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The Effect of Vietnam-Era Conscription and Genetic Potential for Educational Attainment on Schooling Outcomes

Lauren L. Schmitz[1] and **Dalton Conley**[2]

[1]Survey Research Center, Institute for Social Research, University of Michigan, 426 Thompson St., Ann Arbor, MI 48104, USA

[2]Department of Sociology, Princeton University. 153 Wallace Hall, Princeton, NJ 08544, USA

Abstract

This study examines whether draft lottery estimates of the causal effects of Vietnam-era military service on schooling vary by an individual's genetic propensity toward educational attainment. To capture the complex genetic architecture that underlies the bio-developmental pathways, behavioral traits and evoked environments associated with educational attainment, we construct polygenic scores (PGS) for respondents in the Health and Retirement Study (HRS) that aggregate thousands of individual loci across the human genome and weight them by effect sizes derived from a recent genome-wide association study (GWAS) of years of education. Our findings suggest veterans with below average PGSs for educational attainment may have completed fewer years of schooling than comparable non-veterans. On the other hand, we do not find any difference in the educational attainment of veterans and non-veterans with above average PGSs. Results indicate that public policies and exogenous environments may induce heterogeneous treatment effects by genetic disposition.

Keywords

Human capital; educational economics; gene-environment interactions; polygenic score; military service; Health and Retirement Study

1. Introduction

The Vietnam-era draft was a pivotal moment in the lives of thousands of young men who were called to service. Whether they were deployed to Vietnam or served outside the theater of war, past work has suggested the existence of the draft had a profound impact on the

Corresponding author: Lauren L. Schmitz, Survey Research Center, Institute for Social Research, University of Michigan, 426 Thompson St., Ann Arbor, MI 48104, USA. lschmitz@umich.edu.

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[7]We follow RAND and categorize individuals who report receiving a college degree but "less than bachelors" as receiving an "associate degree" (RAND, 2016).

subsequent lives of a generation of men. As Vietnam-era veterans enter their sixties, attention has turned to identifying the lingering effects of military service on the well-being of those who were directly affected. While several studies have used the draft lotteries to evaluate the causal impact of military service on a variety of physical and socioeconomic outcomes, this study is the first to examine whether the educational attainment of conscripts was moderated by genetic influences.

To capture the complex genetic architecture that underlies the bio-developmental pathways, behavioral traits and evoked environments associated with educational attainment, we incorporate the latest approaches from population genetics into a quasi-natural experimental framework to identify the independent and joint effects of compulsory military service and genotype on educational outcomes. Specifically, we interact a polygenic score (PGS) for educational attainment with instrumented veteran status in an instrumental variables (IV) two- stage least squares (2SLS) framework using data from the Health and Retirement Study (HRS). Our genotype measure uses findings from a recent large-scale genome-wide association study (GWAS) of educational attainment to construct a PGS that predicts approximately 14 percent of the variation in years of schooling in our analytic sample (Okbay et al., 2016).

Until recently, research in economics on the genetic and environmental dynamics of human skill formation has been hindered by the availability of a measure that could capture the multifaceted and complex genetic architecture of educational attainment. This study provides an illustration of how economists might use PGSs to assess heterogeneous treatment effects in observational data. Prediction of differential effects across individuals with varying genetic endowments provides a window into understanding how exposure to environmental risks can have lasting effects on the realization of underlying genetic potential.

Importantly, in this study we use an exogenous source of environmental variation in military service—the Vietnam-era draft lotteries—to test for heterogeneous treatment effects between our PGS for educational attainment and military service. To date, the vast majority of gene-by-environment (G × E) studies in the social, behavioral, and epidemiological sciences are often unable to support causal inference because researchers use endogenous measures of environmental risk or fail to adequately address population stratification—i.e. the nonrandom distribution of genes across environments due to ancestral linkages—confounding estimates (Conley, 2009; Fletcher & Conley, 2013; Schmitz & Conley, 2017). In particular, since selection into the military is far from random, and likely to be correlated with socioeconomic background and underlying genotype, it would be impossible to identify G × E effects in a model that uses self-reports of veteran status.

Our findings suggest conscription may have reduced the educational attainment of veterans with below average PGSs. Specifically, the overall direction of effects on the coefficient estimates, though not statistically significant, indicate that draft eligible men with lower PGSs completed fewer years of schooling and were less likely to obtain a postsecondary degree. However, the size of the available data on genotyped veterans in the HRS prevents us from drawing stronger conclusions.

The remainder of the study is outlined as follows. After reviewing the relevant literature on the genetics of educational achievement and the socioeconomic consequences of military service in the “Background” section, we present an in-depth explanation of the data, lottery estimates of draft eligibility, and construction of the PGS in the “Data and Descriptive Statistics” section. The “Empirical Methods and Results” section presents reduced form estimates, outlines our 2SLS approach, and reports corresponding 2SLS results. The “Discussion” section concludes with implications for the future of G × E research in economics.

2. Background

2.1 The Genetics of Educational Attainment

Twin and family studies have linked a range of behavioral, social, and economic outcomes to genetic differences (Beauchamp et al., 2011; Benjamin et al., 2012; Cesarini, Dawes, Johannesson, Lichtenstein, & Wallace, 2009). Recent studies suggest the heritability of educational attainment—or the proportion of observed differences in education that can be attributed to genetic differences in a particular population—ranges from around 25 to 40 percent (Branigan, McCallum, & Freese, 2013; Lucchini, Della Bella, & Pisati, 2013; Nielsen & Roos, 2011). The variation in realized phenotypic or observed outcomes between populations that can be ascribed to genetic differences is in large part regulated by environmental influences or G × E interactions. However, without a direct measure of genotype, extant research cannot assess the degree to which social and genetic inheritances mediate or moderate each other’s influence (Conley et al., 2015).

Finding adequate measures for endowment has long been of interest to economists who recognized that innate, unmeasured biological variation could be biasing models that seek to isolate the causal effect of schooling on human capital development (for a review see Card, 1999; Card, 1995). Pinpointing an exact measure has proven difficult because educational attainment is a complex behavioral trait that is moderately correlated with a host of heritable characteristics, including cognitive function (Deary, Strand, Smith, & Fernandes, 2007) and noncognitive traits like personality (Belskey et al., 2016; Grönqvist, Öckert, & Vlachos, 2016; Heckman & Rubinstein, 2001; Okbay et al., 2016). Discussions of “ability bias” in the estimated return to education literature have focused primarily on cognitive ability and have used measures of IQ or scores on other cognitive tests like the Armed Forces Qualifying Test (AFQT) to proxy missing ability (Blackburn & Neumark, 1993; Griliches & Mason, 1972). On the other hand, finding a single measure for non-cognitive skills has proven difficult if not impossible given the diversity of traits like personality or self-esteem that predict educational attainment. In addition, both cognitive and non-cognitive measures are endogenous to the social environment, which calls into question the extent to which they can be used as unbiased measures of innate ability at any point across the life course.

To circumvent endogeneity issues, studies have used twin or sibling difference models to control for unmeasured family environment and genetic (in the case of monozygotic twins) heterogeneity (for an overview see Griliches, 1979). However, these models do not provide a direct measure of genotype to deploy if we are interested in estimating G × E effects. Similarly, adoptee family studies on socioeconomic success give us a sense of the latent contribution to population variance in a trait of pre-birth (i.e. genetic) and post-birth (i.e. environmental) factors. But this approach as well cannot estimate specific genetic and environmental effects since both parameters are latent. Further, adoption studies cannot rule out the possibility that presumed genetic differences between biological and adopted children are actually proxying unmeasured environmental influences like the birth mother's uterine environment, which has been shown to exert a strong influence on life chances (Almond, Chay, & Lee, 2005; Almond & Mazumder, 2011; Black, Devereux, & Salvanes, 2007; Conley & Bennett, 2000).

Recently, large-scale genome-wide association studies (GWASs) of educational attainment have identified 74 genome-wide significant loci ($p<5 \times 10^{-8}$), or single nucleotide polymorphisms (SNPs), associated with number of years of schooling completed (Okbay et al., 2016; Rietveld et al., 2013). The GWAS findings implicated SNPs in regions that regulate brain- specific gene expression in neural tissue or genotype-tissue expression in the central nervous system (Okbay et al. 2016, p. 540). In addition, results implicate candidate genes and biological pathways that are active during distinct stages of neuronal prenatal development and are correlated with childhood IQ—e.g. forebrain development, abnormal cerebral cortex morphology, sprouting of dendrites and dendritic spine organization, neuronal signaling, and synaptic plasticity throughout the lifespan *(ibid).* Taken together, the linear polygenic score, or joint predictive power of all SNPs from the most recent GWAS, explains approximately seven percent of the variation in years of schooling across the discovery and replication cohorts in the GWAS meta-analysis (Okbay et al. 2016).

These studies have opened up new opportunities for researchers who seek to measure latent biological traits that may be biasing traditional models of socioeconomic attainment. In particular, using a PGS for educational attainment improves upon prior estimates of genetic endowment that use IQ or other cognitive measures because unlike IQ, which is endogenous to the environment and the prenatal environment in particular, genotype is fixed at conception and determined prior to any environmental exposure. In addition, the PGS potentially captures any non-cognitive influences that contribute to educational attainment.[3] This allows researchers to control for potential genetic confounds and strengthens their ability to make causal inferences, regardless of whether or not the exact mechanistic biological pathways are known.

It is important that a PGS for educational attainment not be misinterpreted as "the gene for ability." If anything, the small amount of variation in educational attainment predicted by the PGS emphasizes the importance of environmental influences. For example, a meta-analysis of twin studies found that genetic influences on educational attainment diverge considerably by country and birth cohort (Branigan et al., 2013) and recent studies using HRS data found considerable differences in the returns to genetic ability by childhood socioeconomic status (Papageorge & Thom, 2016) and birth cohort (Conley et al. 2016). That being said, although the PGS may work through many downstream environmental channels, research has not

[3]Results from the GWAS meta-analysis show that on average SNPs associated with educational attainment are also associated with other non-cognitive factors that may affect school performance, including increased intracranial volume, increased risk of bipolar disorder, and lower neuroticism (Okbay et al., 2016).

found any evidence that it is spuriously associated with environmental background. For example, the environment may mediate the relationship between the PGS and educational attainment through evocative gene-environment correlations (rGE), whereby children with high educational PGSs demand (and thus receive) more interactive attention from their parents and teachers, or receive more positive feedback because they have better interpersonal skills, self-control, or are healthier and go to school more days of the year (Belsky et al., 2016). However, these situations are still capturing pure genetic effects, since they all flow from the "treatment" of randomly being assigned a high PGS score as opposed to a low PGS score. In other words, these downstream effects represent mediating pathways that just happen to be environmental.

On the other hand, a valid concern may be that the PGS is proxying for upstream environmental factors, such as toxins *in utero* which, due to the maternal-filial correlation of the PGS, are spuriously correlated with educational attainment. Specifically, by definition, a child's PGS is correlated at 0.5 (or even slightly higher due to assortative mating) with the mother's PGS (Conley et al., 2015). Thus, if the mother, by virtue of her genetically induced educational attainment (or any other related endophenotype), has healthier eating habits during pregnancy, for example, then her PGS is causing an environmental effect on her offspring that would be weakly associated with the offspring's PGS. This, in turn, might create a spurious correlation between the PGS and educational attainment that is actually the byproduct of the mother's PGS. However, while this is indeed possible, sibling fixed effects models have shown that the effect of the PGS on educational attainment is as large within families as it is between families, suggesting that if this sort of spurious effect exists, it is trivial (Conley et al., 2014; Rietveld et al., 2014).

Finally, using a PGS for educational attainment has several attractive features for the detection and estimation of heterogeneous treatment effects by genotype. Mainly, complex behavioral outcomes of interest to social scientists like educational attainment are usually highly polygenic, or reflect the influence or aggregate effect of many different genes (Visscher, Hill, & Wray, 2008). Thus, interacting a PGS with a plausibly exogenous environmental shock allows researchers to detect heterogeneous treatment effects even if each of the individual genetic effects are small or their exact biological mechanisms are not fully understood (Belsky & Israel, 2014). This is particularly important for the aim of the present study, which seeks to identify whether genetic influences moderated the effect of Vietnam-era military conscription on postservice schooling.

2.2 Conscription and Post-Service Socioeconomic Attainment

The effects of compulsory military service on post-service schooling, earnings, and employment have been largely attributed to two central yet countervailing factors in the literature. On the one hand, conscription may act as an implicit tax on the lifetime education and earnings of soldiers who were coerced into service, paid poorly, and lost valuable labor market experience as a result of their time spent in service (Mason, 1970; Oi, 1967). The two or more years spent on active duty in military service may interrupt or impede career progress, and the training and skills acquired in the military may be of little value to civilian employers or may not be easily transferrable to the civilian sector (Wool, 1968). On the other hand, military service may enhance human capital development through its constellation of job training and support services that are available during and after service.

In particular, there has been a large body of literature that has focused on the long-term effects of the World War II, Korean, and Vietnam-era GI Bills, which among other benefits, provided cash payments for university or vocational tuition expenses.[4] Along these lines, studies have found the GI Bill increased the postsecondary educational attainment of World War II (Bound & Turner, 2002; Lemieux & Card, 2001), Korean War (Stanley, 2003), and Vietnam War veterans (Angrist, 1993; Angrist & Chen, 2011) above that of their nonveteran peers. Estimates are surprisingly comparable across studies and indicate benefits from the GI Bill increased college completion by five to six percentage points and average years of college by 0.20–0.33 years.

Conversely, earnings and employment estimates seem to vary considerably across cohorts and over time. Earlier research finds Vietnam-era veterans tended to be worse off than comparable non-veterans and veterans from the WWII and Korean Wars in terms of income, employment, and job prestige (Card, 1983; Rothbart, Sloan, & Joyce, 1981; Schwartz, 1986). For example, Schwartz (1986) compares the earnings of Vietnam and Korean veterans twelve to sixteen years after their discharge and finds the rate of return per year of education was significantly lower for Vietnam veterans than Korean veterans, who were economically indistinguishable from their nonveteran contemporaries. Similarly, cross-sectional comparisons show WWII veterans had higher earnings than comparable nonveterans (Berger & Hirsch, 1983; Rosen & Taubman, 1982), though this premium disappears and WWII veterans actually appear to have earned less than comparable nonveterans when nonrandom selection into the military is accounted for using instrumental variable methods (Angrist & Krueger, 1994).

Results from more recent studies that use the Vietnam-era draft lottery as an instrument for veteran status find the large earnings losses in in the 1970s and 1980s disappear and rapidly converge to zero in the 1990s, or when veterans were approximately 50 years of age (Angrist, 1990; Angrist, Chen, & Song, 2011). Using data from the 2000 Census, Angrist and Chen (2011) reconcile these results by combining earnings estimates with estimated GI Bill effects in a Mincer-style wage equation, and confirm that the near-zero wage penalty at older ages is due to both the flattening of the age-earnings profile in middle-age and economic returns to schooling funded by the GI Bill, which appears to have roughly offset any modest earnings losses due to service. However, the authors point out that lifetime earnings losses to Vietnam veterans have still been negative overall; after accounting for GI Bill benefits, they calculate the present value of lost earnings at around 10 percent of total earnings through the year 2000 *(ibid*, p. 116).

Finally, studies that use the Vietnam-era draft lotteries have not been able to uncover any evidence of lasting changes in employment rates or labor force participation at older ages (Angrist & Chen, 2011; Angrist, Chen, & Frandsen, 2010). However, the studies mentioned

[4]The WWII GI Bill included a $500 tuition supplement and a monthly stipend and was similar to Vietnam-era benefits in the 1970s (adjusting for inflation); benefits were almost double the average cost of tuition, room, and board at a four-year public university during this time period (Angrist & Chen, 2011; Bound & Turner, 2002).

above were not able to estimate the local average treatment effect (LATE) of the draft across the PGS distribution. If individuals experience a differential response or reaction to the environment because of their genotype (Grishkevich & Yanai, 2013), otherwise observationally identical individuals may have had drastically different responses to conscription as a result of their underlying genetic endowment.

3. Data and Descriptive Statistics

3.1 The Health and Retirement Study

The HRS is a nationally representative, longitudinal panel study of individuals over the age of 50 and their spouses. Launched in 1992, the study was designed to paint a detailed portrait of the labor force participation and health transitions individuals undergo towards the end of their work lives and into retirement; comprehensive information about participants' socioeconomic background, income, assets, employment, and veteran status is collected alongside genetic data. The HRS introduces a new cohort of participants every six years and interviews around 20,000 participants every two years. For the purposes of this study, we link the Genotype Data Version 1 (2006 and 2008 samples) of the HRS genetic data to both the RAND HRS Data files for socio-demographic information and the restricted Respondent Date of Birth Files (1992–2010) to code for draft eligibility.[5]

The majority of men who were draft eligible during the Vietnam Era entered the HRS in 2004 as part of the "Early Baby Boomer" cohort—a nationally representative sample of men and women born between 1948 and 1953. However, to maximize sample size, we also include spouses of female respondents from former cohorts that were born between 1948 and 1952. Since we use findings from a GWAS conducted on individuals of European ancestry to construct the PGS, our sample excludes respondents who report being of Hispanic, African, American Indian, Alaskan Native, Asian, or Pacific Islander ethnic origin (Carlson et al., 2013). Our final analytic sample includes 504 white, non-Hispanic men born between 1948 and 1952 who provided DNA samples in 2006 or 2008.

3.2 Draft eligibility

Between 1969 and 1972 the U.S. Selective Service held four Vietnam-era draft lotteries. The first lottery, held in December 1969, affected men born between 1944–1950 who were at risk of conscription in 1970, while subsequent draft lotteries held in 1970 and 1971 affected men who were 19 years old only, or who were born in 1951 or 1952, and at risk of conscription in 1971 or 1972, respectively. A final lottery was held in 1972 for men born in 1953, but no draft calls were issued in 1973.

For each lottery, individual birthdates (including February 29th) were placed in a blue capsule, and then independently drawn until each day of the year was paired with a random sequencing number (RSN) between 1 and 366. After needs for manpower were determined, a draft-eligibility ceiling or cutoff was assigned to each lottery. Men with RSNs below the cutoff were considered draft eligible, while men with RSNs above the cutoff were exempt

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[5]The RAND HRS Data (Version O, 2015) is an easy to use longitudinal data set based on the HRS data. It was developed at RAND with funding from the National Institute on Aging and the Social Security Administration, Santa Monica.

from the draft. Draft eligibility ceilings were pegged at 195, 125, and 95 for the 1969, 1970, and 1971 lotteries, respectively (see Table 1). Based on these eligibility cutoffs, we follow prior studies and code draft eligibility as equal to "1" if the respondent's RSN was called in the corresponding draft year and "0" otherwise.[6]

Due to the high number of men who volunteered for the military or received educational deferments before the first draft lottery, studies that use the draft lottery as a natural experiment typically exclude men born between 1944 and 1949 since the link between draft eligibility and veteran status is far from deterministic in these earlier birth cohorts (Angrist, 1990). Specifically, the leading alternative explanation for schooling effects estimated using draft- lottery instruments is draft avoidance through education-related deferments, whereby students enrolled in college in the 1960s could avoid conscription by staying in school. Eventually, these deferments were phased out in April 1970 by President Richard M. Nixon, and in 1971 all new deferments ended and existing deferments were extended by one term or to graduation for college seniors (Angrist & Chen, 2011). Educational deferments may compromise 2SLS estimates if the high correlation between draft eligibility and schooling is a reflection of draft avoidance behavior rather than military service—i.e. if it is a capturing the effect of men with low draft lottery numbers who "beat the draft" by obtaining educational deferments.

In general, past studies have found strong evidence against draft avoidance behavior in the draft eligible cohorts born after 1951. Card and Lemieux (2001) analyze cohort- and sex-specific college enrollment rates and find relatively little deviation from trends in male-to-female ratios of college graduates in cohorts born after 1950. Similarly, Angrist and Chen (2008) use the 1972–1989 CPS to show that veterans experienced substantial post-service schooling gains relative to nonveterans in the years after they were discharged. Finally, Angrist and Chen (2011) estimate the effects of service on educational attainment by year of birth and find small but statistically significant positive effects for white men born between 1948 and 1952. These effects are largest in the 1951 cohort when deferments were ended or considerably shortened, and in 1952 when college deferments were completely phased out. Weighing this evidence against the sharp decline in educational deferments during the draft lottery period, they argue there is little evidence to support the claim that increases in schooling among draft eligible men are due to draft avoidance behavior.

To determine the probability of service by birth cohort in our HRS sample we regress veteran status on draft eligibility and a constant with controls for month of birth for the 1948– 1949 and 1950–1952 HRS genotyped cohorts. These first stage estimates are comparable to past studies and show that draft eligible men born between 1948 and 1949 were 18.4 percentage points more likely to serve, whereas draft eligible veterans born between 1950 and 1952 were 16.4 percentage points more likely to serve. Thus, due to the similarity of first stage estimates across birth cohorts, we include men of European ancestry born between 1948 and 1952 in our sample to maximize the power of our study.

[6]The results of the Vietnam draft lottery are available at https://www.sss.gov/About/History-And-Records/lotter1

3.3 Schooling Outcomes

The HRS asks respondents their highest grade of school or year of college completed. If the respondent reports 12 or fewer years of school, they are asked whether they obtained a high school diploma or passed a high school equivalency test (GED). If the respondent reports 13 or more years of school, they are asked whether or not they obtained a college degree, and if so the highest degree obtained. These responses are categorized as "less than bachelors", "bachelors", "masters/MBA", or "other" which includes the MD, JD, and PhD degrees. If the respondent reports 13 or more years of education but does not report receiving a college degree, completion of a high school degree is assumed, but the data do not distinguish a high school diploma from a GED.

Based on these data, we analyze five schooling outcomes, including highest grade of schooling attended ("years of education"), total years of college education ("years of college") and a series of dichotomous variables denoting whether a respondent completed each degree level (GED/HS degree +", "associate degree +", and "bachelor's degree +"). Years of education is a continuous variable that can range from "0" for no formal education to "17" for post college education. If the respondent reports receiving at least 12 years of education, we subtract 12 from total years of education reported to obtain "years of college".

Degree variables with a "+" sign are cumulative, so that "GED/HS degree +" indicates those with at least a GED/HS degree. We code these as overlapping subpopulations that diminish from lower to higher levels of schooling so that "associate degree +" compares respondents who received at least an associate degree with those who received a GED/HS degree only while the "bachelor's degree +" variable compares those who received at least a bachelor's degree with respondents who received an associate degree only. This allows us to assess the impact of conscription and genotype on degree transitions, as opposed to their impact on college attendance for all persons, which by definition would compare the cumulative impact of these factors over all previous transitions instead of their respective impact on each degree transition (Mare, 1980). In this way, we can observe changes in the marginal effect of genotype and conscription on degree completion at each level of educational attainment.

3.4 Polygenic Score (PGS)

The linear PGS is calculated for the HRS sample based on results from a GWAS meta-analysis of educational attainment conducted by the Social Science Genetics Association Consortium (SSGAC) among people of European ancestry (Okbay et al., 2016). The PGS aggregates thousands of single nucleotide polymorphisms (SNPs) across the genome and weights them by the strength of their association using beta weights derived from the GWAS to yield a single scalar of genetic propensity for educational attainment. Or, the PGS is a weighted sum across the number of SNPs (n) of the number of reference alleles (x)(zero, one, or two) at that SNP multiplied by the effect size for that SNP (β):

$$PGS_i = \sum_{j=1}^{n} \beta_j x_{ij} \quad (1)$$

We incorporate beta weights from the largest GWAS meta-analysis discovery sample to date, which includes genotype data on individuals from both the UK Biobank and 23andMe in addition to individuals from other participating studies in the SSGAC (N=395,109). Since the HRS is part of the GWAS discovery sample, we use weights estimated by the SSGAC that exclude the HRS sample.[8] HRS Genotype Data Version 1 (2006 and 2008 samples) were used to construct the PGS—i.e. imputed data were not analyzed.[9] A total of 1,411,964 SNPs in the HRS genetic database overlapped or matched SNPs from the GWAS meta-analysis. All available SNPs were used to construct the PGS in the software LDpred (Vilhjalmsson et al., 2015).[10] The PGS is standardized to have a mean of zero and a standard deviation of one for the population of white, non-Hispanic males born between 1948 and 1952.

To control for confounding from population stratification, or to account for any ancestral differences in genetic structures within populations that could bias estimates, we apply principal components analysis to the HRS genotype data and calculate axes of genetic variation that arise from systematic ancestry differences. The resulting principal components (PCs) measure orthogonal genetic variation or dimensions in the genotype data, which accounts for the nonrandom distribution of genes across populations. In other words, the PCs control for any genetic aspects of common ancestry that could be spuriously correlated with the PGS and schooling outcomes, leaving behind residual genetic information that is uncorrelated with any measured or unmeasured ancestral differences. Thus, after including PCs in the regression analysis, there is no need to completely specify ancestry with observables because we are using the genetic data to control for any systematic differences in allele frequencies between subpopulations that may be correlated with different cultural or ethnic norms that also affect schooling. We calculate the principal components using second-generation PLINK software (Purcell et al., 2007) on the entire sample of genotyped respondents of European ancestry (i.e. white, non-Hispanics), and include the first four in our regression analysis—a dimensionality that has generally proven adequate in the literature (Price et al., 2006).[11]

The score is predictive of all education phenotypes in base, non-interactive main effect models for both the entire sample of white, non-Hispanic genotyped males and in our sample of white, non-Hispanic men born between 1948 and 1952 (see Table 2). The score explains 5.5 percent of the variation in years of education for all white, non-Hispanic males and 13.7 percent of the variation in the Vietnam sample.[12] The lower penetrance of the PGS in the entire sample of HRS men is commensurate with other research that has shown a

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[8]The total meta-analysis discovery sample size with the HRS included is 405,072.

[9]Genotyping was performed on the HRS sample using the Illumina Human Omni-2.5 Quad beadchip (HumanOmni2.5-4v1 array). The median call-rate—i.e. the fraction of measured or “called” SNPs per sample divided by the total number of SNPs in the dataset—for the 2006 and 2008 samples is 99.7%. A standard quality control threshold for excluding DNA samples with a low call rate is 95%.

[10]LDpred uses a Bayesian method to calculate PGSs that estimates posterior mean effect sizes from GWAS summary statistics by assuming a prior for the genetic architecture and linkage disequilibrium (LD) information from a reference panel. This method has been shown to increase the predictive accuracy of PGSs because it does not discard informative markers that may increase heritability estimates and also accounts for the effects of linked markers that can lead to biased estimates, unlike other methods that use pruning or clumping to deal with LD (Vilhjalmsson et al. 2015). The PGS is constructed in LDpred using an LD window of 180 and the fraction of SNPs with non-zero effects assumed to be 1.

[11]We also ran the analysis with the first 10 principal components and did not find any significant differences in the results.

[12]In comparison, parental education (i.e. years of education received by a respondent’s mother and father) explains 18.1 percent of the variation in years of education for all white, non-Hispanic males, and 16.1 percent of the variation in the Vietnam sample.

decreasing trend in the genomic influence on education in the 20th century in the U.S. due to the expansion of post- secondary schooling (Conley et al, 2016). Thus, the higher PGS penetrance in the Vietnam sample may be the byproduct of higher college completion rates among men with above average PGSs who took advantage of GI Bill services. Another interpretation of these results is that the GI Bill lifted the financing constraint on higher education for above average PGS individuals who otherwise could not afford to go to college.

3.5 Descriptive statistics

Veterans are likely to have different educational backgrounds than non-veterans for two primary reasons. First, individuals from higher socioeconomic backgrounds are less likely to enlist in the military because they have more employment and educational opportunities. Second, socioeconomic background aside, individuals with greater potential for educational attainment may be more likely to pursue a postsecondary degree after high school. The first few columns in Table 3 verify these differences. On average, non-veterans completed more years of education (14.5) than veterans (13.7) and more years of college (2.7) than veterans (1.8). Veterans were 18.8 percentage points more likely to have only a high school degree or GED compared to non-veterans, and veterans who attended college were more likely to only obtain an associate degree (0.12) compared to non-veterans (0.05). Completion of a bachelor's or advanced degree among non-veterans is nearly double that of veterans; approximately 47.6 percent of non-veterans obtained a bachelor's or advanced degree compared to only 23.6 percent of veterans. The PGS for educational attainment is also 0.32 standard deviations lower in the veteran sample, indicating that veterans with lower PGSs were more likely to select into military service. These observable differences between veterans and nonveterans may lead to biased cross-sectional estimates of the consequences of military service on schooling outcomes if there are other systematic (unobservable) differences between the two groups that cannot be accounted for.

In addition, since the HRS is a study of older adults, potential problems could arise if conscripts who survived to be genotyped in 2006 or 2008 were more likely to be educated and therefore have lower mortality rates than comparable populations (Domingue et al., 2016). If increased education is an alternative pathway affecting the military conscription-mortality relationship, draft eligibility is no longer exogenous, and the exclusion restriction on which our 2SLS estimates rest will be violated.

Prior studies on selective mortality effects in the draft-lottery sample have been mixed. A pioneering study by Hearst, Newman, and Hulley (1986) found excess deaths from suicide and motor vehicle accidents among draft-eligible men from California and Pennsylvania. However, more recently, Conley and Heerwig (2012) find no effect of draft exposure on mortality, including for cause-specific death rates, in a larger sample of national death records, and Angrist et al. (2010) find little evidence of elevated mortality among draft-eligible men in the 2000 Census. Importantly, Conley and Heerwig (2012) do find some evidence of elevated mortality among draft-eligible, college-educated men. However, they argue this effect works in the opposite direction of putative education-enhancing effects that could potentially violate the exclusion restriction in IV regression models—i.e. high SES men were not more likely to survive. Crucially, descriptive statistics in Table 3 reveal no significant difference in draft eligible and ineligible means by PGS, schooling outcome, or marital status, suggesting bias from unobserved differences, mortality selection, sample attrition, or genetic screening consent is modest if nonexistent in the draft eligible birth cohorts.

Of note, in the entire HRS genotyped male sample (N=3,530), year of birth is highly correlated with the PGS (see also Conley et al., 2016)—perhaps not surprising given the well- documented association between education and longevity (Conti, Heckman, & Urzua, 2010; Cutler & Lleras-Muney, 2010; Lleras-Muney, 2005). However, we find that the PGS is not correlated with year of birth in our analytic sample, indicating that men with above average PGSs were not more likely to survive than men at the bottom half of the PGS distribution. The difference between the overall sample and our analytic sample is probably due to the fact that the Vietnam-draft birth cohorts are still on the younger side of HRS respondents and thus may not have fully evinced educational-PGS associated mortality. That said, adjusting for mortality bias generally does not affect cohort-PGS results in the HRS even across the entire range of the sample (Domingue et al., 2016).

Finally, we compare descriptive statistics for men in our sample that were genotyped (N=504) with same-age white, non-Hispanic men who were not genotyped (N=485) (Table A1). Two sample t-tests reveal no significant differences between the means of the two groups with the exception that men who were not genotyped are three percentage points more likely to not have an academic degree. We also find evidence that genotyped men were more likely to be married while they were in the HRS sample (0.87) than men who were not genotyped (0.83), though this difference is only statistically significant at the $p < 0.10$ level.

4. Empirical Methods and Results

4.1 Reduced-Form Estimate Comparisons by Genotype

The estimated effect of military service on educational attainment is dependent on the reduced form relationship between draft eligibility and educational attainment and the first stage relationship between eligibility and veteran status. As described in Section 3.2, our first stage estimates of the effect of draft eligibility on veteran status are comparable with prior studies. To investigate the reduced form relationship, we plot average years of education across PGS deciles by draft eligibility. The results in Figure 1 show preliminary evidence of a negative G × E effect in the bottom 50 percent of the PGS distribution, where draft eligible men appear to have obtained fewer years of schooling on average than draft ineligible men.

To more formally evaluate the reduced form relationship we estimate the following interaction model:

$$Y_i=\alpha_0+\alpha_1 DRAFT_i+\alpha_2 DRAFT_i \times PGS_i+\alpha_3 PGS_i+X_i^{\prime}\alpha_4+\varepsilon_i \quad (2)$$

Where $DRAFT_i$ is coded as "1" for draft eligible men and "0" for draft ineligible men in accordance with the draft eligibility ceilings reported in Table 1, PGS_i is the polygenic score for educational attainment, $DRAFT_i \times PGS_i$ is their interaction, Y_i is the schooling outcome of interest, X_i is a vector of exogenous controls that includes the first four principal components to account for population stratification in the genotype data and dummies for year and month of birth, and ε_i is the disturbance term.[13]

Table 4 reports the reduced form results from Eq. 2. Column 1 reports results without the interaction term; the coefficient on draft eligibility does go in the expected direction (negative), but is not statistically significant. We also do not find a significant interaction between the PGS and draft eligibility for any of the five educational outcomes (Columns 3–7). Given that our sample is considerably smaller than past studies that have used the Vietnam-era draft lotteries as an instrument, we conducted power analysis to see if we are adequately powered to detect the interaction term at a standard level of significance (α = 0.05). Current G × E research that uses PGSs has identified effect sizes for G × E interactions that range from approximately 0.2% to 1.2% (Belsky et al., 2016; Okbay et al., 2016; Papageorge & Thom, 2016). Using these effect sizes and the standard benchmark of 80 percent power (π = 0.80), we find that we are not adequately powered to detect the G × E term (π = 0.044 − 0.40).[14] Moving forward, this underscores the need for G × E interaction research that can exploit quasi-experimental designs with large sample sizes to adequately detect effects.

Since the majority of the differences in the effect of draft eligibility by genotype appear to be occurring in the bottom 50 percent of the genotype distribution, we also estimate the reduced form relationship separately for men with PGSs above and below the median value to see if we are able to detect any effects (Table 5). Here we do see a significant ($p < 0.05$) association between draft eligibility, years of education, and obtainment of at least an Associate degree for men in the bottom half of the PGS distribution. Draft eligible men with a PGS below the median value received 0.587 fewer years of education and were 12.8 percentage points less likely to obtain a posts-secondary degree as compared to their draft ineligible counterparts.

However, although we do find significant effects for draft eligibility in the bottom half of the PGS distribution, these estimates are not statistically different from estimates in the top half of the distribution. Again, this is likely a byproduct of inadequate power to detect a significant difference in the point estimates. Notably, comparable "naive" OLS estimates of veteran status show the opposite relationship from the reduced form—i.e. men in the top 50 percent of the PGS distribution appear to have received less education than men in the

[13]A mechanical failure in the implementation of the first round of the lottery (balls with the days of the year were not mixed sufficiently after having been dumped in a month at a time) resulted in a disproportionately high probability of being drafted for those born in the last few months of the year (Fienberg, 1971). This could bias estimates if those born later in the year differ in important ways from those born at other times during the year. For example, studies have shown health varies with season of birth.

[14]Power was calculated as $\Phi\left(\Phi^{-1}\left(\frac{\alpha}{k}\right) - \sqrt{NR^2}\right) + 1 - \Phi\left(\Phi^{-1}\left(1 - \frac{\alpha}{k}\right) - \sqrt{NR^2}\right)$, where Φ is the cumulative distribution function (CDF) of the standard normal distribution and Φ^{-1} denotes its inverse function, N=504, R^2 = 0.0001–0.012, and α=0.05. Since draft eligibility and the PGS are independent of one another, we set k=15 to account for multiple hypothesis testing (i.e. three independent tests across five educational outcomes).

bottom 50 percent. This estimate is likely biased by other factors that may have affected an individual's decision to select into military service, and again emphasizes the importance of using environments that are plausibly exogenous when estimating G × E interactions.

4.2 2SLS Model with Heterogeneous Treatment Effects by Genotype

To further elucidate how a quasi-experimental framework can be used to identify heterogeneous treatment effects by genotype, we incorporate the PGS into a 2SLS model that instruments veteran status with draft eligibility.[15] Since past research has shown draft eligibility is 1) highly correlated with actual veteran status, and 2) only affects educational outcomes through the first stage channel, or through its correlation with veteran status, it is considered a valid instrument for military service.[16]

On the other hand, unlike past studies, including an interaction effect between instrumented military service and the PGS presents additional challenges that revolve around how the main effect of the PGS is specified. Since conditional on parental genotype, the PGS is randomly assigned prior to birth, we assume it is exogenously determined. However, because the PGS is capturing a wide-swath of genetic influences, we cannot use it as an excluded instrument for veteran status because this would violate the exclusion restriction. In other words, because the pathways through which the PGS may be affecting educational attainment are numerous (and ultimately unknowable) we cannot say with certainty that its effects are operating exclusively through veteran status. Thus, we model the main effect of the PGS as a control (as opposed to an excluded instrument). Consider the following G × E interaction model:

$$Y_i = \delta_0 + \delta_1 VET_i + \delta_2 VET_i \times PGS_i + \delta_3 PGS_i + X_i' \delta_4 + \varepsilon_i \quad (3)$$

Where VET_i is coded as “1” if individual i reports serving in the military and “0” otherwise, PGS_i is the polygenic score for educational attainment, $VET_i \times PGS_i$ is their interaction, Y_i is the schooling outcome of interest, X_i is a vector of exogenous controls that includes the first four principal components to account for population stratification in the genotype data and dummies for year and month of birth, and ε_i is the disturbance term. The VET_i and $VET_i \times PGS_i$ terms are treated as endogenous and the $DRAFT_i$ and $DRAFT_i \times PGS_i$ terms are used as excluded instruments to estimate the following first stage regressions:

[15]The IV estimates of effects of military service using the draft lottery capture the effect of military service on “compliers”, or men who served because they were draft eligible but who would not otherwise have served. It is not, therefore, an estimate of the effect of military service on men who volunteered. See Angrist and Pischke (2008) for a more detailed discussion of the interpretation of the LATE for the Vietnam-era draft.

[16]The first condition is easy to verify, and standard first stage statistics (partial R^2 and F- statistic) for the significance of the instruments in the HRS sample show draft eligibility is a robust predictor of veteran status (see Table 6). The exclusion restriction, or second condition, cannot directly be tested. Heckman (1997) shows the IV estimator is not consistent if heterogeneous behavioral responses to the treatment—or military service in this case—are correlated with the instrument (i.e. draft eligibility). However, past research has provided convincing counterfactuals that suggest the exclusion restriction holds. For example, Angrist (1990) found no significant relationship between earnings and draft eligibility status for men born in 1953 (where draft eligibility was defined using the 1952 lottery cutoff of 95). Since the 1953 cohort was assigned RSNs but never called to service, if the draft affected outcomes directly, we would expect outcomes to vary by draft eligibility for this cohort. A final assumption for the 2SLS estimation is monotonicity of the instrument. In the case of the Vietnam draft lotteries, this means that the draft lottery should only have increased the probability of military service—i.e. we assume that no one was actually kept out of the military by being draft eligible (Angrist & Pischke, 2009).

$$VET_i = \pi_0 + \pi_1 DRAFT_i + \pi_2 DRAFT_i \times PGS_i + \pi_3 PGS_i + X_i^{'} \pi_4 + \eta_i \quad (4)$$

$$VET_i \times PGS_i = \gamma_0 + \gamma_1 DRAFT_i + \gamma_2 DRAFT_i \times PGS_i + \gamma_3 PGS_i + X_i^{'} \gamma_4 + \rho_i \quad (5)$$

Where the model is exactly identified—i.e. the number of excluded instruments ($DRAFT_i$ and $DRAFT_i \times PGS_i$) are equal to the number of endogenous regressors (VET_i and $VET_i \times PGS_i$). The first stage estimates from Equations (4) and (5) are used to construct 2SLS estimates of Equation (3). For all limited and continuous dependent variables in this study, the second stage equation is estimated with a simple linear probability model because it is the ideal specification when faced with a set of simultaneous equations where the instrument, the endogenous regressor, and the dependent variable take on a limited set of values (Angrist & Pischke, 2008).

Table 6 contains first stage estimates from the 2SLS model. Column 1 reports first stage estimates from a model without the PGS and Column 2 reports first stage estimates from the model with the PGS. Measures of instrument relevance (Partial R^2 and Shea's partial R^2) suggest that the standard error of IV estimates are inflated by a factor of approximately seven, which is close to the inflation we observe in the standard errors reported in Table 7. We also include measures of weak instruments based on the degree of finite-sample bias (F-statistic and minimum eigenvalue statistic). The F-statistic reported is the F-statistic for the joint significance of the instruments for the endogenous regressor in each first stage equation reported in Columns 1-3, and the minimum eigenvalue statistic is the critical value for the weak instrument test based on size of the 2SLS estimator (Stock & Yogo, 2005). The F-statistic and minimum eigenvalue statistics decline considerably after the addition of the PGS into the 2SLS framework, and we cannot reject the null hypothesis of weak instruments based on the critical values reported by Stock and Yogo (2005).

Measures of instrument relevance and finite sample bias may in part be lower in the 2SLS PGS model because there appears to be a gene-environment correlation (rGE) between military service and the PGS, whereby individuals with lower PGSs were more likely to select into military service on average (see Table 3). The result of this rGE is that after controlling for the main effect of the PGS in the first stage, there is little variation left from which to identify the interaction term. In other words, because the PGS is highly associated with veteran status the first stage correlation between Veteran × PGS and its instrument, Draft × PGS, is considerably weakened after controlling for the PGS main effect. As a result, researchers should be aware that if they are estimating G × E interactions in a 2SLS framework the existence of rGE between the PGS and the endogenous regressor will affect the strength of the G × E instrument.

Of concern, it would be problematic if the coefficient on the $DRAFT_i \times PGS_i$ term captures both the differential impact of military service on individuals with different PGSs, and the differential impact of draft eligibility on military service for individuals with different PGSs.

However, the first stage results in Table 6 (Column 2) suggests that the second effect is nonexistent—i.e. the impact of draft eligibility on veteran status is not significantly different by PGS. Furthermore, a comparison of first stage results in Table 6 reported from the traditional draft eligibility model without the PGS (Column 1) and with the PGS (Column 2) show that including the PGS does not alter the relationship between draft eligibility and veteran status. Finally, a separate regression of draft eligibility on the PGS reveals that draft eligibility is not correlated with the PGS (Table A2).

4.3 2SLS Results

Table 7 reports the OLS and 2SLS estimates for all five educational outcomes of interest. To facilitate comparison with the previous literature, we also report OLS and 2SLS estimates from a model without the PGS interaction (Columns 1–2). Since we standardize the PGS on our analytic sample, for all models with the PGS interaction (Columns 3–8), 2SLS estimates of "Veteran" capture the effect of conscription on schooling at the mean value of the PGS (i.e. PGS=0), the "Veteran × PGS" coefficient captures the difference in years of education between veterans and non-veterans at all other values of the PGS, and the PGS coefficient is the effect of the PGS on men who were not draft eligible.

Across the board, the magnitude of the OLS estimates and the estimated standard errors are considerably smaller than the 2SLS estimates. To check whether 2SLS estimation is necessary, we ran a Durbin-Wu-Hausman test. Results from the test reject the null hypothesis that the OLS estimator is consistent and fully efficient ($H = 13.516$, $p < 0.001$). Thus, we cannot rule out the possibility that the OLS estimates are biased due to other unobserved characteristics.

Pivoting to the 2SLS estimates, we report coefficients from a model without the PGS in Column 2 and results from the interaction model with the PGS in Columns 4, 6, and 8. In contrast to the education outcomes reported by Angrist and Chen (2011), who find that compulsory military service increased the educational attainment of white Vietnam-era veterans by 0.332 years, our 2SLS estimate for years of education without the PGS is not significant and displays the opposite sign. However, importantly, we note that the 95% confidence interval for our point estimate does contain the Angrist and Chen estimate (95% CI [−4.68, 0.607)]).

Due to the considerable inflation of our 2SLS standard errors, we do not find any significant results from our models with the PGS. However, the consistent pattern in the direction of the sign on coefficient estimates for years of education, years of college, and associate degree + are similar to the results obtained from our reduced form model, and suggest that veterans with lower PGSs obtained less education than non-veterans. Specifically, the negative sign on the "Veteran" coefficients and the positive sign on the "Veteran × PGS" coefficients indicate that individuals with higher PGSs obtained more education on average than veterans with lower PGSs.

Finally, we report the LATE of veteran status by PGS, or the total difference in schooling outcomes between veterans and non-veterans by PGS, in Table 8. The LATEs are estimated by taking a linear combination of the marginal effects for veterans from the 2SLS estimates

("Veteran" + "Veteran × PGS") for each PGS value. Again, although our estimates are not statistically significant, we do see evidence suggesting that veteran status had a larger negative impact at the bottom of the PGS scale.

Interestingly, the opposite appears to be true for obtainment of at least a GED/HS degree. Here, the sign of the effect suggests that veterans with lower PGSs were more likely to complete at least a GED/HS degree than non-veterans. This is not surprising given that the vast majority of veterans—regardless of genotype—were exposed to the draft after they completed high school. In addition, veterans who were scheduled to graduate and left high school prematurely to serve in the war were eligible to receive a GED during service, and since the 1990s, many states have also developed programs to grant high school diplomas to qualifying Vietnam-era veterans (Angrist & Chen, 2011).

5. Discussion

Surviving Vietnam-era veterans are currently the single largest veteran population in the United States. Over 7.2 million Vietnam veterans constitute 32.9 percent of the total veteran population and receive the largest overall share and per veteran share of service-related disability benefits (Statistics, 2015)—a figure that is growing as the population ages. The annual compensation to veterans from the Vietnam Era more than doubled between 2003 and 2012, reaching $19.7 billion of the total paid to veterans that year of $44.4 billion *(ibid).* Given the well-established relationship between investments in human capital and improved health, employment opportunities, and income across the life span, a deeper understanding of whether biological and social forces shape the educational outcomes of veterans is needed.

Overall, results from the reduced form and our 2SLS model suggest that military service may have had a larger effect on individuals in the lower half of the PGS distribution. The direction of the main effects and the interaction effects in both models, though not statistically significant, indicate that draft eligible men with lower PGSs completed fewer years of schooling and were less likely to obtain a postsecondary degree. Taken together, this suggests that high- PGS veterans may have been more likely to take advantage of generous GI Bill benefits, which paid more than double the average cost of tuition, room, and board at a four-year public university in the 1970s. Another possible explanation is that these individuals were more resilient to any potential negative effects from service that may have impacted long-term educational achievement. However, overall the size of the available dataset prevents us from drawing stronger conclusions.

A significant drawback of our 2SLS estimation strategy is we cannot fully describe the underlying theoretical relationships between military service and educational attainment that may be driving our results. For example, we cannot disentangle the particular aspects of military service in Vietnam—such as combat positions, year of service, or number of tours—that may be linked to post-traumatic stress or other debilitating conditions that could have limited the longterm socioeconomic attainment of veterans. In particular, assignment to the theater of war was by no means random and studies have shown that men with lower Armed Forces Qualification Test (AFQT) scores and fewer support role skills were more likely to be exposed to combat (e.g. Gimbel & Booth, 1996). The data reported here are entirely consistent with higher IQ conscripts holding safer non-combat or administrative support positions and taking disproportionate advantage of the GI Bill after service. In other words, our 2SLS model does not explain why Vietnam-era service may have negatively affected the educational attainment of veterans with below average PGSs.

On the other hand, despite weaker claims to external validity, in a larger data set the model's internal validity may direct practitioners to effective treatments that could increase the educational attainment, enhancement of skills, and overall assimilation into civilian life of returning veterans. Although estimates from our study apply specifically to Vietnam-era veterans, and thus cannot be generalized to the population as a whole or even to current military personnel, to the extent that the Vietnam-era draft lottery serves as a proxy for stressful events in young adulthood, or exposure to combat, policymakers may want to design interventions that minimize the negative impact of similar traumatic events on scholastic achievement. In terms of future research, more work is needed to identify how genetic attributes modify the effect of military service on earnings and employment in mid-life. In particular, studies that can track veterans across the entire life course would lend insight into whether intervention during sensitive time periods can reduce long-term disparities in socioeconomic attainment.

More generally, this study suggests a model for deploying molecular genetics to assess heterogeneous treatment effects: We use exogenous variation in the environment and combine it with a well-validated genetic score that displays large and significant effects on the outcome of interest in independent samples. Since heterogeneous treatment effects not only have important implications for social and behavioral research and social policy but also appear to be the norm in response to interventions or to naturally occurring environments (e.g. Angrist & Krueger, 1999; Heckman, 2001; Manski, 2009; Winship & Morgan, 1999), our intuition is that genotype may provide the prism to separate out the "white light" of average treatment effects into its constituent "rainbow" of heterogeneous responses by genotype. The present study, we hope, has provided proof of concept for the utility of genotype as a moderator of social inputs that itself may generate hidden forms of inequality.

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Appendix

Table A1

Descriptive statistics by genotyped status, 1948–1952 birth cohorts, white non-Hispanic males

	All	Not Genotyped	Genotyped	Diff.
Drafted	0.439	0.443	0.435	0.009
Veteran	0.355	0.348	0.361	−0.013
Years of education	14.1	14.008	14.188	−0.180
Years of college (0–5)	2.303	2.247	2.357	−0.110
Highest degree completed				
No degree	0.063	0.078	0.048	0.03**
High school degree	0.496	0.503	0.49	0.013
Associate degree	0.068	0.062	0.073	−0.012
Bachelor's degree	0.216	0.196	0.236	−0.040
Advanced degree	0.157	0.161	0.153	0.008
Ever married	0.848	0.827	0.869	−0.042*
N	989	485	504	

Notes: PGS is standardized on the analytic sample.

* $p<0.10$,

** $p<0.05$.

Table A2

Correlation of the PGS with draft eligibility

	Draft eligibility
PGS	0.0155 (0.0225)
N	504
R^2	0.201

Notes: PGS is standardized on the analytic sample. Regression includes controls for respondent month and year of birth and population stratification in the genotype data.

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Highlights

- An instrumental variable model is used to examine gene-environment interactions.
- A polygenic score for educational attainment is interacted with veteran status.
- The draft lottery is used as an instrument for Vietnam-era military service.
- The effect of Vietnam-era military service on schooling varies by genetic endowment.
- Veterans with below average polygenic scores appear to have completed fewer years of schooling.

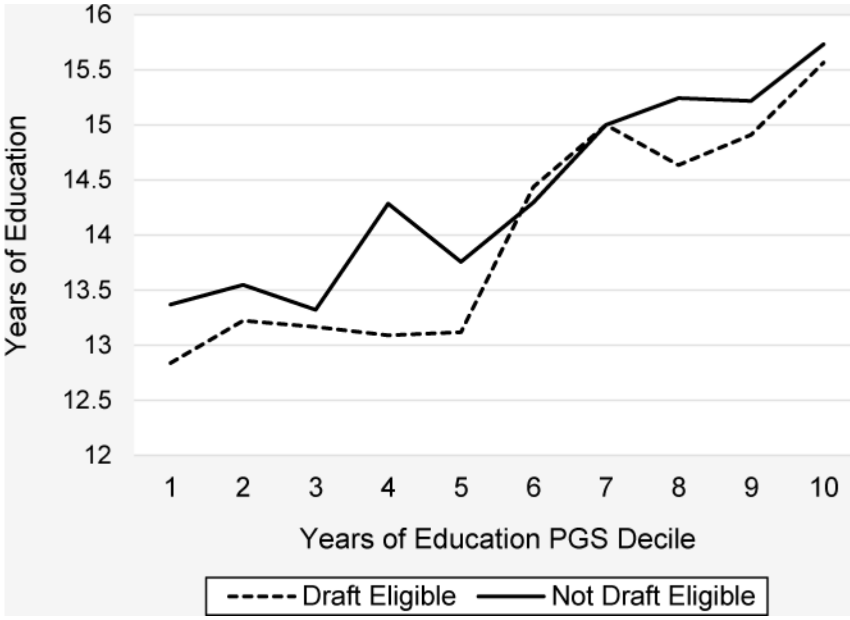

Figure 1.
Average Years of Education by PGS Decile and Draft Eligibility

Table 1

Draft eligibility numbers by birth cohort and lottery year

Lottery year	Birth cohort(s)	Eligibility ceiling
1969	1944–1950	195
1970	1951	125
1971	1952	95
1972	1953	–

Source: U.S. Selective Service

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Table 2

Main effect of the PGS on schooling outcomes

	Years of education	Years of college	GED/HS degree +	Associate degree +	Bachelor's degree +
All white males	0.639 *** (0.045)	0.478 *** (0.031)	0.043 *** (0.006)	0.100 *** (0.008)	0.045 *** (0.010)
N	3530	3530	3530	3070	1250
R^2	0.055	0.058	0.017	0.042	0.017
Veteran sample	0.845 *** (0.087)	0.762 *** (0.071)	0.022 ** (0.008)	0.153 *** (0.022)	0.097 *** (0.025)
N	504	504	504	480	233
R^2	0.137	0.154	0.010	0.094	0.064

Notes: Each column reports a separate ordinary least squares (OLS) regression of the dependent variable on a constant and the years of education PGS. R^2 reported is the incremental R^2 for the PGS. All regressions control for population stratification in the genotype data. PGS is standardized on the analytic sample. Robust standard errors are in parentheses.

** p<0.05,

*** p<0.01

Table 3

Descriptive statistics by veteran and draft eligibility status, 1948–1952 birth cohorts, white non-Hispanic males

	All	Non-veteran	Veteran	Diff.	Not Draft Eligible	Draft Eligible	Diff.
Drafted	0.435	0.366	0.555	−0.188 ***	0	1	
Veteran	0.361	0	1		0.284	0.461	−0177 ***
Years of education PGS	0	0.114	−0.202	0.315 ***	−0.025	0.032	−0.057
Years of education	14.188	14.478	13.676	0.802 ***	14.316	14.023	0.293
Years of college (0–5)	2.357	2.665	1.813	0.851 ***	2.446	2.242	0.204
Highest degree completed							
No degree	0.048	0.056	0.033	0.023	0.049	0.046	0.003
High school degree	0.49	0.422	0.61	−0.188 ***	0.467	0.521	−0.054
Associate degree	0.073	0.047	0.121	−0.074 ***	0.081	0.064	0.017
Bachelor’s degree	0.236	0.28	0.159	0.120 ***	0.232	0.242	−0.010
Advanced degree	0.153	0.196	0.077	0119 ***	0.172	0.128	0.0441
Ever married	0.867	0.86	0.879	−0.019	0.87	0.863	0.007
N	504	322	182		285	219	

Notes: PGS is standardized on the analytic sample.

* p<0.10,

** p<0.05,

*** p<0.01

Table 4

Reduced form estimates of draft eligibility and the PGS on education outcomes

	Years of Education			Years of College	GED/HS degree +	Associate degree +	Bachelor's degree +
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Draft	−0.328 (0.235)	−0.384* (0.219)	−0.383* (0.219)	−0.307* (0.181)	0.011 (0.021)	−0.074 (0.048)	−0.021 (0.064)
PGS		0.816*** (0.090)	0.831*** (0.116)	0.710*** (0.102)	0.036*** (0.011)	0.113*** (0.029)	0.098** (0.041)
Draft × PGS			−0.033 (0.178)	0.007 (0.153)	−0.018 (0.017)	0.053 (0.044)	−0.040 (0.062)
N	504	504	504	504	504	480	233
R^2	0.139	0.254	0.254	0.262	0.133	0.227	0.339

Notes: Regressions control for respondent month and year of birth and population stratification in the genotype data. PGS is standardized on the analytic sample. Robust standard errors are in parentheses.

* p<0.10,

** p<0.05,

*** p<0.01.

Table 5

OLS estimates of the effect of military service and reduced form estimates of the effect of draft eligibility for men with PGSs above and below the median

	OLS			Reduced Form		
PGS ≥ median value	**Yes**	**No**	**Diff.**	**Yes**	**No**	**Diff.**
Years of education	−0.892*** (0.268)	−0.400 (0.274)	−0.492 (0.382)	−0.136 (0.251)	−0.587** (0.293)	0.451 (0.384)
N	256	248		256	248	
Years of college	−0.891*** (0.248)	−0.536** (0.217)	−0.355 (0.328)	−0.099 (0.227)	−0.428* (0.224)	0.330 (0.318)
N	256	248		256	248	
GED/HS degree +	−0.002 (0.021)	0.057* (0.031)	−0.059 (0.037)	−0.004 (0.019)	0.008 (0.033)	−0.012 (0.038)
N	256	248		256	248	
Associate degree +	−0. 180*** (0.067)	−0.121* (0.062)	−0.059 (0.091)	−0.008 (0.062)	−0.128** (0.062)	0.119 (0.087)
N	250	230		250	230	
Bachelor's degree +	−0. 188*** (0.070)	−0.304*** (0.114)	0.116 (0.133)	−0.045 (0.047)	0.087 (0.109)	−0.131 (0.117)
N	156	77		156	77	

Notes: OLS estimates are from separate regressions of the educational outcome on a constant and veteran status for each PGS category. Reduced form estimates are from separate regressions of the educational outcome on a constant and draft eligibility for each PGS category. The difference between estimates is calculated using a post linear combination of the estimation results that accounts for the between-model covariance of parameter estimates. Robust standard errors are in parentheses. No control variables.

* p<0.10,

** p<0.05,

*** p<0.01.

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Table 6

Draft-lottery first stage statistics

	Veteran	Veteran	Veteran × PGS
	(1)	**(2)**	**(3)**
DRAFT	0.164*** (0.0464)	0.165*** (0.0457)	0.087* (0.0470)
DRAFT × PGS		0.041 (0.0433)	0.155** (0.0652)
PGS		−0.090*** (0.0269)	0.253*** (0.0395)
Partial R^2	0.028		
Shea's partial R^2		0.016	0.020
F-Statistic	12.56	7.459	5.637
Minimum eigenvalue statistic	12.70	2.127	
N	504	504	504

Notes: Column (1) reports first stage statistics from a model without the PGS and Columns (2)–(3) report first stage statistics from the interaction model with the PGS. *"DRAFT"* is equal to "1" if an individual was draft eligible and "0" otherwise. All regressions control for respondent month and year of birth; regression results in (2) and (3) include additional controls for population stratification in the genotype data. The F-statistic reported is the F-statistic for the joint significance of the instruments for the endogenous regressor in each first stage equation reported in Columns (1)– (3). Robust standard errors are in parentheses.

* $p<0.10$,

** $p<0.05$,

*** $p<0.01$.

Table 7

OLS and 2SLS estimates of military service and PGS on education outcomes

	Veteran Model		Veteran Model with PGS					
	Veteran		Veteran		Veteran × PGS		PGS	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of education (N=504)	−0.900 *** (0.214)	−2.037 (1.349)	−0714 *** (0.208)	−2.561 (1.874)	−0.289 (0.200)	0.455 (1.563)	0.855 *** (0.117)	0.486 (0.623)
Years of college (N=504)	−0.940 *** (0.181)	−1.590 (1.098)	−0.780 *** (0.176)	−2.187 (1.559)	−0.190 (0.172)	0.618 (1.329)	0.716 *** (0.094)	0.357 (0.531)
GED/HS degree + (N=504)	0.020 (0.0202)	0.068 (0.119)	0.026 (0.021)	0.147 (0.183)	−0.030 (0.019)	-0.157 (0.152)	0.039 *** (0.013)	0.089 (0.060)
Associate degree + (N=480)	−0197 *** (0.0500)	−0.372 (0.280)	−0.163 *** (0.050)	−0.708 (0.518)	−0.027 (0.048)	0.489 (0.435)	0.134 *** (0.027)	−0.080 (0.183)
Bachelor's degree + (N=233)	−0.243 *** (0.0760)	−0.155 (0.255)	−0.237 *** (0.076)	0.039 (0.945)	0.092 (0.073)	−0.211 (0.691)	0.040 (0.028)	0.135 (0.244)

Notes: All regressions control for respondent month and year of birth and population stratification in the genotype data. PGS is standardized on the analytic sample. Robust standard errors are in parentheses.

*p<0.10,

**p<0.05,

***p<0.01.

Table 8

Local average treatment effect (LATE) of veteran status for selected PGS values

	Years of education	Years of college	GED/HS degree +	Associate degree +	Bachelor's degree +
PGS=2	−1.651 (2.086)	−0.951 (1.774)	−0.168 (0.194)	0.269 (0.538)	−0.382 (0.506)
PGS=1	−2.106* (1.221)	−1.569 (1.011)	−0.010 (0.112)	−0.219 (0.300)	−0.172 (0.313)
PGS=−1	−3.016 (3.228)	−2.805 (2.715)	0.304 (0.317)	−1.197 (0.908)	0.249 (1.625)
PGS=−2	−3.472 (4.712)	−3.423 (3.981)	0.461 (0.462)	−1.686 (1.326)	0.460 (2.312)
N	504	504	504	480	233

Notes: The local average treatment effects are estimated by taking a linear combination of the marginal effects for veterans from the 2SLS estimates ("Veteran" + "Veteran × PGS") for each of the above PGS values. Estimated standard errors for the linear combination of marginal effects are in parentheses.

* $p<0.10$.