Draft Lottery Effects on Schooling, Earnings and the Next Generation

Paul Bingley[1], Kristoffer Markwardt[1], Vera Ehrenstein[2] and Lars Pedersen[2]

[1]SFI - The Danish National Centre for Social Research

[2]Department of Clinical Epidemiology, Aarhus University

Abstract

Do parents with more schooling have children with more schooling because of their schooling? To identify the effect on offspring schooling, we study fathers subject to a peacetime military draft lottery in Denmark, a lottery providing exogenous schooling variation. The father-offspring schooling correlation is 12 per cent after controlling for father cognitive test scores and grandparent schooling. We find that father random assignment to nine months of military service reduces father's schooling by nine months, implying a return to schooling of 4.9 per cent and a reduction in father's lifetime earnings by 4.2 per cent, but find no significant effect on offspring schooling.

Keywords: Draft lottery, schooling, lifetime earnings, intergenerational transmission

JEL Classification: I21, J13

We are grateful to Josh Angrist, Arnaud Chevalier, Paul Devereux, Mette Ejrnæs, Stephen P. Jenkins, Søren Leth-Petersen, Gunnar Lauge Nielsen, Kjell Salvanes and Henrik Toft Sørensen for comments. Funding was provided by the Danish Strategic Research Council (grants DSF-09-065167 and DSF-09-070295). All errors and omissions are our own. Corresponding authors: Bingley: pbingley@sfi.dk; Markwardt: ksm@sfi.dk

I. Introduction

Parental education explains a large part of offspring success at school and later in life (Haveman and Wolfe, 1995; Björklund and Salvanes, 2011). However, parental ability and financial resources also explain much about offspring outcomes, and correlations between different measures of parental resources make the differentiation of causal relationships from other intergenerational associations a challenge (Duncan, Morris and Rodrigues, 2011; Black and Devereux, 2011). Whereas cognitive ability is largely set early in life, greater scope exists for interventions aimed at changing schooling and financial resources (Cunha, Heckman, Lochner and Masterov, 2006; Cunha and Heckman, 2007). The malleability of schooling, in combination with the strong association between parental and offspring schooling, leads to the question of whether the relationship between parental schooling and offspring schooling is causal. Different approaches to the identification of causal effects either control for selection of parents into education on the basis of their ability or find exogenous variation in parental education or financial resources (Holmlund, Lindahl and Plug, 2011). However, as the remainder of this section will show, studies that control for selection on ability require rather strong assumptions. Our study is the first to use a peacetime military draft lottery to provide variation in parental schooling for estimating intergenerational transmission effects.

Adoption studies deal with the correlation between parental ability and schooling – a correlation that might otherwise produce misleading inferences – by distinguishing between birth and rearing parents. In the absence of selective placements, the schooling of birth and rearing parents should be uncorrelated, as should the ability of birth and rearing parents. Although no studies observe ability and schooling for both sets of parents, studies that observe schooling find both birth and rearing parents to be important (Björklund, Lindahl and Plug, 2006). Whilst adoption studies help resolve the relative importance of endowments at birth versus environment later in life, they do not deal with the simultaneity of parental ability and parental schooling, a simultaneity that might bias inferences. Even if researchers observe both measures for birth and rearing parents, if they are correlated within parents, adoption studies will not be informative about the distinction between ability and education playing an independent causal role in determining child outcomes.

Children-of-twins designs control for selection into education on the basis of ability by differencing within pairs of monozygotic (MZ) twin parents. MZ twins share all segregating genes at the time of conception. Under the assumption that MZ twins continue to share ability as adults and when they become parents, differences in MZ twin parent schooling will be purged of differences in MZ twin parent ability. Behrman and Rosenzweig (2002) find that the schooling of MZ mothers does not affect offspring schooling, whereas that of MZ fathers does. Similar to the adoption design, the children-of-twins design resolves the importance of endowments early in life from later environmental influences. The crucial assumption is that no MZ ability differences emerge after conception, However, if MZ twin parental ability differences exist, and if they are correlated with schooling (see Sandewall, Cesarini and Johannesson, 2014), then children-of-twins studies cannot be informative about whether parental schooling reflects selection on ability, or whether schooling itself has a causal effect on child outcomes.

Instrumental variables (IV) approaches aim at obtaining exogenous variation in parental schooling through policies directly affecting only parental schooling, thereby providing exclusion restrictions for identification of the parental schooling effects on their children's schooling. Under the assumption that instruments do not have a direct effect on the next generation's outcome of interest, other than through parental schooling, the IV approach identifies a local average treatment effect (LATE), on outcomes for children whose parents behavior is changed by the instrument.

Several IV strategies have been applied to intergenerational transmission, and distinguishing between dictatorial and incentive-based instruments is useful (Belzil and Hansen, 2010). Mandates for minimum years of schooling or grade completion are dictatorial because those who would have previously chosen minimum schooling must take more schooling to satisfy the new higher minimum requirement, regardless of ability and the opportunity costs of (otherwise legal) alternative employment. An example of a dictatorial policy appears in the Black, Devereux and Salvanes (2005) study of Norwegian compulsory school grade completion.

Alternatively, incentive-based instruments use changes to direct schooling costs, such as school building, education subsidies, or schooling disruption. These are incentive-based because the cost of alternatives changes, without any compulsion. Amongst groups with changed costs, individuals can still choose schooling on the basis of ability and opportunity cost, and it is differences in cost changes that provide identification. An example of an incentive-based policy appears in Currie and Moretti (2003), who use college openings in county of residence as an instrument for schooling.

Military draft lotteries during times of conflict have been used as instruments for studying the effect of military service on health (Hearst, Newman and Hulley, 1986) and lifetime earnings (Angrist, 1990). Peacetime draft lotteries have more recently been used for explaining military service effects on crime (Galiani, Rossi and Schargrodsky, 2011; Albæk, Leth-Petersen, Le Maire and Tranæs, 2013). We are the first to use a draft lottery to provide instruments for parental schooling for estimating intergenerational transmission. Although the draft is a mandate, for the purposes of schooling transmission we consider the draft an incentive-based intervention that affects the costs of schooling through disruption to educational careers. As always, a LATE interpretation prevails (Imbens and Angrist, 1994), and we identify the effects on offspring schooling for fathers whose assignment to military service disrupted their schooling careers.

We use the Danish 1955 draft lottery, which randomly assigned 18-year-old men to nine months of peacetime military service. Ordinary Least Squares (OLS) estimates controlling for Armed Forces Qualifications Test (AFQT) scores and grandparents' (the parents of the draftee) schooling give low intergenerational correlations. The draft lottery instrument is random and balanced after weighting by AFQT scores, and we find that assignment to the military significantly lowers father schooling and lifetime earnings. We find no significant intergenerational effects of fathers' schooling for fathers whose schooling attainment was affected by assignment to military service.

The estimation of intergenerational effects requires data covering a period long enough to observe outcomes for both father and child generations. As many individuals complete schooling in their mid- or late twenties, the data requirements for our analysis are substantial. To benchmark our findings, we exploit a schooling reform that affected older birth cohorts. Although for these older cohorts we find significant intergenerational transmission, our censoring of the data to have a shorter follow-up, shows insignificant effects, similar to those for our draft lottery cohort.

The remainder of the paper is organized as follows. Section II explains the Danish draft lottery procedure, describes the data, and outlines our empirical strategy. Section III presents our estimates of the draft lottery effect on schooling attainment and shows that the draft lottery provides a strong instrument for schooling. Section IV presents and discusses our results for the the draft lottery effects on the next generation and the implied intergenerational transmission of schooling. Section V illustrates how estimates of intergenerational transmission of schooling are affected by the length of the follow-up period and sample size. As a robustness check of the applicability of the draft lottery as an instrument for schooling, Section VI presents estimates of draft lottery effects on the fathers' earnings and the implied financial returns to schooling. Section VII concludes.

II. Institutional details and data

A. Institutional details

At age 18, almost all Danish men are called to appear before the military draft board for physical and mental examinations.[1] The assessment of their physical condition is based on vision and hearing tests and an examination by a military physician.

[1]Some are exempt due to severe physical or mental impairment, as documented by a regional public hospital physician.

Cognitive test results and the physical examination determine draft eligibility. The information to which we have access is for men who passed all the tests and were deemed draft eligible. All of these men participate in the draft lottery. Those who draw a lottery number below a certain threshold are drafted. The precise threshold value depends on the staffing needs of the military and the civil defense, as well as the outcome of the recruitment process in each of the other five conscription districts. Although drafted men can request placement with a particular service (e.g., army, air force, navy), assignment is subject to capacity constraints. The young men in our sample were all born in 1955. By default, they would enter the draft lottery in 1973 for service in 1974.

During the 1950s and 1960s the Danish armed forces consisted entirely of draftees, and a large majority of draft-eligible men were drafted (Bjerg, 1991). The military draft sought to increase social cohesion by random selection across the population of Danish men. All (draft-eligible) men had to make themselves available to serve their country for a limited period and thus ex ante faced the same risk of being drafted. A reform of Danish military recruitment was enacted in 1973, aimed at reducing the size of the armed forces and eventually leading to an even balance of draftees and career soldiers volunteering for subsequent service.

From 1973 the share of draft-eligible men who were drafted began to decrease, and by the end of the 1970s the drafted share of a cohort was approximately 50 per cent (Lyng *et al.*, 2000). As the demand for draftees decreased, an increasing share of draft-eligible men were exempted from military service—even if they wished to serve. Thus to ensure that any draft-eligible young man willing to volunteer was given the opportunity, Danish legislators voted in March 1975 to allow voluntary sign-up for military service regardless of the draft lottery outcome (Bjerg, 1991).

The men in our sample, all born during the first six months of 1955, underwent draft examinations in autumn 1973. Structural changes implemented in 1973 caused bottlenecks in the recruitment system (Bjerg, 1991) and may have affected the timing of some draft examinations. However, Statistics Denmark, (1975-1980) shows that almost all of the 1955 birth cohort participated in draft examinations before 1975. That is although a significant proportion were not drafted, volunteering remained impossible.

Whilst our data contain the individual lottery numbers, we do not know the draft or service status of each of the individuals. However, we know that 90 per cent of draft-eligible men who entered the lottery nationwide were drafted in 1974 (Lyng *et al.*, 2000).[2] Although we do not know the draft rate at the regional level, the then impossibility of self-selection by volunteering implies an expected draft rate of 90 per cent for our sample of draft-eligible men.[3]

Before the 1973 reform the length and nature of military service differed substantially amongst the various branches. After the reform was enacted, the length became generally nine months. Whilst draftees had the opportunity of becoming conscientious objectors, they would then be forced to provide unskilled minimum wage labour (e.g., as an aide in a kindergarten) for a year. As our data is uninformative about actual military service, we do not distinguish between military service and conscientious objection but refer to military service generically.[4]

B. Data

We use a data set of 2,722 Danish men born within the first six months of 1955, all of whom participated in the draft lottery in the fifth conscription district of Denmark, which mainly covers the two counties of North Jutland and Viborg.[5] These data contain the draft lottery numbers together with information on height, weight and mental condition.

Mental condition is measured by a test of cognitive abilities, called the Børge Prien test, developed for the Danish draft and first used in 1957. The test comprises four time-limited

[2]Although a draftee could potentially affect the timing of military service, his draft status was determined by the outcome of the draft lottery in which he participated, and he would remain drafted even were he able to postpone service.

[3]We assess the robustness of the draft status random assignment in Section III.A.

[4]At the time around 15 per cent of draft-eligible men were conscientious objectors (Bjerg, 1991).

[5]These counties refer to the administrative division of Denmark prior to the merging of some counties in 2007.

subtests—letter matrices, verbal analogies, number series and geometric figures—with a total of 78 items. The questions are not multiple choice and the score is the total number of correct answers. The test is highly correlated with the Wechsler Adult Intelligence Scale verbal Intelligence Quotient (IQ) (0.78), performance IQ (0.71), and full-scale IQ (0.82) (Mortensen *et al.*, 1989).[6] As the Børge Prien test is the Danish equivalent of the U.S. AFQT, we use the term AFQT throughout the remainder of the paper.

We link the draft lottery number, AFQT score, height and weight to tax authority records of annual labor earnings and education ministry records of the highest level of completed schooling. We have access to annual register data from 1980 to 2011. As the men in our sample were all born in 1955, the data from the registers span ages 25-56, i.e., almost their entire working career.

In addition, the fertility register provides links between parents and children, enabling us to link these men to their parents, to each of their children, and to the mother of each child. For each family member we extract information on schooling attainment from the administrative registers. As reports on schooling attainment date back to 1920 births, we are unable to retrieve this information for the parents of some draft-eligible men.[7] For the parents of draft-eligible men who have missing schooling information we impute seven years of schooling, which was the minimum requirement before 1972 and is the modal value in our data (61 per cent of fathers and 77 per cent of mothers of the draft eligibles obtained seven years of schooling). Our initial data set consists of the 2,722 draft-eligible men for whom we observe a draft lottery number.

Table 1 presents summary statistics for our sample of draft-eligible men born within the first six months of 1955 (column one), all men from the catchment area (column two) and the population of Danish men born within the same six-month period. The table presents means and standard deviations for both pre-lottery characteristics (pre-determined) in the upper pane

[6]For further information on the Børge Prien test, see Kousgaard (2003) (in Danish).

[7]We are able to link 81 per cent of the draft eligibles to their father and 84 per cent to their mother.

and post-lottery characteristics (potentially endogenous) in the lower pane.

[Table 1 about here]

On average, the draft-eligible men in our sample are representative of the local catchment area for military recruitment. This area is mostly rural and includes Denmark's third largest city. However, our draft-eligible sample differs somewhat from the population at large. We can link 81 per cent of the draft-eligible men in our sample to their parents through the fertility register. For the male population in these cohorts in Denmark as a whole we can only link 69 per cent to their parents. Linkage to parents is less complete for earlier cohorts of parents and more links for our sample suggests that the parents of our draft lottery participants are younger than the population average. Of the observed parents of our sample, those with a draft-eligible son on average completed less schooling than parents of sons born in 1955 in the population as a whole. Parents who have a son in 1955 are of similar age in our sample as in the population. The lower pane of Table 1 shows that whilst both completed years of schooling and earnings for the draft-eligible men at age 35 are similar to those of the population as a whole, the draft-eligible men in our study were more likely to have become fathers by age 41.

The differences between our sample and the population at large are consistent with known regional differences across Denmark. We study men who took part in a draft lottery in a particular region of the country and were randomly assigned to military service. We investigate this random assignment, which is essential to our analysis design, in the next subsection.

C. Randomness of draft lottery assignment

In a properly administrated randomized controlled trial, random assignment is assured by design. However, although draft lottery numbers are drawn at random, the draft lottery is not a randomized controlled trial for the effects of military service. Therefore, we show here that our observational study has balanced random assignment. The military recruitment procedure was such that all those judged fit-for-service drew a lottery number and were assigned to service on the basis of this draw. The data to which we have access for the 1955 cohort contains information only for men deemed fit for service. Figure 1a presents AFQT score distributions by draft status. The distribution of light bars on the right for those not drafted is the more noisy because of fewer observations (10 per cent) than for the drafted (90 per cent) which is the dark bars to the left. It is evident that those with low AFQT scores are somewhat more likely to be drafted than not.

[Figure 1 about here]

For more recent cohorts we observe the full distribution of AFQT scores for all draft examination participants (born in 1992) and for those fit for service (born in 1979-1981). Figure 1b presents these distributions, with the dark bars on the left showing AFQT scores for all draft exam participants, and the light bars on the right showing the distribution of scores above the eligibility cutoff of 27, which pertains to recent cohorts. This data comes from the National Board of Health, and although we do not know about draft status, the truncated distribution shows the explicit selection of those entering the lottery in recent years. For our 1955 cohort there appears to be some selection into the draft amongst those with low AFQT scores.[8] To restore balance in draft assignment across the AFQT distribution, we weight observations according to the uncensored AFQT score distribution for the 1979-1981 draft-examination participants in bins of five scores; i.e. we give each combination of draft assignment and AFQT score (in bins of five points) a weight to align the AFQT distributions by draft status.[9]

Table 2 presents tests for whether draft assignment is balanced on pre-assignment characteristics, after having weighted by population AFQT score bins. Only the linear term in Body

[8]It is not clear whether unbalanced draft assignment across the AFQT score distribution is due to manipulation by draft lottery participants or administrators, or simply a result of variation in our relatively small sample. Our empirical approach is agnostic with regard to the cause and we re-weight to restore balance.

[9]Weighting according to observed AFQT scores within the 1955 birth cohort gives similar results, which are available upon request.

Mass Index (BMI) is significant at the 10 per cent level.[10] Importantly, father and mother schooling do not determine assignment.[11] The insignificant F-Statistic shows that AFQT score weighted lottery assignment is balanced on pre-assignment characteristics. Throughout the remainder of the paper we use the weighted balanced assignment data.

[Table 2 about here]

III. Draft lottery effects on schooling

A draftee is called for military service around the age of 19 and serves either during a year out from school or during the very early stage of his working career.[12] Military service is therefore a disruption in human capital accumulation for draftees, increasing the relative cost of the human capital investment outside the military. As a result, we expect draftees to complete less formal schooling than they would have had they not been drafted.

The draft lottery provides an instrument for military service during peacetime. However, as the detailed register data on individual occupation and employment starts in 1980, we do not observe who actually served in the military. We estimate an intention to treat (ITT) for the effect of being drafted on completed years of schooling. Volunteering was not possible during our sample period, and draftees who ignored the call for military service would face legal proceedings. Compliance was high but not complete.

We estimate the effect of the draft lottery on schooling attainment from the following specification:

$$S_i = \alpha_0 + \alpha_1 D_i + f(A_i, B_i) + e_i \tag{1}$$

[10] $BMI = \frac{weight}{height^2}$ (weight in kilograms and height in meters).

[11] The insignificance of the estimates on father and mother schooling is unaffected by the choice of imputation measure. For example, imputing mean father and mother schooling for parents without schooling information, yields similar, insignificant estimates.

[12] He can postpone military service until after a period of continuous *s*chooling but then has to serve immediately upon graduation.

where S denotes years of schooling and $f(A,B)$ represents second order polynomials in the pre-lottery characteristics, standardized AFQT score (A) and BMI (B). Throughout the paper, we include BMI as a measure of physical condition rather than height and weight separately.[13] D is an indicator of draft status taking a value of 1 if individual i draws a lottery number below the threshold value for a 90 per cent draft rate. We estimate (1) by OLS. Given the randomness of the draft lottery, $\hat{\alpha}_1$ is an unbiased and consistent estimate of the draft lottery effect on years of completed schooling.

Table 3 shows the estimates of (1) with and without pre-lottery control variables; the two specifications produce similar draft lottery effects. The estimated effect implies that being drafted, on average, reduces completed years of schooling by nine months. The results further show that the AFQT score is a strong predictor of schooling outcomes, as an increase of one standard deviation in the AFQT score is associated with one more year of completed schooling. The correlation between BMI and years of schooling is positive (negative) at BMI values below (above) 21.

[Table 3 about here]

A. Draft assignment robustness check

For our 1955 cohort, we do not observe the actual draft status, and we therefore infer the draft lottery threshold value from the nationwide draft rate of 90 per cent. Although the impossibility of volunteering implies an expected local draft rate of 90 per cent, we cannot know whether the draft share varied locally. Therefore, to assess how the estimated effect of draft lottery status on schooling varies by threshold values, we conduct a a robustness check of our implied lottery threshold of 22,000 out of 36,000 by running the regression specified in (1) for all possible thresholds.

[13] When we run the analyses with height and weight separately, all conclusions remain the same, and point estimates of the main variables change only marginally.

We loop through the possible thresholds and for each instance create the binary draft indicator, *D*, and estimate (1). Figure 2 presents the point estimate of α_1, together with its F-statistic from each of these regressions. The figure shows that, at low threshold values, the estimated effect is small and statistically weak. For threshold values above 15,000, the (negative) effect becomes bigger and statistically stronger. At a threshold value of 22,000, the F-statistic peaks at a value of about 35, and the point estimate stabilizes between -0.70 and -0.75. At higher threshold values, the point estimate remains stable but becomes statistically weaker.

[Figure 2 about here]

The graphs in Figure 2 clearly show that the threshold value of 22,000 produces the statistically strongest estimate of α_1 in (1). We argue that if draft status affects schooling outcomes through military service due to disruption then—given the distinct pattern in the figure—22,000 is most likely the correct threshold value.

For more recent (1976-83) cohorts, draft status and service status are observed. Bingley *et al.* (2014) find that draft status is a very strong predictor of service status, with a coefficient of 0.52 and standard error of 0.003 in a period with 27 per cent non-compliance with assignment to serve. Moreover, the implied effects of draft status on schooling attainment at age 25 was -0.36 years in Bingley *et al.* (2014). For the 1976-83 cohorts, assignment was to 8 months of military service rather than the 9 months for the 1955 cohort. Although data constraints preclude similar estimations for the 1955 cohort, evidence from these later cohorts clearly shows draft status effects on schooling operate via military service, even in a period with less compliance.

IV. Intergenerational transmission of schooling

As our analysis of intergenerational transmission of schooling includes observations across three generations, we adopt the following terminology: We refer to the draft lottery participants as ‘fathers’, to their fathers (mothers) as ‘grandfathers’ (‘grandmothers’), and to draft lottery participants’ offspring as ‘children’ whose schooling attainment is the outcome of interest. We use the term ‘child’ regardless of age.

As schooling of children is the outcome of interest, we restrict the sample to draft-eligible men with children. Moreover, because the education registers include only individuals enrolled in the educational system, our sample includes those children who reached the school-starting age during our sample period. This condition reduces the sample from 2,717 to 2,147 draft-eligible men. Between them they have a total of 4,746 children, whom we observe in the education registers. As each of the children enters the sample once, some of the draft-eligible fathers enter the sample multiple times. From the sample of 4,746 children we exclude observations if the mother’s length of schooling is missing, and to avoid incorrect assignment of schooling—systematically too low—to children in the sample, we exclude observations if the child was enrolled in the Danish schooling system at the end of the sample period in 2011. This leaves us with 3,032 observations.

Although we exploit information on a draft lottery from approximately 40 years ago and are able to follow individuals until four years ago, the sample period is not long enough for us to observe completed schooling outcomes for all children. The fathers in our sample were all born in 1955 and were thus 56 years old by the end of our sample period in 2011. If a father had a child at age 25, we would observe his child until age 31, but if he instead had the child at age 35, we would be able to observe his child only until age 21. The timing of parenthood is thus important for our observation of the child’s schooling attainment, as is the length of the child’s schooling. In Denmark most of those attending college graduate in their late 20s (Danish Ministry of Education, 2010). Therefore, if we were to observe realized schooling attainment for those graduates, we would preferably observe schooling attainment by the age of 30.

Figure 3 illustrates the issue. It shows the mean of the children’s years of schooling by their birth cohorts. The left panel contains children who were not enrolled in the Danish schooling system in 2011. The right panel shows the children who were excluded from the analysis due to current education enrollment, assuming they complete the education in which they were currently enrolled.

[Figure 3 about here]

Figure 3 clearly shows that those still enrolled in the educational system are expected to complete more schooling. However, the figure also suggests that the young children (late birth cohorts) may cause problems not only among the excluded observations (right) but also for estimations on the sample of children not enrolled (left). It appears that some individuals registered as not enrolled in 2011 have not yet completed their schooling. Although both graphs show the expected increase in schooling attainment by birth cohort, the trend breaks for birth cohorts around the mid-1980s, i.e., children aged approximately 25 in 2011. Therefore, we restrict the sample to children who were neither enrolled in the Danish schooling system nor less than 25 years old in 2011. The final intergenerational estimation sample comprises 2,418 observations.

We estimate the intergenerational transmission of schooling from the following specification

$$S_i^c = \delta_0 + \delta_1 S_i^f + \delta_2 S_i^m + \delta_3 S_i^{ff} + \delta_4 S_i^{fm} + h(A_i^f, B_i^f) + \eta X_i + \varepsilon_i \qquad (2)$$

with superscripts c for child, f for father, m for mother, ff for grandfather on father's side, and fm for grandmother on father's side. Equation (2) expresses schooling of the child as a function of parental schooling, grandparental schooling, a function (second-order polynomials) of pre-lottery characteristics of the father, and other characteristics (X), including father's birth month and child's gender and birth year.

Because schooling choices of both fathers and their children are affected by unobserved factors such as ability, OLS estimation of (2) may return a biased estimate of β_1 . One common solution to this problem is to find an instrumental variable that is (strongly) correlated with schooling and correlated with the outcome of interest *only* through the father's schooling (see Card, 2001). Given the results in the previous section, we propose the draft lottery as such an instrument and estimate (2) by two-stage least squares (2SLS) with the draft indicator (section III) as the excluded instrument for the endogenous father schooling variable. The first-stage equation for the father's schooling is shown in Equation (1).

The LATE interpretation of our estimates is for men whose schooling status is affected by the draft lottery disruption of educational career. The draft lottery is not an educational intervention per se but an incentive-driven instrument that substitutes military training and experience for time in the classroom and the civilian labor market. Draftees might learn about the virtues of discipline, benefit from the physical exercise, and form social and professional networks. The effect we estimate is the net effect of schooling disruption plus military training and experience, versus civilian life as usual with undisrupted educational and civilian labor market choices.

We estimate four different specifications of (2), by OLS and 2SLS, gradually adding more explanatory variables. The first specification controls for the child's birth year and gender and for the father's birth month. The second specification adds the father's pre-lottery characteristics, i.e., AFQT score and BMI. The third specification adds grandparental schooling to capture the father's family background. The fourth specification includes the schooling of the mother to control for assortative mating. That is, in addition to a wide range of other factors, couples tend to match on educational attainment.

The issue of assortative mating is important to the analysis of intergenerational transmission of schooling (see Holmlund *et al.*, 2011, for a review). How to deal with it depends on the question of interest. If we are interested in the combined effect of the father's schooling and assortative mating, we should not include the mother's schooling in the regression. If, instead, we are interested in the intergenerational transmission of the father's schooling net of assortative mating, we should include the mother's schooling as a regressor. However, multicollinearity between parents' schooling, together with the potential endogeneity of the mother's schooling, complicates the interpretation of the estimated effect. The literature is mixed with respect to specifications with and without the spouse's schooling. Therefore we consistently report both.

Another approach, proposed by Oreopoulos *et al.* (2006), is to assume that the transmission of schooling to children is the same for both parents and to include the sum of parents' schooling as the endogenous regressor. This approach is well suited for the case of a universal schooling reform with two excluded instruments—one for each parent—that are strongly correlated. The restriction to one common excluded instrument increases precision, and if the assumption of equal effects of both parents holds, then the estimated coefficient remains consistent. However, the draft lottery provides an instrument for only the father's schooling, making it difficult to defend the implementation of this strategy and rendering it impossible to test the assumption of equal transmission of fathers and mothers. Therefore, we choose not to include results from such a specification.[14]

Many IV studies use variation in minimum schooling requirements, which affect only those individuals at the bottom part of the schooling distribution.[15] In contrast, the draft lottery puts draft-eligible 18-year-olds at risk of being drafted. Being drafted changes the relative cost of schooling at the high school level and above. Consequently, the LATE we identify is later than the minimum schooling margin, as investigated by Black *et al.* (2005), but before tertiary education, as investigated by Currie and Moretti (2003). Moreover, in contrast to studies that take an IV approach to schooling across generations, our richest specification identifies the effect of the father's schooling on his child's schooling, net the father's cognitive abilities and

[14]For completeness we ran the specification, taking the sum of parents' schooling as the endogenous regressor. The results differ only marginally from the gross effect of the father's schooling and assortative mating, i.e., the specification without the mother's schooling. The results from the "total parental schooling specification" are available upon request.

[15]This local effect assumes that signaling is not strong enough to affect the rest of the schooling distribution. Chevalier *et al.* (2004) employ such a test for signaling in the UK. They exploit the change in minimum schooling requirements to analyze whether the increase in educational attainment at the bottom part of the schooling distribution implies a shift of the entire distribution. They find no support for an effect higher up the schooling distribution.

grandparental background.

Tables 4−7 present the results from OLS, reduced-form, first-stage, and second-stage regressions, respectively, when all schooling variables are defined as completed years of schooling. All results are clustered by fathers (draft lottery participants). The OLS results show a positive and statistically significant intergenerational correlation of schooling. With only the child's birth year and gender and the father's birth month as controls, the estimated coefficient on father's schooling is 0.19, i.e., an extra year of father's schooling is associated with an increase of two months in the schooling of his child. This estimate is in line with comparable studies using children's years of schooling as the outcome measure: Black *et al.* (2005) for Norway (0.22) and Holmlund *et al.* (2011) for Sweden (0.23).[16] The OLS estimate captures endowments passed from the father to his child and "nurture" components provided by the home environment.

The second column of Table 4 includes the father's endowments, in particular cognitive test scores, which are strongly correlated with the schooling of his child and statistically significant. The inclusion of the AFQT scores reduces the intergenerational correlation of schooling by around one third from 0.19 to 0.14. Given an endowment component of cognitive abilities, this finding is expected. Whilst grandparental schooling on the father's side is not significantly correlated with schooling of the child (third column), the mother's schooling (fourth column) is: the estimate is 0.17 and statistically significant. Although some of the father-child schooling covariation is absorbed by the inclusion of the mother's schooling, both estimates are substantial in size and significant at the one per cent level. Thus the OLS estimates show substantial intergenerational father-child schooling correlation, even when we include both parents' schooling attainment and a proxy for the father's cognitive abilities. Over and above the AFQT score, grandparental schooling and assortative mating, the father-child correlation of years of schooling is 0.12 and precisely estimated.

[16]Other papers on intergenerational transmission of schooling rely on intermediate schooling outcomes such as grade repetition (Oreopoulos *et al.*, 2006; Maurin and McNally, 2008; Carneiro *et al.*, 2013) or post-compulsory school attendance (Chevalier *et al.*, 2013).

[Table 4 about here]

Tables 5 and 6 show the association of the father's draft lottery outcome with his own schooling and the schooling of his child, respectively. The effect of the father's draft status on his own schooling is strong; being drafted implies significantly less schooling (table 5). The direct effect of the father's draft status on the schooling of his child is also negative, yet smaller and *imprecisely* estimated (table 6). When we control for the father's birth month and the child's birth year, the direct effect on child schooling is -0.09, with a standard error of 0.21. Adding the father's AFQT score and BMI reduces the effect to -0.04, and adding controls for grandparental schooling and mother's schooling reduces the effect even further. The intergenerational effect is never significant.

[Table 5 about here]

[Table 6 about here]

Given the negative direct effect of the father's draft status on both his own schooling and his child's schooling, the implied intergenerational transmission of schooling from 2SLS regression becomes positive (table 7). The estimate is between 0.01 and 0.09, depending on the set of controls included in the regression. While the point estimates in Table 7 suggest a positive father-child intergenerational transmission of schooling, the coefficients are imprecisely estimated, and we cannot draw any conclusions based on these results.

[Table 7 about here]

Two factors are likely to influence the estimation of schooling transmission in our sample of draft-eligible fathers and their children: the restriction on the length of the follow-up period, and sample size. De Haan and Plug (2011) find that OLS estimates of intergenerational schooling transmission are slightly downward biased by censoring of the follow-up period in the Wisconsin Longitudinal Study. Using an alternative, reform-based, source of variation in Danish fathers' schooling choices, we explore the sensitivity of instrumental variables estimates to both censoring and sample size in the next section.

V. Results from a reform-based instrument affecting earlier birth cohorts

In this section we use a Danish 1958 school reform to show how much the estimate of intergenerational transmission of schooling is affected by data censoring and sample size.

The 1958 school reform affected the cost of accessing post-compulsory education in rural areas. This reform has previously been used for studying health returns to schooling (Arendt, 2005, 2008). Background to the reform was compulsory schooling lasting seven years, with market towns (an administrative definition for towns of medium and large size) offering 8thand 9th grade post-compulsory schooling. The 1958 reform required rural areas to also offer 8th and 9th grade schooling. Thus the cost of attending post-compulsory schooling was reduced in rural areas for later cohorts. The reform particularly affected all individuals (living in rural areas) who were either enrolled in 7th or a lower grade in the 1957/1958 school year.

As we have individual-level data on month and year of birth for the population of the relevant birth cohorts, we can use the rules for school-starting age in effect at the time to calculate the grade level for all men around the time of the reform. We include all men born between 1940 and 1948.[17]

We use coordinates of parish of birth and a market town indicator to calculate individual distances from parish of birth to nearest market town prior to 1958.[18] The shortest distance—calculated as the crow flies—from parish of birth to nearest market town serves as a proxy for the pre-reform distance to the nearest institution providing post-compulsory schooling.

This instrument is similar to instruments using college proximity to study returns to college education, such as Card (1995) and Kane and Rouse (1995), in that we assume that individuals

[17]Except for those born between April 1, 1943, and March 31, 1944, who could be enrolled in either 7th or 8th grade in the 1957/1958 school year. A child was required to enter first grade if he or she had turned seven when the school year started but was eligible to start if he was at least six.

[18]Parish coordinates are given by the main church within each parish, while market town coordinates are given by the town center. We assume children stay resident in their parish of birth until reaching 7th grade.

respond to the costs of schooling and that the cost of schooling increases with distance to the institution providing education. Thus the 1958 schooling reform provides a supply-side instrument for schooling attainment by changing the cost of attending post-compulsory schooling. Our use of a continuous instrument (distance in kilometers) implies varying treatment intensities, as those living farther from a post-compulsory education institution before the reform experienced the largest cost reduction.

Figure 4 shows a map of the distance from each parish of birth to the nearest market town. At the time, Denmark consisted of 2,116 parishes and the figure clearly shows that the distance instrument provides a great deal of variation across the country.

[Figure 4 about here]

Whilst the draft lottery instrument affected men born in 1955 (section II), those in the relevant sample for the proximity instrument were born between 1940 and 1948 and are thus considerably older than the 1955 cohort by the end of our sample period. Using the proximity instrument for the father's schooling, we observe the child's schooling outcomes when the father is between 63 and 72 years old, compared to age 56 for the draft lottery instrument. Thus, even if the youngest father in the sample had his child at age 35, we observe this child's schooling outcome at age 28. Therefore, the estimate of intergenerational transmission of schooling based on the 1958 reform instrument is much less likely to suffer from selection on the father's fertility timing and censoring of the child's schooling outcome. In addition, because the 1958 reform spanned the country, the sample size is large and estimates are likely to be more precise.

Table 8 presents the results. As in the previous regressions, the sample includes children who were at least 25 years old in 2011. The table reports the intergenerational association of schooling between father and child as 0.20 (first column); this coefficient is precisely estimated even with the inclusion of a full set of indicators for the child's age and the father's age and municipality of birth (there were 1355 municipalities at the time). As we would expect, the first-stage results (column three) show that the distance to the nearest market town is negatively correlated with the father's schooling attainment, due to the higher attendance cost. Living one kilometer farther from nearest market town reduced the father's length of schooling by approximately one week on average. As the father's distance to the nearest market town also affects his child's schooling outcome negatively (column two), our causal estimate of the father-child intergenerational transmission of schooling becomes positive. Column 4 reports the 2SLS results: a precisely estimated effect of 0.16, implying that an increase of one year in the father's length of schooling causes an increase in his child's length of schooling of almost two months. Given partial compliance, this estimate is a LATE for the offspring of parents who stayed in school longer due to a reduction in the cost of attending post-compulsory schooling.

[Table 8 about here]

Although no previously reported results compare directly to those we present in Table 8, results from Norway (Black *et al.*, 2005) and Sweden (Holmlund *et al.*, 2011) provide the closest comparisons because of the similar educational systems and institutional changes used for identification. Although these two studies exploit an increase in minimum grade completion, i.e., a mandated instrument, as opposed to the incentive-driven supply-side instrument that we exploit here, both types of instrument primarily affect individuals in the bottom part of the educational distribution, and have a similar LATE.

Table 9 shows the estimates of the father-child intergenerational transmission of schooling found in Norway and Sweden (columns one and two), together with our estimates for Denmark (column three). The table shows that the OLS estimation of the child's years of schooling on the father's years of schooling is stable at around 0.2 and precisely estimated across countries and cohorts.

[Table 9 about here]

Despite the similarity in institutional settings, excluded instruments and OLS estimates, the 2SLS estimates show significant heterogeneity across the three studies on data from

Nordic countries listed in the table. This heterogeneity in the intergenerational transmission of schooling could potentially reveal true differences across the Nordic countries. However, a closer look at key characteristics of the estimation samples suggests that this difference is driven by data censoring. Black *et al.* (2005) find no effect in a small sample of fathers who are still rather young (aged 42-53) in the year of observation. Holmlund *et al.* (2011) have many more observations for a longer period and estimate an effect of 0.09 (significant at the 5 per cent level) in a sample of fathers aged 51-63. Using even more years of data, we observe schooling outcomes until the fathers are 63-71 years old, and our sample includes *all* men who became fathers no later than age 38 and many who became fathers at later ages. Our estimate of the father-child intergenerational transmission of schooling is 0.16 and very precisely estimated.

The results in Table 9 suggest that limiting the sample period (and the sample size) could substantially affect the estimate of intergenerational transmission of schooling. This finding gives rise to a reassessment of our estimate of the father-son schooling transmission based on the draft lottery amonst draft-eligible men born in 1955 – a small sample that allows us to observe schooling outcomes only until the father is 56 years old.

To illustrate the consequences of censoring and sample size within our Danish data, Table 10 shows four different estimates of the transmission of schooling from father to offspring with proximity to the nearest post-compulsory educational institution as the excluded instrument. The four estimates rely on different selections of the available sample (all Danish men born between 1940 and 1948).

[Table 10 about here]

For the estimation in column one, to mimic the data available on the draft-eligible men born in 1955, we select a 10 per cent subsample and use as the outcome the child's schooling attainment when the father is 56 years old. Using this small and censored data set, we obtain an imprecisely estimated coefficient of 0.09, which is of the same order of magnitude as the estimate from the draft lottery experiment (table 7). In column two, we use the same 10 per cent subsample but impose no restrictions on the father's age. Although the point estimate increases, the standard errors remain large, suggesting that observing schooling outcomes for 10-15 more years in itself does not change the results.[19] Column three re-imposes the age censoring but uses all available observations. As expected, the estimate is similar in magnitude to that in column one, but the standard errors are much smaller. Yet the effect is still imprecisely estimated, even though we use almost 300,000 observations. Column four presents the estimate obtained from the full sample with no restrictions on the sample period: a precisely estimated 0.16. The contrast between columns three and four differs from the contrasts found in De Haan and Plug (2011) of only small negative OLS bias due to censoring in the Wisconsin Longitudinal Study. Our instrumental variables estimates on Danish administrative data are more sensitive to censoring.

The estimates in Table 10 suggest that both sample size and span of cohorts are important factors for the estimation of intergenerational transmission of schooling. Both of these issues are relevant in the draft lottery experiment we present in this paper, and also affect Norwegian and Swedish estimates. Censoring of schooling observations for the second generation in our draft lottery sample is similar to that in Norwegian and Swedish studies which exploit first generation variation for comparable cohorts to our draftees. We have shown that the draft lottery outcome substantially affects schooling decisions but do not find an effect carrying over to the next generation. In common with Norwegian and Swedish findings, first stages are strong for first generations, but second stage effects for second generation transmission are weak to non-existent.

[19] The results in columns one and two are based on one 10 per cent subsample. To check if these estimates were substantially influenced by the representativeness of the subsample, we performed the estimations on 100 different subsamples; the mean point estimates were 0.04 and 0.19 corresponding to columns one and two, respectively.

VI. Robustness check: Lifetime earnings

The results in the previous section suggest that the draft lottery could serve as an instrument for father's schooling for estimating the father-child intergenerational transmission of schooling but that the data at hand is insufficient with respect to the sample size and follow-up period. Therefore, in this section we estimate draft lottery effects and implied father schooling effects on the father's own earnings, an outcome that (a) is unaffected by the restricted follow-up period and (b) allows us to use a larger sample size. Angrist and Chen (2011) analyse long-run earnings effects of draft lottery assignment to military service in the Vietnam-era United States and find zero effects at around age 50, in contrast to negative effects at younger ages (Angrist, 1990). These findings are reconciled by the G.I. Bill which increases schooling for veterans. In our Danish draft lottery sample for a similar (1955) cohort and earnings observations spanning these two Angrist studies, there are no similar compensatory schooling programs for Danish veterans, and the draft is unambiguously disruptive to schooling, as shown in Section 3. This reduction in schooling allows us to calculate the implied financial returns to schooling via the draft and consequent schooling disruption.

As the analysis in this section concerns only one generation (men born in 1955), we simplify notation only in this section. Let w_i denote log earnings of draft-eligible man i in year t and let S denote years of completed schooling. We may then write

$$w_{it} = \beta_0 + \beta_1 S_i + f(A_i, B_i) + \eta_t + \varepsilon_{it} \tag{3}$$

where $f(A,B)$ represents second order polynomials in AFQT (A) and BMI (B). Because all men in the sample were born in 1955, including the year in the regression is equivalent to including the age. Therefore, the specification in (3) is equivalent to a Mincer formulation of the returns to schooling. We include binary indicators for all years and thus impose no restrictions on the functional form of the experience profile.

The sample used in the earnings regressions consists of the 2,713 individuals for whom we observe both a lottery number and a schooling outcome. We gather earnings information from the entire sample period of 32 years (1980-2011). From these we exclude missing or zero earnings in a given year and public-sector employment.[20] To deal with outliers, we drop records if earnings belong to the top or bottom one per cent of the current-year earnings distribution. The final sample thus comprises 54,041 person-year observations.

Table 11 presents the OLS estimates, together with the reduced-form, first-stage, and second-stage estimates from the 2SLS regressions. All four estimation equations are represented by two specifications: one that controls only for year and birth month and one that also includes second-order polynomials in standardized AFQT scores and BMI. To account for correlation across multiple observations of each individual, we cluster standard errors at the individual level. The estimated coefficients in the reduced form, first stage, and second stage change only marginally between the two specifications. As the data covers 32 years of wage earnings from ages 25 to 56, they span (almost) the entire work career. Thus we use the term lifetime earnings in our interpretation of the results.

[Table 11 about here]

The OLS estimates show that without controlling for the AFQT scores, an additional year of completed schooling is associated with a lifetime wage premium of 2.4 per cent. When we include the AFQT scores, the correlation between years of schooling and wage earnings drops to 1.5 per cent. An increase of one standard deviation in the AFQT score is associated with an earnings increase of 5.6 per cent, whereas the correlation between earnings and BMI depends on the BMI level—for values below (above) 24, the correlation is positive (negative). The second stage column of Table 11 shows—for each of the two specifications—the Angrist and Pischke F-statistic on the excluded instrument. As these F-statistics are around 40, they

[20]To avoid earnings from the military, which we are unable to distinguish from other public sector employers, we consider only private sector wages . In the data to which we have access, we do not have sectoral affiliation after 2003. However, rather than excluding all observations after 2003, we assign the observed 2003 value to all subsequent years (2004-2011), effectively assuming no switch between private and public sectors after 2003.

verify that the instrument is statistically strong. Being drafted reduces lifetime earnings by 4.2 per cent, and, given a first-stage coefficient on the draft indicator of -0.87, the implied returns to schooling become 4.9 per cent.

VII. Conclusion

Parental schooling explains a large share of offspring schooling, and the literature has put forward several different approaches to identifying the extent to which the intergenerational schooling association is causal. The most popular approach uses Instrumental Variables (IV), which provide exogenous variation in parental schooling. All IV approaches have a Local Average Treatment Effect (LATE) interpretation specific to the context and source of exogenous variation, and for intergenerational schooling these have always been local to a particular part of the schooling distribution.

We bring a new instrument to the study of intergenerational transmission, one that affects the middle of the schooling distribution. We study men subject to a peacetime military draft lottery in Denmark, a lottery that provides exogenous schooling variation, to identify the effect on offspring schooling. After we control for father AFQT scores and grandparent schooling, the father-offspring schooling correlation is 12 per cent. We find that father random assignment to nine months of military service reduces his own schooling by nine months, implying a return to schooling of 4.9 per cent and a reduction in his lifetime earnings by 4.2 per cent We find no significant effect on his children's schooling. However, reform-based results for earlier cohorts suggest that our small draft lottery sample and the short follow-up period from the 1973 lottery could lead to insignificant effects.

The LATE interpretation of our estimates is for fathers whose schooling is affected by draft lottery assignment to the military. Rather than using a direct educational intervention, we observe the random assignment to military training and experience for young men who could otherwise have allocated their time freely between the classroom or the civilian labor market. This caveat notwithstanding, we find strong within-generation negative schooling and lifetime earnings effects, but we cannot say that father schooling has any causal effect on the next generation.

References

Albæk, K., Leth-Petersen, S., Le Maire, D. and Tranæs, T. (2013) Does peacetime military service affect crime?, IZA Discussion Paper 7528, Bonn.

Angrist, J. D. (1990) Lifetime earnings and the vietnam era draft lottery: Evidence from social security administrative records, *American Economic Review*, **80**, 313–36.

Angrist, J. D. and Chen, S. H. (2011) Schooling and the vietnam-era gi bill: Evidence from the draft lottery, *American Economic Journal: Applied Economics*, **3**, 96–118.

Arendt, J. N. (2005) Does education cause better health? a panel data analysis using school reforms for identification, *Economics of Education Review*, **24**, 149 – 160.

Arendt, J. N. (2008) In sickness and in health–till education do us part: Education effects on hospitalization, *Economics of Education Review*, **27**, 161–172.

Behrman, J. R. and Rosenzweig, M. R. (2002) Does increasing women's schooling raise the schooling of the next generation?, *The American Economic Review*, **92**, pp. 323–334.

Belzil, C. and Hansen, J. (2010) The distinction between dictatorial and incentive policy interventions and its implication for iv estimation, IZA Discussion Papers 4835, Institute for the Study of Labor (IZA).

Bingley, P., Lundborg, P. and Vincent Lyk-Jensen, S. (2014) Opportunity cost and the incidence of a draft lottery, IZA Discussion Papers 8057, Institute for the Study of Labor (IZA).

Bjerg, H. C. (1991) *Til fædrelandets forsvar - værnepligten i Danmark gennem tiderne*, Værnepligtsstyrelsen.

Björklund, A., Lindahl, M. and Plug, E. (2006) The origins of intergenerational associations: Lessons from swedish adoption data, *The Quarterly Journal of Economics*, **121**, 999–1028.

Björklund, A. and Salvanes, K. G. (2011) Education and family background: Mechanisms and policies, in *Handbook of the Economics of Education* (Eds.) E. Hanushek, S. Machin and L. Woessman, North Holland, vol. 3.

Black, S. E. and Devereux, P. J. (2011) Recent developments in intergenerational mobility, Elsevier, vol. 4, Part B of *Handbook of Labor Economics*, pp. 1487 – 1541.

Black, S. E., Devereux, P. J. and Salvanes, K. G. (2005) Why the apple doesn't fall far: Understanding intergenerational transmission of human capital, *American Economic Review*, **95**, 437–449.

Card, D. (1995) Using geographic variation in college proximity to estimate the return to schooling, in *Aspects of labour market behaviour: essays in honour of John Vanderkamp* (Eds.) L. N. Christofides, J. Vanderkamp, E. K. Grant and R. Swidinsky, University of Toronto Press.

Card, D. (2001) Estimating the return to schooling: Progress on some persistent econometric problems, *Econometrica*, **69**, pp. 1127–1160.

Carneiro, P., Meghir, C. and Parey, M. (2013) Maternal education, home environments, and the development of children and adolescents, *Journal of the European Economic Association*, **11**, 123–160.

Chevalier, A., Harmon, C., O' Sullivan, V. and Walker, I. (2013) The impact of parental income and education on the schooling of their children, *IZA Journal of Labor Economics*, **2**, 8.

Chevalier, A., Harmon, C., Walker, I. and Zhu, Y. (2004) Does education raise productivity, or just reflect it?, *The Economic Journal*, **114**, pp. F499–F517.

Cunha, F. and Heckman, J. (2007) The technology of skill formation, *American Economic Review*, **97**, 31–47.

Cunha, F., Heckman, J. J., Lochner, L. and Masterov, D. V. (2006) *Interpreting the Evidence of Life Cycle Skill Formation*, Elsevier, vol. 1 of *Handbook of the Economics of Education*, chap. 10, pp. 577–633.

Currie, J. and Moretti, E. (2003) Mother's education and the intergenerational transmission of human capital: Evidence from college openings, *The Quarterly Journal of Economics*, **118**, pp. 1495–1532.

Danish Ministry of Education (2010) Key figures in education 2009, Statistical Publication 2010-02, Danish Ministry of Education.

De Haan, M. and Plug, E. (2011) Estimating intergenerational schooling mobility on censored samples: consequences and remedies, *Journal of Applied Econometrics*, **26**, 151–166.

Duncan, G. J., Morris, P. A. and Rodrigues, C. (2011) Does money really matter? estimating impacts of family income on young children's achievement with data from random-assignment experiments., *Developmental psychology*, **47**, 1263.

Galiani, S., Rossi, M. A. and Schargrodsky, E. (2011) Conscription and crime: Evidence from the argentine draft lottery, *American Economic Journal: Applied Economics*, **3**, 119–36.

Haveman, R. and Wolfe, B. (1995) The determinants of children's attainments: A review of methods and findings, *Journal of Economic Literature*, **33**, pp. 1829–1878.

Hearst, N., Newman, T. B. and Hulley, S. B. (1986) Delayed effects of the military draft on mortality, *New England Journal of Medicine*, **314**, 620–624, pMID: 3945247.

Holmlund, H., Lindahl, M. and Plug, E. (2011) The causal effect of parents' schooling on children's schooling: A comparison of estimation methods, *Journal of Economic Literature*, **49**, 615–51.

Imbens, G. W. and Angrist, J. D. (1994) Identification and estimation of local average treatment effects, *Econometrica*, **62**, pp. 467–475.

Kane, T. J. and Rouse, C. E. (1995) Labor-market returns to two- and four-year college, *The American Economic Review*, **85**, pp. 600–614.

Kousgaard, E. (2003) 50 år med intelligensprøven bpp, Tech. rep., Forsvarsakademiet (Institut for Militærpsykologi).

Lyng, J., Faurby, I., Helms, A., Krogen, P. B., Ramlau, V. B. A., Skov, N. K. and Sørensen, E. H. (2000) *Ved forenede kræfter*, Forsvarskommandoen.

Maurin, E. and McNally, S. (2008) Vive la revolution! long-term educational returns of 1968 to the angry students, *Journal of Labor Economics*, **26**, pp. 1–33.

Mortensen, E. L., Reinisch, J. M. and Teasdale, T. W. (1989) Intelligence as measured by the wais and a military draft board group test, *Scandinavian Journal of Psychology*, **30**, 315–318.

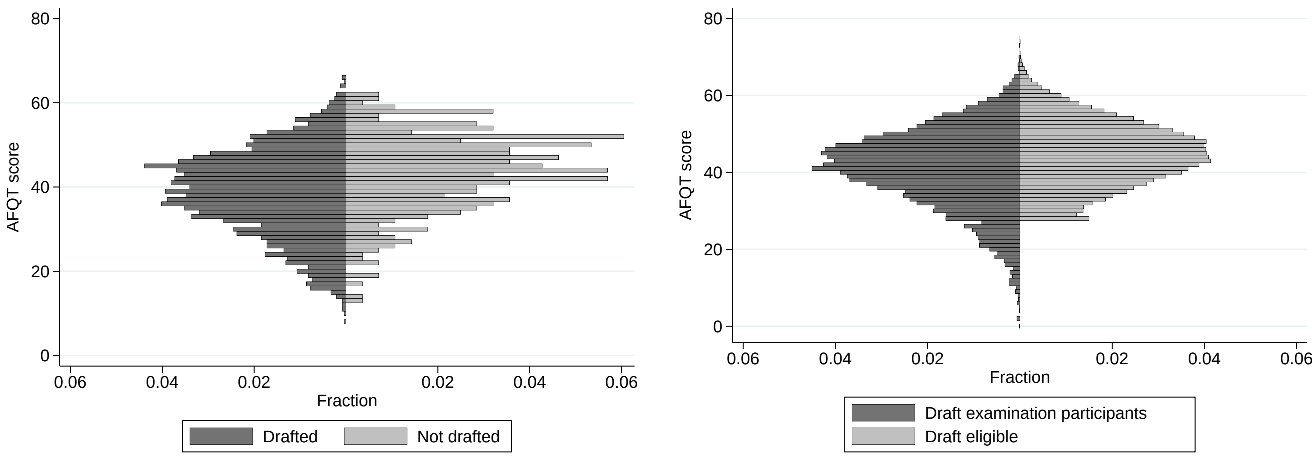
Oreopoulos, P., Page, M. E. and Stevens, A. H. (2006) The intergenerational effects of compulsory schooling, *Journal of Labor Economics*, **24**, pp. 729–760.

Sandewall, O., Cesarini, D. and Johannesson, M. (2014) The co-twin methodology and returns to schooling—testing a critical assumption, *Labour Economics*, **26**, 1 – 10.

Statistics Denmark, . (1975-1980) Statistical yearbooks.

Figures

Figure 1.—Distributions of AFQT scores in our sample and in the population

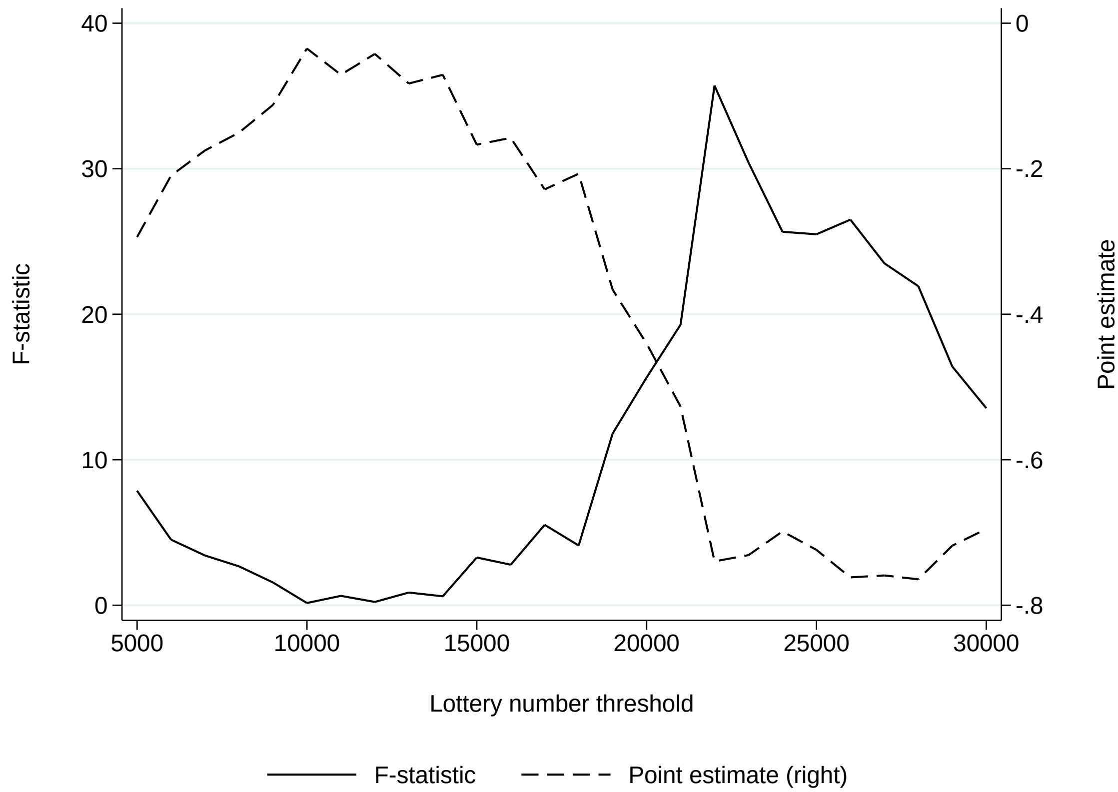


(a) Our sample of 1955 cohort draft-eligible

(b) Population of draft exam participants born in 1992 (left) and the draft eligible born 1979-81 (right)

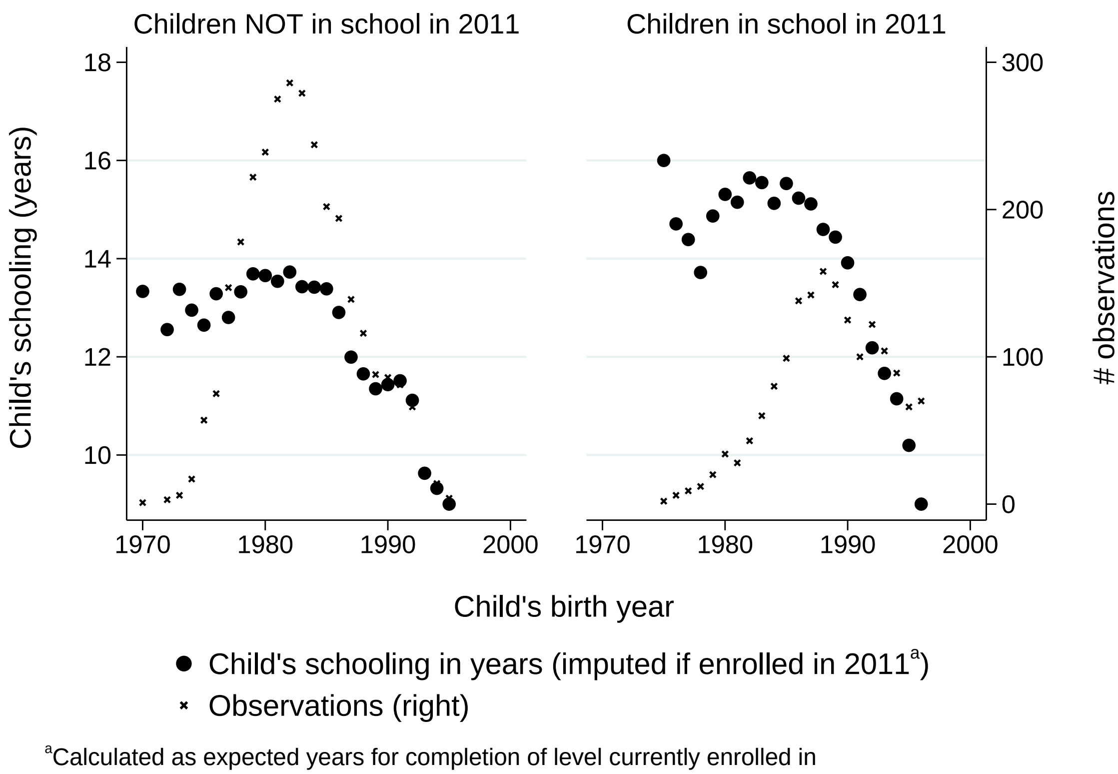
NOTE.—The figure presents the distribution of AFQT scores from different populations. The left pane shows score distributions by draft status among the draft-eligible men in our sample: dark bars for those who were drafted. The right pane shows scores for the full population of more recent birth cohorts. The dark bars show scores for everyone born in 1992 attending the draft examinations. The light bars show scores for everyone born 1979-81 attending the draft examinations, who met the draft eligibility cutoff of 27.

Figure 2.—Draft assignment robustness check



NOTE.—The figure shows point estimates and F-statistics from separate OLS regressions of schooling (years) on a binary draft indicator for each hypothetical lottery threshold value. The lottery numbers run from 1 to 36,000. For each potential threshold value, we assign hypothetical draft status according to that threshold and run the regression of schooling on draft status. The graph depicts results for threshold values in steps of 1,000 and excludes estimates close to the minimum (maximum) lottery number, as sample size in the drafted (not drafted) group approaches zero.

Figure 3.—Schooling for children in our draft-lottery sample, by birth cohort.



NOTE.—The figure shows years of schooling for children of the draft-eligible fathers in our sample. The left graph shows years of completed schooling for those children who were not enrolled in education in Denmark in 2011. The right graph shows imputed years of schooling of those children who were enrolled in 2011, assuming they eventually complete the education in which they enrolled.

Figure 4.—1950 map of distance (km) to nearest market town by parish.

NOTE.—The figure shows the distance from each of the 2116 parishes in Denmark in 1950 to the nearest market town. Distance is measured as the crow flies from the parish church to the center of the nearest market town. Darker colors correspond to longer distances.

Tables

Table 1
Summary statistics.

	Sample of draft eligible	Catchment area	Population
Pre-lottery			
Father's schooling (years)	9.5	9.2	10.4
	(3.3)	(3.1)	(3.5)
Mother's schooling (years)	8.2	8.0	9.0
	(2.4)	(2.2)	(2.9)
Matched fathers (share)	0.81	0.77	0.69
	(0.40)	(0.42)	(0.46)
Matched mothers (share)	0.84	0.81	0.74
	(0.37)	(0.39)	(0.44)
Father's age at birth	32.1	32.0	31.3
	(6.3)	(6.4)	(6.2)
Mother's age at birth	28.4	28.3	28.0
	(6.0)	(6.0)	(5.8)
Post-lottery			
Had become father by age 41	0.79	0.78	0.72
	(0.41)	(0.42)	(0.45)
Schooling (years)	12.5	12.0	12.5
	(2.6)	(2.6)	(2.9)
Earnings at age 35 (2013 USD)	58,305	55,053	60,071
	(25,756)	(24,197)	(29,869)
Observations	2722	3623	22209

NOTE.—The table shows summary statistics (means and standard deviations) for our sample of draft-eligible men born between January and June, 1955, who underwent draft examination in the fifth conscription district of Denmark (first column); all men born in the same six months and living within the catchment area, which is a mostly rural area of Denmark (second column); and all men born in the same six months in the Danish population as a whole (third column). Our gross sample of 2,722 includes four men with missing AFQT scores, and five men with missing schooling information. All nine men are excluded from the estimation sample.

Table 2
Check for balanced assignment on pre-lottery characteristics.

	Draft status
Std. AFQT	0.00317
	(0.00692)
Std. AFQT^2	0.00110
	(0.00494)
BMI	0.0430*
	(0.0261)
BMI^2	-0.000840
	(0.000559)
Father's education (years)	-0.00443
	(0.00273)
Mother's education (years)	-0.00419
	(0.00339)
Birth month	✓
Observations	2718
F-test for joint exclusion (pval)	0.264

NOTE.—The table shows estimates from an OLS regression of a binary indicator for military draft status on pre-lottery characteristics. This sample of 2,718 men used for the randomization balance check excludes four men with missing AFQT scores, but includes five men with missing schooling records. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Table 3
Draft lottery effects on schooling.

	Schooling (years)	
Draft status	-0.740***	-0.727***
	(0.163)	(0.138)
Std. AFQT		1.130***
		(0.0494)
Std. AFQT^2		-0.0319
		(0.0393)
BMI		0.294*
		(0.159)
BMI^2		-0.00714**
		(0.00338)
Birth month	✓	✓
Observations	2713	2713

NOTE.—The table shows estimated coefficients from an OLS regression of completed years of schooling on a binary indicator of draft status and pre-lottery characteristics. This sample of 2,713 men excludes four men with missing AFQT scores and five men with missing schooling information. Standard errors in parentheses; * p < 0.1, ** p < 0.5, *** p < 0.01.

Table 4
Father-child intergenerational association of schooling.

	Child's schooling (years)			
Father's schooling (years)	0.189***	0.140***	0.143***	0.115***
	(0.0218)	(0.0234)	(0.0234)	(0.0235)
Child's gender (male)	-0.728***	-0.737***	-0.733***	-0.723***
	(0.0927)	(0.0926)	(0.0925)	(0.0910)
Father's std AFQT		0.349***	0.359***	0.321***
		(0.0637)	(0.0646)	(0.0644)
Father's std $AFQT^2$		0.0173	0.0204	0.0194
		(0.0480)	(0.0481)	(0.0475)
Father's BMI		0.480*	0.460*	0.415
		(0.258)	(0.257)	(0.257)
Father's BMI^2		-0.0102*	-0.00980*	-0.00885
		(0.00563)	(0.00561)	(0.00560)
Grandfather's schooling (years)			-0.0249	-0.0323
			(0.0271)	(0.0271)
Grandmother's schooling (years)			-0.00505	-0.0146
			(0.0359)	(0.0351)
Mother's schooling (years)				0.166***
				(0.0225)
Child's birth year	✓	✓	✓	✓
Father's birth month	✓	✓	✓	✓
Observations	2418	2418	2418	2418

NOTE.—The columns contain estimated coefficients from four different OLS regressions of child's years of schooling on father's years of schooling with different sets of controls. The estimation sample contains all children (of the draft-eligible 1955 fathers), who were at least 25 years old in 2011 and not enrolled in education in Denmark. Standard errors clustered by fathers in parentheses; * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Table 5
First-stage regressions. Effect of the father's draft status on his own schooling.

	Father's schooling (years)			
Father's draft status	-0.981***	-0.841***	-0.784***	-0.759***
	(0.233)	(0.201)	(0.199)	(0.195)
Child's gender (male)	-0.0497	-0.0461	-0.0647	-0.0529
	(0.0989)	(0.0925)	(0.0917)	(0.0902)
Father's std AFQT		0.923***	0.858***	0.797***
		(0.0764)	(0.0770)	(0.0760)
Father's std AFQT^2		0.0288	0.0124	0.0111
		(0.0654)	(0.0650)	(0.0644)
Father's BMI		0.121	0.217	0.166
		(0.277)	(0.283)	(0.275)
Father's BMI^2		-0.00310	-0.00508	-0.00400
		(0.00598)	(0.00613)	(0.00593)
Grandfather's schooling (years)			0.0728**	0.0634**
			(0.0310)	(0.0297)
Grandmother's schooling (years)			0.114***	0.101***
			(0.0367)	(0.0350)
Mother's schooling (years)				0.165***
				(0.0264)
Child's birth year	✓	✓	✓	✓
Father's birth month	✓	✓	✓	✓
Observations	2418	2418	2418	2418

NOTE.—The columns contain estimated coefficients from four different OLS regressions of the father's years of schooling on his own draft status with different sets of controls. The estimation sample contains all children (of the draft-eligible 1955 fathers), who were at least 25 years old in 2011 and not enrolled in education in Denmark. Standard errors clustered by fathers in parentheses; * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Table 6
Reduced-form regressions. Effect of father's draft status on child schooling.

	Child's schooling (years)			
Father's draft status	-0.0894	-0.0376	-0.0390	-0.0105
	(0.208)	(0.197)	(0.197)	(0.192)
Child's gender (male)	-0.739***	-0.744***	-0.744***	-0.730***
	(0.0946)	(0.0938)	(0.0936)	(0.0918)
Father's std AFQT		0.479***	0.482***	0.413***
		(0.0610)	(0.0625)	(0.0620)
Father's std AFQT^2		0.0211	0.0219	0.0204
		(0.0484)	(0.0486)	(0.0479)
Father's BMI		0.492*	0.488*	0.431
		(0.268)	(0.270)	(0.266)
Father's BMI^2		-0.0105*	-0.0105*	-0.00924
		(0.00586)	(0.00588)	(0.00580)
Grandfather's schooling (years)			-0.0142	-0.0248
			(0.0275)	(0.0273)
Grandmother's schooling (years)			0.0117	-0.00246
			(0.0362)	(0.0351)
Mother's schooling (years)				0.185***
				(0.0223)
Child's birth year	✓	✓	✓	✓
Father's birth month	✓	✓	✓	✓
Observations	2418	2418	2418	2418

NOTE.—The columns contain estimated coefficients from four different OLS regressions of child's years of schooling on father's draft status with different sets of controls. The estimation sample contains all children (of the draft-eligible 1955 fathers), who were at least 25 years old in 2011 and not enrolled in education in Denmark. Standard errors clustered by fathers in parentheses; * p < 0.1, ** p < 0.5, *** p < 0.01.

Table 7
Instrumental-variable regressions. Causal estimates of the father-child intergenerational transmission of schooling based on the draft lottery.

	Child's schooling (years)			
Father's schooling (years)	0.0911	0.0447	0.0498	0.0138
	(0.202)	(0.229)	(0.245)	(0.251)
Child's gender (male)	-0.734***	-0.742***	-0.740***	-0.730***
	(0.0926)	(0.0924)	(0.0926)	(0.0908)
Father's std AFQT		0.437*	0.439*	0.402*
		(0.232)	(0.230)	(0.221)
Father's std AFQT^2		0.0198	0.0213	0.0202
		(0.0485)	(0.0481)	(0.0477)
Father's BMI		0.487*	0.477*	0.429
		(0.263)	(0.266)	(0.264)
Father's BMI^2		-0.0104*	-0.0102*	-0.00919
		(0.00576)	(0.00583)	(0.00578)
Grandfather's schooling (years)			-0.0178	-0.0257
			(0.0320)	(0.0308)
Grandmother's schooling (years)			0.00605	-0.00386
			(0.0474)	(0.0449)
Mother's schooling (years)				0.183***
				(0.0475)
Child's birth year	✓	✓	✓	✓
Father's birth month	✓	✓	✓	✓
Observations	2418	2418	2418	2418
F-statistic (excluded instrument)	17.69	17.52	15.59	15.11

NOTE.—The columns contain estimated coefficients from four different 2SLS regressions of child's years of schooling on father's years of schooling, using father's draft status as the excluded instrument for father's schooling. The regressions differ by the set of controls. The estimation sample contains all children (of the draft-eligible 1955 fathers), who were at least 25 years old in 2011 and not enrolled in education in Denmark. Standard errors clustered by fathers in parentheses; * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Table 8
Estimates of the father-child intergenerational transmission of schooling based on the 1958 Danish school reform.

	OLS	Reduced form	First stage	2SLS
	Child's schooling (years)	Child's schooling (years)	Father's schooling (years)	Child's schooling (years)
Father's schooling (years)	0.203***			0.156***
	(0.00115)			(0.0498)
Distance to market town (km)		-0.00300**	-0.0209***	
		(0.00130)	(0.00176)	
Distance^2		0.0000276	0.000230***	
		(0.0000257)	(0.0000350)	
Controls (dummies)				
Father's age	✓	✓	✓	✓
Child's age	✓	✓	✓	✓
Municipality	✓	✓	✓	✓
Observations	380166	380166	380166	380166
F-stat (excl. instruments)				101.0

NOTE.—The table collects estimated coefficients for the father-child intergenerational transmission of schooling. The first column presents estimates from an OLS regression of child's schooling on father's schooling. The second column contains estimated coefficients from an OLS regression of child's schooling on distance (and distance squared) from the father's parish of birth to nearest market town (reduced form). The third column contains the results of an OLS regression of father's schooling on the distance to nearest market town (first stage). The fourth column contains the results from a 2SLS regression of child's schooling on father's schooling, using the distance and distance squared as the excluded instrument for father's schooling. All the regressions include dummy variables for child's age and father's age and municipality of birth. The estimation sample consists of all children, who were at least 25 in 2011 and whose fathers were born between 1940 and 1948 (except for 1 April 1943 – 31 March 1944). Standard errors in parentheses; * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Table 9
Father-child intergenerational transmission of schooling. Evidence from Norway, Sweden and Denmark.

	Norway Black *et al.* (2005)	Sweden Holmlund *et al.* (2011)	Denmark
OLS	0.217**	0.23***	0.203***
	(0.003)	(0.00)	(0.00115)
2SLS	0.030	0.09**	0.156***
	(0.132)	(0.04)	(0.0498)
Father's age	✓	✓	✓
Child's age	✓		✓
Child's gender		✓	
Municipality	✓		✓
Observations	46,783	365,461	380,166
F-stat	20.9	160	101.0
Father's age	42 – 53	51 – 63	63 – 71
Child's age	25 – 35	$\geq$ 23	$\geq$ 25

NOTE.—The table collects results from OLS and instrumental-variables (2SLS) regressions of child's years of schooling on father's years of schooling across Norway, Sweden and Denmark. Results from Norway and Sweden are reprints of Black *et al.* (2005, Table 3) and Holmlund *et al.* (2011, Table 3), respectively; the last column repeats the estimates from Table 8. In the Norwegian and Swedish studies a change in minimum requirements for grade-level completion serves as the excluded instrument for father's schooling. For Denmark distance from father's parish of birth to nearest market town provides the excluded instrument. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Table 10
Causal estimates of the father-child intergenerational transmission of schooling by sample size and length of follow-up period.

Father's age in year of observation	56	63 – 71	56	63 – 71
	Child's schooling (years)			
Father's schooling (years)	0.0893	0.175	0.0498	0.156***
	(0.172)	(0.157)	(0.0577)	(0.0498)
Controls (dummies)				
Father's age	✓	✓	✓	✓
Child's age	✓	✓	✓	✓
Municipality	✓	✓	✓	✓
Observations	28966	37954	289643	380166
F-stat (excl. instruments)	7.83	10.31	70.71	101.0
Subsample	10%	10%	100%	100%

NOTE.—The table presents estimated coefficients from four different instrumental-variable regressions (2SLS) of child's schooling on father's schooling, using the distance of the father's parish of birth to the nearest market town as the excluded instrument for father's schooling. The children are all at least 25 years old and the fathers are all born between 1940 and 1948. The regressions differ by their estimation sample: The first regression uses a ten per cent subsample and reports the child's schooling from the year in which the father turns 56. The second regression uses the same ten per cent subsample but imposes no restriction on father's age (i.e., child schooling observed in 2011). The third regression reimposes the age restriction on fathers but uses the full sample. The fourth regression relaxes both the age and sample size restrictions; it repeats the 2SLS result from Table 8. Standard errors in parentheses; * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$.

Table 11
Lifetime wage returns to draft status and schooling.

	OLS		Reduced form		First stage		Second stage	
	Log earnings		Log earnings		Schooling (years)		Log earnings	
Schooling (years)	0.0241***	0.0151***					0.0496**	0.0486**
	(0.00294)	(0.00321)					(0.0199)	(0.0233)
Draft status			-0.0498**	-0.0422**	-1.003***	-0.870***		
			(0.0212)	(0.0210)	(0.157)	(0.142)		
Std AFQT		0.0563***		0.0719***		1.054***		0.0206
		(0.00843)		(0.00783)		(0.0570)		(0.0279)
Std AFQT^2		-0.00600		-0.00602		-0.00248		-0.00590
		(0.00644)		(0.00651)		(0.0469)		(0.00655)
BMI		0.0383*		0.0424**		0.174		0.0339
		(0.0211)		(0.0210)		(0.181)		(0.0229)
BMI^2		-0.00081*		-0.00090**		-0.00435		-0.00069
		(0.00044)		(0.00044)		(0.00389)		(0.00049)
Year	✓	✓	✓	✓	✓	✓	✓	✓
Birth month	✓	✓	✓	✓	✓	✓	✓	✓
Observations	54041	54041	54041	54041	54041	54041	54041	54041
F-statistic (excluded instrument)							40.82	37.56

NOTE.—The table presents estimates of the draft lottery effects on schooling and log annual earnings together with the implied private financial returns to schooling, using the indicator of draft status as the excluded instrument for schooling. The data used in the regressions contains all private-sector wage-earnings observations of the 1955 draft-eligible men between 1980 and 2011, thereby covering the age range 25-56. Standard errors clustered by individuals in parentheses; * $p < 0.1$, ** $p < 0.5$, *** $p < 0.01$