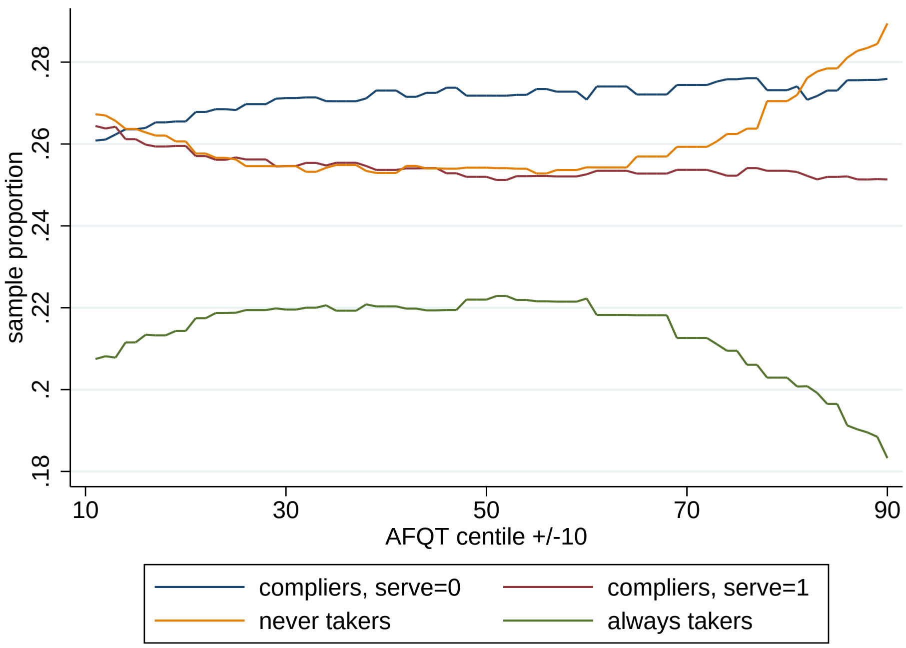

FIGURE 4.—RESPONSE TYPE BY AFQT SCORE

Notes: Assuming draft assignment is independent of response type, implied compliance type proportions are calculated for a moving 20 percent window of AFQT scores, centered on the AFQT centile denoted on the horizontal axis.

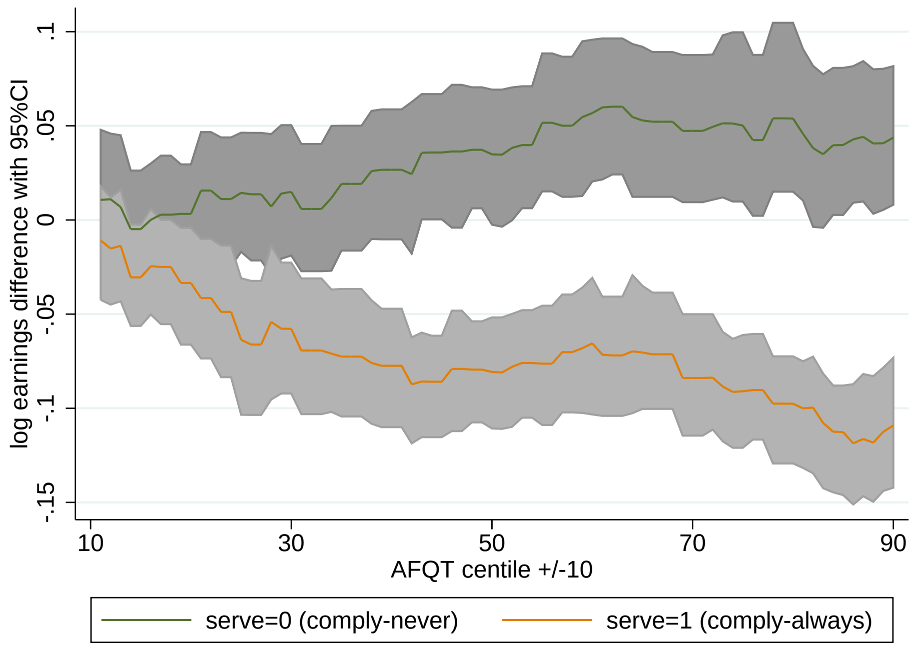

FIGURE 5.—UNDERSTANDING OLS-IV DIFFERENCES BY RESPONSE TYPES OVER AFQT SCORES

Notes: Differences in log earnings coefficients, without additional covariates, for those who serve, between compliers and always takers are denoted by the yellow line. Differences for those who do not serve, between compliers and never takers are denoted by the green line. Estimates are for a moving 20 percent window of AFQT scores, centered on the AFQT centile denoted on the horizontal axis. Calculations follow Imbens and Rubin (1997). 95 percent confidence bands are computed from 1000 bootstrap replications, clustering at the individual level.

Appendix for online publication

A. Additional tables and figures

TABLE A.1.—EFFECT OF MILITARY SERVICE ON EARNINGS ACROSS QUARTILES OF AFQT SCORES. OUTCOME: LOG OF EARNINGS EXCLUDING TAXABLE BENEFITS.

	Full sample (1)	1st quartile (2)	2nd quartile (3)	3rd quartile (4)	Top quartile (5)
Service status=1	-0.0287***	-0.00222	-0.0180	-0.0251*	-0.0709***
	(0.00634)	(0.0121)	(0.0120)	(0.0132)	(0.0134)
Observations	1027534	280673	267042	226050	253769
Individuals	145571	40084	37732	31751	36004
Angrist Pischke F-stat	41060	9927	10896	9852	10672

Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.010$.

Notes: Each cell represents the coefficient of interest from separate instrumental variables regressions. Specification is the same as for Table 4, except here the earnings measure excludes sickness and leave benefits. The number of observation is different because of missings for this earnings measure.

TABLE A.2.—EFFECTS OF MILITARY SERVICE ON EARNINGS AT AGES 19-24

	Full sample (1)	1st quartile (2)	2nd quartile (3)	3rd quartile (4)	Top quartile (5)
Service status=1	-0.0344***	-0.0598***	-0.0558***	0.00712	-0.0196
	(0.00756)	(0.0145)	(0.0146)	(0.0158)	(0.0156)
Observations	854972	237168	223032	186464	208308
Individuals	42970	11780	11248	9432	10510
Angrist Pischke F-stat	45097	11433	11847	10493	11516

Standard errors in parentheses.* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Notes: Each column contains the coefficient of interest from separate instrumental variables regressions, corresponding to the specification of first stages presented in Table 3. The dependent variable is log annual labor earnings during the period 2001-2011 in age range 19-24, reflated to 2012 Danish Kroner.

For our analysis sample we only know AFQT scores for men judged fit for service, but for more recent cohorts, we have access to AFQT scores for all men attending the AFD. In this appendix we compare AFQT scroes with Grade Point Averages

(GPAs) from tests taken in grade 9, the final year of compulsory schooling. All public lower secondary school pupils must take 9th grade tests, with the exception of pupils with special educational needs status.

Figure A1 graphs attendance rate at the AFD across the distribution of 9th grade GPA in the mandatory subjects: Danish, Math, Science and English. Average attendance is 97 percent and attendance increases from about 93 percent in the lowest quintile GPA to the middle quintile where it stabilizes at 98 percent. Since on average 15 percent of a cohort do not attend an AFD, most of those not attending the AFD did not have a 9th grade GPA, probably due to special educational needs status.

Conditional on attending the AFD, we can see from Figure A2 that on average 80 percent pass the AFQT threshold for being fit for service. The AFQT pass rate increases steeply in the lowest GPA quintile, and continues to increase, but more slowly, higher up the GPA distribution. Similarly conditioning on AFD attendance, Figure A3 shows an almost linear relationship between AFQT score and 9th grade GPA, with correlations in Danish, Math, Science and English of 0.61, 0.71, 0.56 and 0.52 respectively.

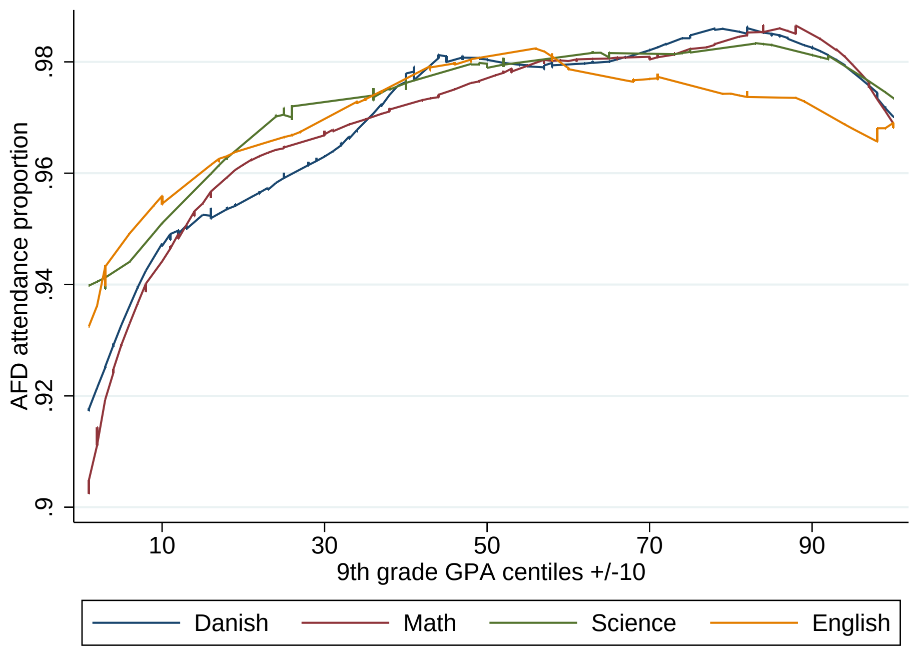

FIGURE A.1.—AFD ATTENDANCE PROPORTION BY GRADE POINT AVERAGE AT 9TH GRADE.

Notes: Attendance rate at the AFD is calculated as the proportion with an AFQT score among those in a 20 percent window of 9th grade test scores in the mandatory subjects. Birth cohorts 1988-90 are the first with AFQT scores for all AFD participants and overlap with the last three years with a 13-point GPA grading scale.

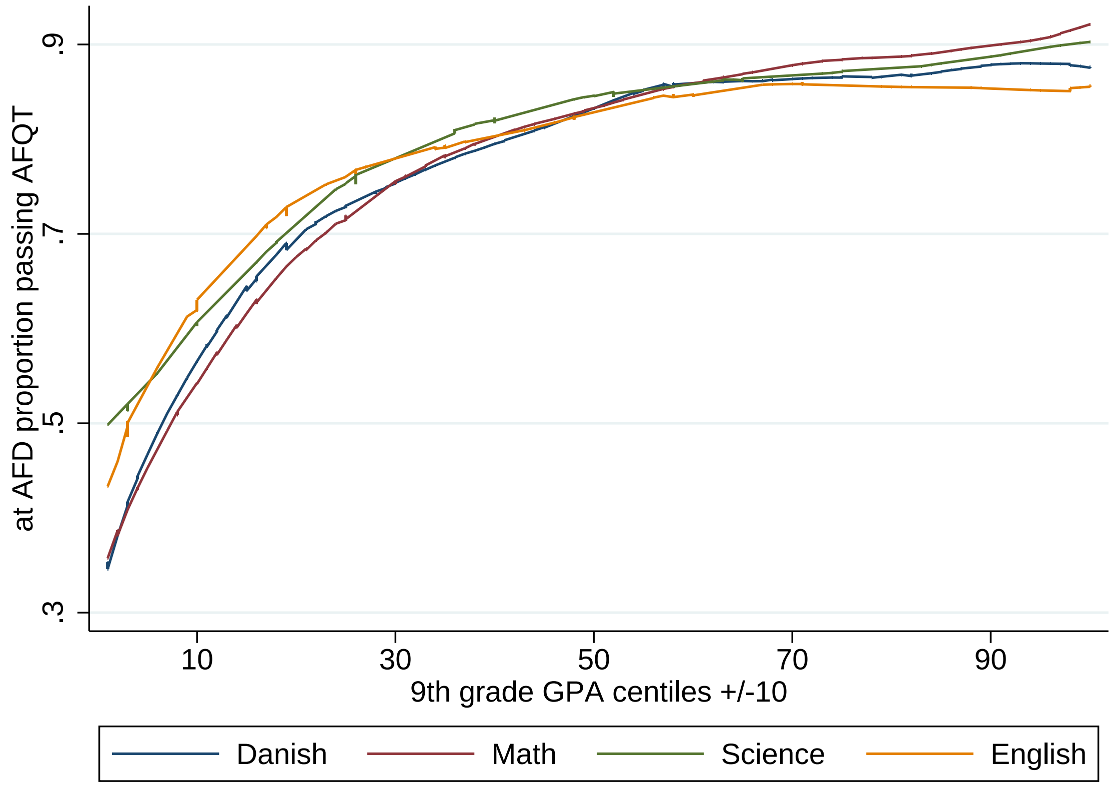

FIGURE A.2.—AFQT PASS RATE AT AFD BY GRADE POINT AVERAGE AT 9TH GRADE.

Notes: Pass rate at the AFD is calculated as the proportion with an AFQT score above the fit-for-service cutoff, among those in a 20 percent window of 9th grade test scores in the mandatory subjects.

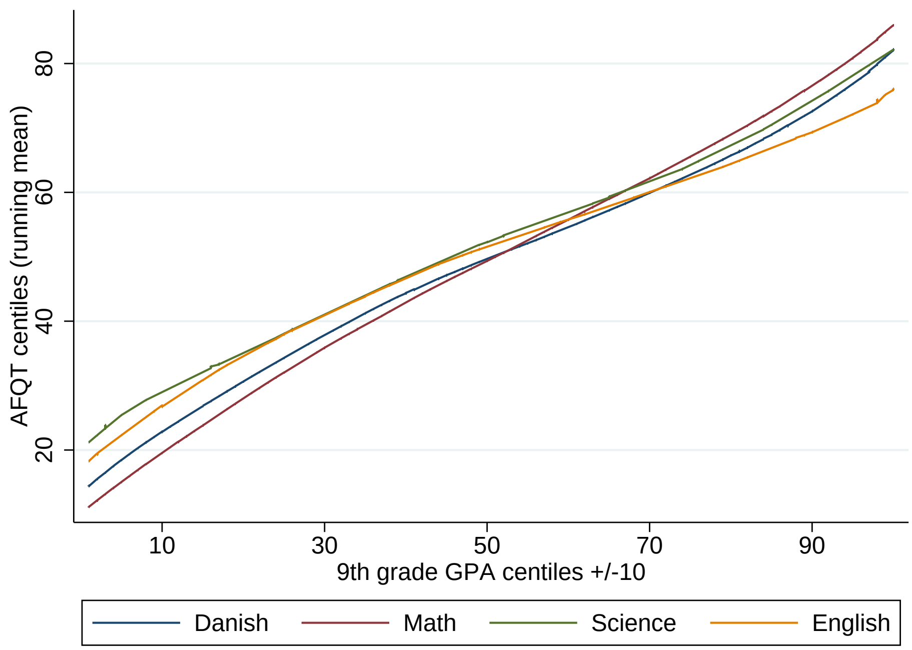

FIGURE A.3.—AFQT SCORE MEAN BY GRADE POINT AVERAGE AT 9TH GRADE.

Notes: AFQT scores are graphed as a running mean over a 20 percent window of 9th grade test scores in the mandatory subjects.

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Notes

[1]Remarks written by Benjamin Franklin with a pencil on the margin of a report by Judge Foster, containing the judge's arguments in favor of the right of impressing (conscripting) seamen. Published posthumously in, Benjamin Franklin and William Temple Franklin *Memoirs of the Life and Writings of Benjamin Franklin* Vol.3, p. 169.

[2]Lyrics from a rap song describing reluctance to join the army when drafted. From Public Enemy (1989) Black Steel in the Hour of Chaos, from the album "It Takes a Nation of Millions to Hold Us Back". Def Jam Records. Track 12, verse 1.

[3]In 1966 a University of Chicago conference brought together academics and politicians to discuss the draft. Among the politicians were Congressman Donald Rumsfeld (Republican-Illinois), Senator Edward Kennedy (Democrat-Massachusetts) and among the economists were Milton Friedman and Walter Oi. The conference contributions from Friedman (1967), Oi (1967a) and others were published in Tax (1967). Friedman and Friedman (1998) viewed this conference as having been the key event starting momentum towards ending the draft. Friedman was a member of the Gates Commission, Oi gave influential testimony, and the Gates et al. (1970) report found that an adequately sized military could be recruited without conscription. Henderson (2005) and Warner and Asch (2001) are excellent overviews. U.S. military conscription ended in 1973. Recently, a number of countries, e.g., Sweden, France, and Germany, have abolished military conscription. In November 2012 a majority in the Danish Parliament agreed to keep the draft lottery but to lower the number of conscripts.

[4]See Altman and Fletcher (1967), Altman and Barro (1971), Hansen and Weis-

brod (1967) and Oi (1967b).

[5]An overview of conscription ages and length of military service across the world appears in CIA's world fact book (see https://www.cia.gov/library/publications/the-worldfactbook/fields/2024.html).

[6]Although most countries draft only men, according to the CIA world fact book, as of 2013 nine countries also drafted women.

[7]During the period of exposure to military service that we consider from 1994 to 2007, the Danish military was involved in peace keeping and peace enforcing operations in Afghanistan, Bosnia, and Iraq. While conscripts were not required to serve abroad, they could volunteer to do so. Throughout the paper we refer to this period as "peacetime", to distinguish it from, say, conscription during the Vietnam war, and to place ours alongside other recent studies in which conscription does not entail military combat.

[8] In an early study of the distribution of opportunity costs of the draft, Berney (1969) focuses on progressivity of the draft tax. The tax is progressive when tabulating civilian mean earnings by schooling attainment but weighting by probability of serving shows that high school graduates face most of the burden.

[9]A related literature estimates the effect of conscription on other outcomes, such as education, disability and crime. Maurin and Xenogiani (2007), exploiting the abolition of mandatory conscription in France, show that educational achievements fell after the abolition, as incentives for men to stay in education weakened. Cipollone and Rosolia (2007) use an exemption from compulsory military service granted to a few cohorts of Italian men and show that the exemption increased their high-school graduation rates. Card and Lemieux (2001) look at college attendance and draft avoidance in the Vietnam-era US. Keller et al. (2010), using aggregate data

from OECD countries, find that conscription is weakly associated with lower enrollment in higher education. Autor, Duggan and Lyle (2011) find a rise in disability benefit receipt among U.S. Vietnam veterans. Galiani et al. (2011) finds that the draft lottery in Argentina increased the risk of committing crime. In contrast, Albæk et al. (2013), in work parallel to ours, also use the Danish conscription lottery and show that risk of military service reduces crime among youth offenders. Their data includes only a cohort of men born in 1964 and residing in the eastern part of Denmark.

[10]Since 2004 women have been invited to participate in the AFD, but not in the lottery.

[11]A small minority of army placements can last longer than 8 months, e.g., service with the Royal Guards lasts 12 months. At the end of our sample period, 336 men (0.2 percent) were subject to the new four month service requirement from 2006.

[12]For comparison, the minimum monthly wage in construction was 16,000 DKK (3,300 USD).

[13]A third party, TDC/AS, previously Tele Danmark, is responsible for generating and delivering the lottery numbers.

[14]For the remainder of the paper we refer to men only because women do not particpate in the draft lottery

[15]Article 25 (paragraph 2) Law of Military Service.

[16]For practical purposes, criminal background checks are only run after the lottery and for draftees and volunteers.

[17]The standard earnings measure that we use includes sickness benefits and pa-

ternity leave benefits. In sensitivity checks we exclude these benefits from our earnings measure.

[18]The earnings measure excludes occupational pension contributions made by employers and employees. The reason is that tax payment is deferred until the pension is drawn.

[19]The Ministry of Education calculates the minimum time necessary for achieving each qualification by the shortest route. Qualifications are ranked according to this normed time. Individuals are imputed the schooling length associated with their qualification with the longest normed time.

[20]Estimates available on request.

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

[22]We define the sons of immigrants as individuals born in Denmark and having neither parent born in Denmark.

[23]The variable indicates whether the person was placed in out-of-home care (including foster care and residential care homes) before the age of 18. Registered stays range from one week to several years.

[24]The measure takes on the value zero if the person was living together with both legal parents on his 17th birthday.

[25]Estimates by AFQT quartile for sub-groups are available on request.

[26]Earnings are observed until 2011. Earnings at age 35 are only observed for the 1976 birth cohort, earnings at 34 are also observed for 1977 births, etc. Estimates separately by age are available on request.

[27] See Card and Lemieux (2001) for similar findings.

[28]Another channel, which we do not explore in this section, is that military service may build human capital by teaching certain vocational skills. 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.

[29]Study grants and study loans are not included in our earnings measures.

[30]Throwing the zeros out might be problematic if serving is related to the chance of having zero recorded earnings. As it turns out, only 3481 observations, or 2 percent, have missing data on earnings. We have run regressions on the effect of serving on the probability of having zero recorded earnings on serving, where we use the lottery outcomes as our instrument. For the top quartile of AFQT scores, where the earnings penalty is largest, serving is never a significant predictor of having missing earnings. At the lower quartile, serving is a significant predictor of missing earnings at some ages, mostly at the 10 percent level, and insignificant at other ages. Results are available on request.

[31]Mortgage debts and value of stocks and bonds are excluded from this measure.

[32]For bank balance, crime, and health outcomes, we include the pre-conscription values of these variables as additional controls. As expected, none of these predict assignment to military service.

[33]Albaek et al. (2013) found that conscription reduces property crime for youth offenders. However, checking specific types of crime is beyond the scope of our paper.

[34]We obtain data from the Danish Medicines Agency about mental health-related

medicine which is prescribed by general practitioners and purchased in drug stores. Data on diagnoses for psychiatric problems and treatments for addiction problems are obtained from the National Board of Health hospital discharge register.

[35] In the U.S. rates of compliance with assignment to serve were 2-16 percent for Whites and 1-6 percent for non-Whites, but these are percentages of everyone with a selected date of birth and do not account for ineligibility. In Denmark only those fit for service enter the draft lottery, and 73 percent comply with the assignment to serve. Also including the unfit for service in the denominator, for the sake of comparison with the U.S., we obtain a compliance rate of (20/65) 31 percent.