Un-Fortunate Sons: Effects of the Vietnam Draft Lottery on the Next Generation's Labor Market[[1]](#footnote-1)

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**Abstract**

We study how conscription in one generation affects the next, exploiting randomized variation from the Vietnam draft lottery.  Using federal tax data, we link fathers from draft cohorts to their sons and offer two primary findings.  First, sons of fathers at risk of being drafted have lower earnings and labor force participation than their peers.  Second, they are more likely to volunteer for military service themselves.  Similar but smaller effects are uncovered for daughters. Our findings are most consistent with two separately operating channels: (1) parental inputs as important determinants of human capital development and (2) intergenerational transmission of occupation.

1. **Introduction**

The economic consequences of Vietnam-era conscription for military draftees have been well studied. Early seminal work estimated substantial earnings losses in the decades following the war, while subsequent analyses found that the gap in earnings disappeared over time (Angrist, 1990; Angrist and Chen, 2011; Angrist, Chen, and Song, 2011). Other related research has documented swollen disability rolls among servicemen from this period, reflecting a combination of negative health outcomes for these men and increased generosity in the program (e.g., Angrist, Chen, and Frandsen, 2010; Duggan, Rosenheck, and Singleton, 2010; Autor, Duggan, and Lyle, 2011).[[2]](#footnote-2) How the draft—via these labor market, health, and other potential effects—influences the next generation is not yet understood.

In this paper, we investigate whether and how conscription spills over to future generations. Using the universe of federal tax returns, we ask two questions: (1) does a father’s risk of draft service affect his son’s earnings in the long run? and (2) does risk of draft service today beget voluntary enlistment tomorrow?[[3]](#footnote-3) To answer these questions, we leverage the random assignment of Vietnam draft numbers among men in two key cohorts and compare the labor market outcomes of sons whose fathers were at risk of being selected by the draft with those of sons whose fathers were not. Namely, we first identify 1996 tax-filing men who, based on the year they were born, were subject to the draft lotteries of 1970 and 1971. Following Angrist (1990), we use their exact dates of birth to associate these men with their draft numbers and to determine whether they were therefore at risk of being selected by the draft (i.e., draft eligible). We then link these men to their sons’ outcomes through adulthood and examine how, if at all, a son’s labor market is affected by his father’s draft eligibility.

We generate two main findings, which we anchor within several lines of research. First, we estimate that sons of draft-eligible men fare worse in the labor market down the road. Compared with their peers, they have lower earnings, which holds even conditional on labor force participation (which is also lower). This finding suggests that the negative earnings consequences of the Vietnam draft appear to be more persistent than recent papers, by restricting their focus to draft cohorts, have found. Second, we find that sons of draft-eligible men are more likely to enlist in the military and are generally more wedded to defense work, which bears implications for the persistence of war. In addition, we offer supplemental evidence that our results contribute to the growing literatures on (1) how adverse shocks to parental inputs can reduce children’s later-life outcomes and (2) the intergenerational transmission of occupational choice.

In our analyses, we investigate important dimensions of a son’s labor market that could be affected by his father’s draft eligibility. Our framework relies on comparisons between the outcomes of sons whose fathers were at risk of being drafted and those of sons whose fathers were not. Before we endeavor to make these comparisons, we first demonstrate that there is no evidence of survival or other sample selection bias that would make these two groups incomparable.

We then begin the main analysis by examining son’s outcomes in 2013, when his average age was 31 years. We estimate that a father’s draft eligibility, on average, significantly reduces his son’s earnings, by $257, and labor force participation, by 0.14 percentage point (p.p.). When we disaggregate our labor market outcomes into distinct civilian- and military-specific categories, we estimate that a father’s draft eligibility slightly increases his son’s military earnings and participation, but that these increases are fully eclipsed by large decreases in civilian labor market outcomes.

Next, we examine whether a father’s draft eligibility affects his son’s enlistment behavior more generally (i.e., over the full period we can observe). We estimate that, between 1999 and 2013, sons of draft-eligible fathers are 0.26 p.p. more likely to serve in the military, and serve for about 5.77 days longer, than their peers.[[4]](#footnote-4) Probing further into the nature of a son’s service, we find that a father’s draft eligibility increases the likelihood his son enlists in both active duty (0.25 p.p.) and reserve (0.11 p.p.) service. In addition, we distinguish service during periods of war and peace and recover substantial effects on enlistment during both the War on Terror (beginning in 2001) and the prior (nonwar) period. Altogether, sons of draft-eligible fathers appear to be generally more attached to the military, which extends beyond participation in wars.

Throughout our paper, to aid in the interpretation of our results, we also present illustrative estimates linking a father’s draft service to his son’s labor market. We derive these estimates by scaling our reduced-form coefficients by the extent to which draft eligibility induced military enlistment among the men from the two lottery years we consider.[[5]](#footnote-5) For instance, our earnings estimates suggest that a father’s service lowers his son’s annual earnings by about $2,000 (equivalent to a reduction of more than 5%). In addition, a father’s draft service appears to increase the probability his son volunteers for the military by 2.09 p.p., and the (unconditional) duration of his son’s service by about one-tenth of a year, each representing an increase of more than 25% over baseline participation.

Our rich data set also affords us the ability to examine an array of other outcomes of interest among the sons in our sample, some of which could inform our main estimates. Specifically, we investigate the extent to which we can observe differences in education (e.g., postsecondary attendance, duration, and quality), work experience, mortality, family formation, and disability earnings. Altogether, we find that sons whose fathers were at risk of being drafted tend to have slightly less labor market experience; however, they have very similar broad health and college outcomes (except for a slightly lower likelihood of attending graduate school) in the period we observe. Holding health and college attendance roughly constant, it is perhaps unsurprising that less labor market experience and graduate training are associated with lower earnings.

While we primarily focus on sons, we also present estimates of how draft eligibility affects daughters’ labor market outcomes.[[6]](#footnote-6) Among daughters, the estimated effects on military service are, on average, about one-fourth as large as those among sons, which broadly corresponds to the relative enlistment rates and average durations of service by sex. The effect on their earnings is almost one-third as large and marginally significant, the log earnings estimate similar to the result for sons, and the labor force participation effect is indistinguishable from zero.

We conclude by evaluating the channels underlying our main results. We first propose several mechanisms that could explain why sons of draft-eligible fathers tend to earn less than their peers: (1) inherited traits from lower-skilled mothers made them genetically less able; (2) their increased enlistment rate lowered their earnings; (3) different household environments as children led them to have lower reservation wages or prefer lower-paying sectors; or (4) reduced parental inputs, potentially resulting from lower income or other negative consequences of the Vietnam draft, lowered their general human capital. We provide suggestive evidence that our earnings result is most consistent with reduced parental inputs lowering sons’ earnings trajectories. We then consider two candidate channels that could explain increased military participation among draft-eligible sons: (1) a transmission of occupational preferences or occupation-specific skills and (2) fewer (or lower-paying) civilian labor market opportunities that raise the relative returns to military service. Though the return to military service may be higher for those with fewer opportunities, there also appears to be a role for the transmission of occupational skills or preferences—perhaps due to a culture of military service specific to our context—that is passed down from one generation to another.

Our findings relate to four strands of literature. First, they offer new insight into the *intergenerational* consequences of Vietnam-era conscription, adding a new dimension to our understanding of the legacy of this war,[[7]](#footnote-7) and the intergenerational effects of policy more generally. By focusing on the children of men subject to the draft, our results demonstrate that even though observed labor market and other setbacks to draftees dissipated over time, the negative consequences of conscription persist. These setbacks may have occurred over a formative period for the children of these men and then ultimately penalized their future earnings as well.

Second, our findings point to a strong causal role linking environmentally manipulable parental inputs to children’s earnings trajectories. Thus, we provide evidence that adverse shocks to such inputs can reduce children’s later-life outcomes, contributing to an ongoing debate on whether and how household circumstances affect children’s outcomes (e.g., Sacerdote, 2007; Oreopoulos, Page, and Stevens, 2008; Akee et al., 2010; Duncan, Morris, and Rodrigues, 2011; Milligan and Stabile, 2011; Dahl and Lochner, 2012; Aizer, Eli, Ferrie, and Lleras-Muney, 2014; Dahl, Kostøl, Mogstad, 2014; Cesarini, Lindqvist, Östling, and Wallace, 2015; Manoli and Turner, 2015).[[8]](#footnote-8)

Third, a number of studies have examined the determinants of military service and participation in conflict (Hosek and Peterson, 1985; Kilburn and Klerman, 1999; Blattman and Miguel, 2010; Mann, 2011). These are crucial concerns of today’s military, which relies on voluntary enlistment and must continually replenish its ranks (Segal and Segal, 2004). To date, there is little causal evidence of familial transmission of military service (Kleykamp, 2006; Campante and Yanagizawa-Drott, 2015).[[9]](#footnote-9) The large enlistment increases we detect demonstrate that inducing one generation into military service may make it easier to recruit subsequent generations to join the military. Moreover, our findings hold over periods of war and peace and are therefore consistent with occupational choice as a viable channel for transmission.

Lastly, there is a recent surge of attention within labor economics on intergenerational mobility. Historically, much of this literature has tended to focus on ability and earnings outcomes across socioeconomic groups and on whether specific channels, such as neighborhoods, health, schooling, or transfer programs, level the playing field or deepen the divide (see Black and Devereux (2011) for an extensive review). A new set of papers draws on administrative tax data and documents the degree of mobility and its changes over time (Gee, Gerald, and Turner, 2013; Chetty, Hendren, Kline, and Saez, 2014; Chetty, Hendren, Kline, Saez, and Turner, 2014; Chetty and Hendren, 2015; Mitnik, Bryant, Weber, and Grusky, 2015). Our paper helps inform the extent to which tax policy can address these causes by pinpointing mechanisms that contribute to persistence in earnings across generations. In addition, though some work endeavors to link occupations of parents and their children, there is so far a paucity of causal evidence on how such choices transmit across generations (Dal Bó, Dal Bó, and Snyder, 2009; Hellerstein and Morill, 2011).[[10]](#footnote-10) (Further, there is also little, if any, causal evidence linking a parent’s occupation to a child’s later-life income.) This is largely because professions typically are not randomly assigned, so it is difficult to find a convincing quasi-experimental design. A randomized draft offers unique insight into the transmission of occupation across generations.

The rest of the paper is organized as follows. The next section describes the data and presents validity tests. Section 3 estimates the effects of Vietnam draft eligibility on sons’ earnings and military service outcomes. Section 4 extends the analysis to investigate other down-the-road effects. Section 5 discusses and explores different possible mechanisms behind the results. Section 6 concludes.

1. **Data Description**

Federal income tax records from 1996 and 1999 to 2013 form the basis of our data set. Our first step is to create a data set of the universe of males potentially affected by the Vietnam draft lotteries in 1970 and 1971—namely, those born in 1951 and 1952—using the DM-1 file[[11]](#footnote-11) and assign them their respective draft number and eligibility based on their exact dates of birth. Those subject to the lotteries of 1970 and 1971 faced the prospect of being drafted in 1971 and 1972, respectively. We also repeat the same exercise for those born between 1948 and 1953 to examine robustness across the different draft lotteries, though randomization errors in the 1948–50 cohorts draft lottery, and the lack of a binding draft for the 1953 cohort, lead us to exclude them from our main analysis. We then link these individuals to their tax filings for 1996 (the first year such data is available), including the dependents they claimed on their Form 1040 in that year.[[12]](#footnote-12) Note that because we rely on tax filings to identify father-children links, if there were fathers that did not file a tax return in 1996 (or did not claim their children), they would be missing from our main sample. We test for whether draft eligibility is correlated with this as well as other sample selection concerns at the end of this section. We also show in the appendix that the results are very similar when using a non-tax-filing linkage for a subsample of sons using Kidlink (a database generated from Social Security card applications that require identifying information about parents to receive a Social Security number).

To examine children outcomes, we take advantage of an array of information returns filed with the Internal Revenue Service, available for the period from 1999 to 2013. We construct our measure of earnings as the total amount reported on Forms W-2 and 1099-MISC—filed by employers on behalf of their employees and contractors, respectively, regardless of individual filing decisions. Each of these forms is associated with an individual Employer Identification Number (EIN). Particular EINs can be linked to the military, allowing us to observe whether and how many years of military service occurred. We link each serviceperson to the specific years he was enlisted, which will be useful in delineating wartime and peacetime service.

We also make use of information available on Form 1098-T, which includes tuition charges and scholarships filed by postsecondary institutions on behalf of their students, allowing us to create a full picture of any postsecondary education pursued over the period as well as the level of study (undergraduate or graduate). We observe disability insurance receipt from Form 1099-SSA, military pension receipt from Form 1099-R, unemployment insurance receipt from Form 1099-G, and fertility behavior using Kidlink. Finally, we make use of a crosswalk between birthdates to Vietnam draft eligibility (i.e., random sequence numbers) and, to relate draft eligibility to military participation, a Defense Manpower extract.[[13]](#footnote-13) Generally speaking, in our analysis, “year” refers to a tax year (i.e., the calendar year to which the income, employment, and education returns refer), and “cohort” refers to the birth year of the father.

Each unit of observation in our final analysis sample is the unique son (or daughter) of a tax-filing male in 1996 who belongs to a cohort that was subject to the 1970 or 1971 draft lotteries.[[14]](#footnote-14) There are 2,153,234 sons and 2,071,417 daughters. Each child is associated with a dichotomous labor force participation status—equal to 1 if we observe at least one W-2 or 1099 indicating the presence of labor income in 2013, 0 otherwise—and corresponding earnings in that year. About 83% of sons in the control sample (78% of daughters) worked in 2013, and their average income (including zeros for nonparticipants) was $36,449.49 ($26,026.89 for daughters). Each child is also assigned a dichotomous military participation status—equal to 1 if we observe at least one W-2 from a military EIN, 0 otherwise—and a corresponding duration of service, ranging from 0 to 15 years, reflecting the total number of years for which military-based W-2s were filed. About 8% of sons in the control sample enlist (2% of daughters), and the average duration of service is a half-year (one-tenth year for daughters). Because some fathers have multiple children, there are 2,178,651 unique fathers in our sample. Among fathers, the likelihood that at least one of a father’s sons serves in the military is 10.4%.

Before we turn to our main exercise, we confirm that use of the Vietnam draft lottery does not introduce any sample attrition and selection issues across survival, tax-filing, or child-claiming margins. Table 1 presents evidence that our analysis sample is balanced across draft eligibility. First, we use population records to ascertain that the draft lottery generated comparable numbers of eligible and ineligible men with a valid Social Security number (or other Taxpayer Identification Number). Then, we examine whether attrition from the two groups is about equal by the time we observe them in 1996, at which point the men are around 45 years old. We find no evidence to support survival bias or, because we rely on tax records, selection bias induced by differential rates of filing a return. Next, we confirm that there is no differential probability that a man on each side of the draft appears in our sample (i.e., claims at least one dependent). We then test for selection in the sex of the dependents, the number of dependents, and the number of dependents of each sex. Across all of these tests, we can rule out even small amounts of bias, thus verifying the validity of the analysis that follows.

Finally, in Appendix Table 1, we investigate differences by draft eligibility among fathers in reported total income in 1996; whether they filed as married; and the average income in their zip code. Any observed differences here do not pose validity concerns with our design but will help us contextualize our results. We find some evidence that draft-eligible fathers are marginally more likely to live in slightly lower-income zip codes (0.2%, representing about $70), but no other meaningful differences. This result is consistent with Angrist and Chen (2012), who find few differences between draft-eligible and non-draft-eligible men by the time they were sampled in the 2000 census.

1. **Main Results: Sons’ Earnings and Military Service**

We would like to estimate how a father’s risk of military service affects his son’s labor market and military outcomes in young adulthood. Following Angrist (1990), we exploit the draft lottery, which was randomized within cohorts over birth dates, to generate an exogenous shock to the probability of military service among otherwise comparable men. Our main estimates are thus reduced-form effects of a father’s draft eligibility on his son’s outcomes.

In addition, to aid in the interpretation of these estimates, we separately make use of a Defense Manpower Data Center data set on military service by birth date among men from these same cohorts. We sometimes scale our reduced-form estimates by these “first stage” estimates to derive illustrative Wald estimates of the effect of service.[[15]](#footnote-15) We first offer a brief description of these first-stage estimates and then spend the remainder of this section characterizing how a father’s draft eligibility affects his son’s outcomes.

1. **Father’s Military Service Outcomes**

In Appendix 1, we derive a correspondence between father’s draft eligibility and service, and we estimate that the draft induced service by 11.7 and 13.7 p.p. in the 1951 and 1952 cohorts, respectively, for a combined effect of 12.7 p.p. (Appendix Table 2). While we focus our discussion on the reduced-form estimates of draft eligibility, we also scale each of our main reduced-form estimates by this correspondence to derive a Wald estimate, which we present in the second-to-last row of each panel in our tables. In each case, this coefficient represents the change in the outcome attributable to the father’s (draft-induced) service (or any byproduct thereof). Because these first-stage estimates are constructed from a sample of all men in these cohorts, rather than the sample of fathers, our Wald estimates may not be scaled properly if the effect on military service is different for the fathers in our sample (i.e., those who filed and claimed a dependent in 1996) than for the overall population.[[16]](#footnote-16)

Moreover, for these estimates to represent the causal effect of service, it must be the case that a son’s income (or labor force participation or enlistment behavior) is correlated with his father’s Vietnam draft eligibility only through its effect on his father’s service in the military. Thus, one natural concern is that drafted fathers may have gone to great lengths to avoid service, including, for example, deferring their service call by enrolling in educational institutions. Such behavior might lead to differential human capital investment by draft eligibility, which could in turn affect their children’s outcomes and likely dampen our earnings estimate. For the cohorts we consider, this particular type of avoidance may have been relatively minimal (Card and Lemieux, 2001; Angrist and Krueger, 1992b). Nonetheless, because the draft was randomized, draft avoidance poses no threat to our primary strategy, but rather in the interpretation of our results as the byproduct of a father’s service. The scaled estimates presented should be considered suggestive in nature, and our reduced-form estimates remain independently interpretable as the relationship between draft eligibility and our outcomes. In the text, unless explicitly noted, we discuss reduced-form estimates, which can be multiplied by approximately 7.87 to yield illustrative Wald estimates.

1. **Labor Market Outcomes in 2013**

We begin by examining whether a father’s draft eligibility broadly affects his son’s employment and earnings. We estimate that

*ys,c = β0 + β1 \* eligibles,c + γc + εs,c* (1)

over our sample of sons, where *y* is one of three labor market outcomes in 2013—(1) a continuous measure of the gross (pre-tax) income the son earned,[[17]](#footnote-17) (2) the log-transformed value of these earnings, or (3) a dummy variable indicating that he worked (i.e., he had positive income)—and *eligible* is an indicator for his father’s draft eligibility, derived from whether his birth date corresponds to a draft lottery number at or below the draft-eligibility threshold (i.e., men with draft numbers above this cutoff were not at risk of conscription). Our specification pools the sons across the two draft cohorts we consider and therefore includes a cohort effect, *γc,* because the draft lottery was run separately within each cohort. Allowing for a cohort effect also nets out any cohort-specific fluctuations in our measures. Thus, *β1* is an estimate of the causal effect of having a draft-eligible father on a son’s earnings or labor force participation. Because these lotteries were randomized, we will recover unbiased estimates of the effect of father draft eligibility. Nonetheless, as we show in the appendix, the results are extremely similar with additional control variables. Standard errors are clustered on father’s birth date.

In 2013, sons of draft-eligible fathers earned $257.21 less than sons of draft-ineligible fathers, lowering their income by 0.71% of the mean (Table 2, left column). This figure reflects two negative labor market outcomes for these men: first, they were 0.14 p.p. less likely to work in 2013, and, second, conditional on working, their log earnings were 0.6% lower than untreated sons (i.e., sons whose fathers were not draft-eligible). The level earnings result is significant at 1% (*p* = 0.001); the participation and log-transformed results are significant at 5% (*p* = 0.043 and *p* = 0.012, respectively).

The scaled estimates suggest that having a father who served lowered a son’s earnings in young adulthood by more than $2,000 (a 5.58% reduction), labor market participation by 1.4 p.p., and conditional earnings by 4.8%. The average age of a son in our sample is 31 years in 2013. Income at this age is highly correlated with the lifetime earnings profile, so the 5 to 6% differential is likely to reflect close to the difference in the full earnings trajectories between treated and untreated sons in our sample (Mincer, 1974; Murphy and Welch, 1990).

Next, we consider whether these measures mask heterogeneity brought on by the son’s sector of employment (Table 2, middle and right columns). Using employer identifiers from the W-2 and 1099-MISC data, we disaggregate our measures into distinct civilian- and military-specific outcomes and find that, to some extent, this is the case: a father’s draft eligibility slightly increased his son’s 2013 military earnings ($37.82) and participation (0.14 p.p.), but these increases are fully eclipsed in our aggregate measures by large decreases on the civilian side of the labor market ($295.03 and 0.21 p.p., respectively). Further, conditional on working in the sector, civilian earnings are a statistically significant 0.78% lower among treated sons, but the military earnings differential is indistinguishable from zero. We examine military participation further in the next table.

1. **Military Outcomes from 1999 to 2013**

To probe how a father’s draft eligibility affects the likelihood his son serves in the military more generally, we re-estimate equation (1) and vary how we define *y*—now, either (1) a dummy variable indicating at least one year of observed military participation between 1999 and 2013 or (2) an integer-valued variable ranging from 0 to 15, indicating the number of years the son served in the military.[[18]](#footnote-18),[[19]](#footnote-19)

Sons of draft-eligible fathers are 0.26 p.p. more likely to serve in the military than sons of non-draft-eligible fathers (Table 3, left column). Moreover, sons born to draft-eligible fathers serve for about 6 days longer than sons born to non-draft-eligible fathers (Table 3, right column). Results are statistically significant at 1% (*p* < 0.001).

Further, the Wald estimates suggest that a father’s Vietnam-era service increases the probability his son voluntarily enlists by 2.09 p.p. and increases the (unconditional) duration of his son’s service by 45 days. Putting these numbers in context, among sons of non-draft-eligible fathers, about 8% enlist, and the average duration of service (including those who do not serve) is a half-year. In other words, the scaled results suggest a father’s service increases his son’s military service by about 25%.

Because we are interested in the mechanisms underlying the transmission of military service from fathers to sons, we next probe the nature of a son’s military service. Again using equation (1), we first examine draft-induced changes in enlistment according to type of duty (active or reserve). We estimate that sons of draft-eligible fathers are more likely to enlist as both active and reserve military personnel, though the increase we detect is larger for active duty (0.25 p.p.) than reserve (0.11 p.p.) (Table 4). While these magnitudes are statistically different from one another, the increases relative to baseline participation rates in active and reserve duty are near equal—5.32% and 4.20%, respectively—suggesting that military service transmits in a rather general way from fathers to sons.

In the appendix, we offer additional insight into the nature of transmission by separately examining whether treated sons are more likely to serve in particular military branches (i.e., the Army, Navy, and Air Force) and work in the civilian defense sector (i.e., the Department of Veterans Affairs (VA) and the Department of Defense (DOD)).[[20]](#footnote-20) Draft-eligible fathers induce sons’ service within each branch we consider, ranging from 0.05 p.p. in the Navy to 0.14 p.p. in the Army. We estimate that treated sons are also a bit more likely (0.03 p.p.) to hold a civilian military position in the VA or DOD than untreated sons. It appears that treated sons are generally more attached to national defense work.

Our setting also enables us to probe sons’ service during periods of war and peace by comparing the participation effect recovered during the War on Terror (beginning in 2001) with the effect during the prior (nonwar) period. We estimate a reduced-form peacetime coefficient of about one-tenth of 1 p.p. (translating to a 1 p.p. effect of fathers’ service) and a wartime coefficient of two-tenths of 1 p.p. (translating to a 2 p.p. effect of fathers’ service) (Table 4, bottom panel). Statistically, these results do not differ. In fact, our peacetime estimate represents a larger percent increase from the mean than the wartime estimate. (The service rate among the sons of non-draft-eligible fathers is 3% during the peace period, compared with 7% during the war period.) Altogether, we surmise that occupational choice is at least partially responsible for the higher enlistment rates we obtain in our main results.[[21]](#footnote-21)

1. **Labor Force and Military Outcomes among Daughters**

Table 5 re-estimates our main outcomes among daughters. First note that the earnings effect we obtain for daughters is almost one-third the size of sons’ and marginally significant (*p* = 0.065). In addition, the labor force participation effect is indistinguishable from zero. When we consider log earnings, which omits nonparticipants, the estimate for daughters (0.6% lower, *p* = 0.022) is similar to the finding for sons. On the whole, we read the sum of evidence to suggest that (1) fathers’ draft eligibility is driving more marginally attached sons out of the labor market entirely and (2) among generally more labor market attached children, the draft consistently lowers earnings around 0.6%.[[22]](#footnote-22)

Turning to the service results, fathers’ draft eligibility, on average, increases daughters’ military outcomes—participation by less than 1 p.p. and duration by two days. Both results are statistically significant at 1% (*p* = 0.003). While these estimates are substantially smaller than those we recovered among sons, they appear to mostly reflect differences in military participation between the two sexes. Specifically, average enlistment rates and years of service among untreated daughters are each about 20% of those of untreated sons. Thus, even though the estimated enlistment boost among daughters is about 20% as large as that among sons, relative to the mean, draft eligibility induces a similar percentage increase, both nominally and statistically, across sexes. Further, while the coefficient on duration is closer in magnitude (30% the size) between the two sexes, the mean-scaled difference (i.e., 1.5 times) is not statistically significant. In Appendix Table 3, we present the results from pooling the sons and daughters samples. Unsurprisingly, the results are extremely significant at 1%, with the exception of labor force participation.[[23]](#footnote-23)

1. **Robustness and Heterogeneity**

For both our labor market and military outcomes, we present additional tables in the appendix that examine the robustness of our results and probe additional questions of interest that arise from what we have shown in the main text. In the remainder of this section, we briefly describe our findings.

First, in Appendix Tables 5 and 6, we demonstrate we can reproduce our earnings and enlistment estimates for several alternative samples and specifications. In columns (1) and (2) of each table, we show these results are driven by the sons of citizens (which is consistent with draft rules). In column (3), we include duplicate dependents (i.e., sons also claimed by other fathers), whom we had excluded from our main analysis sample. In column (4), we add fathers’ birth month-year and state-of-birth fixed effects, following specifications in Angrist and Chen (2011), and, in column (5), we also include fixed effects for the son’s age. In addition, we present in the final column alternative functional forms of our estimating equation, reflecting the range of values that the dependent variables can assume (i.e., probit for binary outcomes and Poisson for count outcomes), as well as the results for earnings winsorized at the 99.99th percentile and raw earnings. Across all of these robustness checks, our estimates are extremely similar to those presented in the main text.

Turning to Appendix Table 7, we present two-sample instrumental variable results for all of our main outcomes, where we separately estimate in the Defense Manpower Data Center data set the probability a father’s draft eligibility induces his service, and we estimate the effect of that predicted probability on his son’s outcomes. We consider four specifications, in which we derive variation from (1) draft eligibility alone; (2) draft eligibility interacted with year of the draft; (3) five draft lottery group bins (so as to exploit within-eligibility changes in the probability of enlistment, following Angrist and Chen (2011) and Angrist, Chen, and Frandsen (2010));[[24]](#footnote-24) and (4) within-eligibility changes in the probability of enlistment interacted with year of the draft. The results are generally similar to our illustrative Wald estimates.

While the average age in our son sample is 31 years in 2013, the ages of the children vary, so for a full exposition of the results, we re-estimate our equations separately by single age and exploit all years of data available (1999–2013). Figures 1a and 1b plot the earnings (in 2013 dollars) and enlistment estimates by son’s age, together with the 95% confidence band surrounding each estimate. The earnings results are unsurprisingly insignificant in the late teens and early 20s. Only beginning in the mid-20s do we see an effect on earnings, which appears to grow as age increases. The military effect, on the other hand, appears beginning in young adulthood and is relatively constant across ages. It is clear from these figures that the results are not specific to any age.

In Appendix Table 8, we re-estimate earnings and enlistment effects among the main birth cohorts affected by the 1969 draft lottery (1948–50 cohorts) and the 1972 draft lottery (1953 cohort, though no one was ultimately drafted), as presented, for example, in Angrist and Chen (2011). Our estimates, where they are indistinguishable from zero, are directionally similar no matter the cohort. Generally, the larger the first stage, the more positive the effect on military service and the more negative the effect on earnings. Altogether, this array of results lends credence to our main conclusions.

Though our results are population-level causal estimates of the effect of the draft on children claimed by their fathers in 1996, there are some potential limitations to generalizing our findings beyond our sample. First, we cannot directly speak to the effect on sons born too early to be claimed in tax year 1996, e.g., 1970. Tabulating from the Statistics of Income 1987–96 Family Panel, which is a representative panel of returns filed in those years, suggests that approximately one-third of children from the 1951–52 birth cohorts were born too early to be claimed by their fathers in 1996.[[25]](#footnote-25) To explore this issue further, we test in Appendix Table 9 whether there is heterogeneity in the effects on earnings and military service by year of birth among sons we can observe. We find that the directions of the interaction between year of birth and father’s draft eligibility are consistent with decreased earnings and increased military service among those born earlier, though the results are mostly not significant. Note that this set of results would suggest that the effects we detect likely represent a lower bound. In other words, were we to examine all sons born to these fathers, we would likely find effects no smaller than we find for the children in our sample.

The other potential limitation to generalizability is that the children of fathers who did not file their taxes in 1996 are excluded from our main sample (though nonfiling was shown to be random to draft-eligibility status). We investigate this issue by taking advantage of Kidlink, which facilities the linkage of a subsample of children (i.e., those born beginning in 1983) to fathers without relying on tax filing, to produce an estimate of how many children might be missing. The exercise indicates that approximately only 5% are missing. In addition, in Appendix Table 10, we explore whether there is a difference between our main estimates when we restrict our main sample to those born in 1983 or later and those same earnings and military estimates constructed from a Kidlink-based linkage.[[26]](#footnote-26) Across all outcomes, the difference is always statistically and economically insignificant, suggesting nonfilers are not affecting the generalizability of our results.

We also consider other military outcomes of interest in Appendix Table 11, which are discussed in more detail in Appendix 2. In the first column, we present estimates of the years of military service, conditional on serving. This effect is indistinguishable from zero. In the right three columns, we consider an alternative model of transmission in which a son’s voluntary enlistment in the military could be the result of a cooperatively-made household decision. A convenient story is one of primogenitary inheritance: if a father feels strong duty to his country as a result of his own service, perhaps he imparts that duty only to his eldest son. Varying the unit of analysis to the father, draft-eligible fathers are about 0.3 p.p. more likely to have a son who enlists in the military. (The estimate ranges from 0.2 to 0.3 p.p. depending on the number of sons he has.) About 10% of families enlist at least one son, so altering the nature of transmission does not appear to materially change our estimate.

Finally, we investigate heterogeneity in effects by parental income (measured in1996) and sons’ earnings (measured in 2013).[[27]](#footnote-27) First, we probe in Appendix Table 12 the extent to which particular parent-income groups drive our estimates.[[28]](#footnote-28) Interestingly, the earnings effect is apparent for essentially all but the highest income group, whereas the military effect is apparent for essentially all but the lowest income group. When we flip the question around and probe whether sons are differentially likely to earn above different earnings percentiles (Appendix Table 13), we find that treated sons are worse off no matter the income cutoff, indicating that the decrease is not particularly concentrated in any part of the earnings distribution.

1. **Sons’ Work Experience, Health, and Education**

We wish to better understand the disparities in treated and untreated sons’ realized labor market outcomes we uncovered in the previous section. It may be that systematic differences in human capital accumulation between the two groups underlie these disparities. In this section, we follow the same estimation framework as before but examine intermediate outcomes that we can observe in our data. Systematic differences in these outcomes could help explain why a father’s higher risk of being drafted into the military, on average, translates into lower earnings and increased enlistment for his son.

First, we investigate whether a father’s draft eligibility translated into less work experience, overall and specifically within the civilian labor market, by the time we observe him in 2013. Our outcome is an integer-valued variable, reflecting years of positive earnings in each category. It is constructed over the period from 1999 to 2012 so that it ranges in value from 0 to 14. On average, a father’s eligibility induces about 8 fewer days of work experience, suggesting that his service induces about 63 fewer days (2% below mean experience) (Table 6, first column). These estimates are a bit higher when we consider civilian work experience alone (Table 6, second column). It is notable that the hit to prior civilian work experience is larger than the increased time in the military. Thus, we see that extra days in the military are more than fully eclipsed by fewer days in the civilian labor force, resulting in less labor market experience overall for these men. Treated sons are generally less attached to the workforce, which may be cumulatively affecting their 2013 labor outcomes (in addition to underlying mechanisms driving all sets of outcomes).

In the final three columns of Table 6, we examine differences in postsecondary training. Angrist and Chen (2011) studied the effect of Vietnam military service driven by draft eligibility on the men’s own educational levels and found an increase of about 0.27 years of college, which they attributed to GI Bill benefits. As a result, all else equal, we would expect an increase in education among their children if there is an intergenerational transmission of education. However, the negative earnings effect on sons documented earlier would make a negative effect unsurprising. To investigate these outcomes, we derive three measures of a son’s education from information returns filed by all institutions on behalf of their attendees as they pertain to sons in our sample: (1) integer-valued years of postsecondary attendance, representing the number of years that at least one Form 1098-T was filed and ranging from 0 to 15; (2) a dummy variable indicating that at least one Form 1098-T was filed pertaining to undergraduate training; and (3) a dummy variable indicating that at least one Form 1098-T was filed pertaining to graduate training. We detect no systematic differences in the first three outcomes, so the differences in earnings are apparently not driven by lower college attendance rates or cumulative time spent on education, nor is the lower prior work experience among treated sons a byproduct of more time spent in school. However, we estimate a marginally statistically significant decrease in graduate training, implying that sons of servicemen are, on average, about 7% less likely to attend graduate school. This finding raises additional questions regarding the fewer years they spend in the labor market, while it could help partially explain some of the differences in earnings we estimate for 2013.

We probe several additional questions in the appendix that supplement our findings in this section. In the top row of Appendix Table 14, we investigate other intermediate outcomes. In the first two rows, we show that there is no difference in whether treated sons are in school in 2013 (which could mechanically lower labor market outcomes) or in the average quality of the postsecondary schools they attended over the full sample period (as measured by the log average 2013 earnings of the other attendees of each school, weighted by the time spent at each school). The last two columns in the top panel probe health outcomes to the extent this is feasible with our data. Specifically, we investigate whether sons are differentially likely to receive any income from the Social Security Disability Insurance program or be alive by 2013. Neither of these estimates is distinguishable from zero, indicating there are no differences in health capital, at least in these rather extreme measures, that would translate into systematic differences in labor market outcomes. Finally, we find no evidence that sons are more or less likely to receive unemployment insurance payments in 2013.

Then, in the bottom row, we examine differences in family structure and the transition to college. First, using the Kidlink database, we explore the fatherhood outcomes of the sons sample. We find that treated sons are statistically no more likely to have more children than untreated sons. However, draft-eligible sons have about 4% more children as teenagers. (About 0.016 children were born while the untreated son was a teenager.) Teenage fatherhood represents a substantial shock during a critical period of human capital investment, so perhaps it is no surprise that given a rockier start, we detect fewer years of work experience and, ultimately, lower earnings down the road among this group. In addition, we investigate differences in each group’s college attendance rate at age 19 to probe whether the postsecondary paths began as similarly as they ended. We find a small gap in attendance rates, which, as was shown earlier, is erased by 2013. The size of the gap—0.23 p.p.—more than offsets the military enlistment bump at the same age in Figure 1b. This result suggests that, at early stages of young adulthood, differential rates of human capital investment may be contributing to the later-life differences we observe.

1. **Discussion of Mechanisms and Interpretation**

In Appendix 3, we explore possible mechanisms that explain our main results. This exploration is suggestive in nature. The purpose is to identify which mechanisms, of those available, are most consistent with the data, but we cannot determine with certainty which hypotheses are correct. In the appendix, we consider each of the explanations we present here in turn, develop some suggestive evidence, and then assess the extent to which they are operating. This section summarizes our analyses and corresponding findings.

We first investigate several mechanisms that could explain why sons of draft-eligible fathers tend to earn less than their peers: (1) inherited traits from lower-skilled mothers made them genetically less able; (2) their increased enlistment rate lowered their earnings; (3) different household environments as children led them to have lower reservation wages or prefer lower-paying sectors; or (4) lower parental inputs reduced their general human capital. Note that these theories are not mutually exclusive; for instance, parental inputs broadly defined could also drive military participation (#2) and a preference for lower-paying sectors (#3). To the extent possible, we attempt to analyze each explanation in isolation so that, in this example, explanation 4 is the effect of parental inputs exclusive of any transmission of preferences and/or job-specific human capital that underlie explanation 2 or 3.

We reach several conclusions. First, we cannot find any evidence consistent with a genetic explanation: mothers’ earnings and labor force participation by draft-eligibility status are nearly identical, as shown in Appendix Table 15. Second, the increased enlistment rate cannot explain much of the lost earnings, because, taken alone, it would imply that military service reduces annual earnings by an implausibly large amount, on the order of $100,000 and close to three times the mean earnings of draft-ineligible sons. Third, neither a lower reservation wage nor a preference for lower-paying work appears to be driving earnings down. In the main text, we had found reduced labor force participation and earnings conditional on working, which are inconsistent with lower (or higher) reservation wages. In Appendix Table 16, we do not find much evidence that treated sons differentially sort into lower-paying industries. By elimination, the evidence suggests that reduced parental inputs are a key mechanism for our earnings result. While the children we study were too young to be directly affected by their father’s risk of draft service (very few children claimed in 1996 could have been born before the Vietnam War was over), prior literature has documented lower resources in the decades after the war, increased behavioral health disorders, and various other setbacks among draft-eligible men that may have indirectly impaired their sons’ human capital development.

We next examine two candidate channels that could explain increased military participation among draft-eligible sons: (1) occupational preferences or occupation-specific skills are transmitted across generations,[[29]](#footnote-29) and (2) fewer (or lower-paying) civilian labor market opportunities raise the relative returns to military service.[[30]](#footnote-30) Because of our earnings results (as well as prior literature documenting lower earnings among draft-eligible men in the 1970s and 1980s) and what is conventionally presumed to drive enlistment decisions (Hosek and Peterson, 1985; Kilburn and Klerman, 1999; Kleykamp, 2006), we view the second explanation as the default channel. As a result, our focus is on whether this channel is operating alone or whether the first could explain any of the effect we detect. We stress that, no matter the cause, our main estimates show that conscription in one generation heavily drives voluntary enlistment in the next, which, in and of itself, is a novel result.

First, the effect of a father’s draft eligibility on earnings relative to the control group mean is -0.7%, while for military enlistment it is 3.4%. If the story were fully one of fewer labor market opportunities, these estimates would suggest there is a large negative elasticity between earnings opportunities and enlistment. To assess the plausibility of the implied elasticity, we undertake several back-of-the-envelope calculations that relate enlistment probabilities to sons’ opportunities, using only the sons of non-draft-eligible fathers. No matter how we approximate “opportunities”—1996 family income, average family income over the entirety of childhood, or 2013 sons’ earnings themselves—these suggestive exercises deliver estimates orders of magnitude below what would likely be necessary to explain much of the result (Appendix Table 18). Second, the set of earnings and enlistment estimates across the family-income distribution suggests that service and labor market opportunities operate at least somewhat separately: sons at the very top of the distribution experienced no (opposite signed) earnings reduction but were more likely to enlist, whereas sons at the very bottom experienced no (opposite signed) enlistment increase but an earnings reduction. Third, if treated sons have a preference to enlist relative to untreated sons, we would expect to see that during times of economic distress—when economic opportunities are more likely to be driving the decision to enlist—the relative gap in participation between the two groups would shrink. Consistent with this expectation, our relative estimate for the effect of draft eligibility on military service during the Great Recession is statistically significantly lower than during other years in our sample (Appendix Table 19). Finally, the extent to which different military branches drafted servicemen was uneven across cohorts. We attempt to exploit this variation, investigating whether a son’s branch of service systematically varies with his father’s likely branch. We find some support for a transmission of branch in the data (Appendix Table 20). We take the sum of this evidence to imply that, in addition to whatever reduced labor market opportunities that sons of drafted fathers likely experienced relative to their peers, there is a separately operating transmission of preferences (or skills) that explains their increased likelihood to serve in the military.

1. **Conclusion**

Evidently, the effects of the Vietnam draft persisted into the next generation. In this study, we have uncovered two facts regarding the sons of draft-eligible fathers: (1) they earned over $250 less in 2013, when they were, on average, 31 years old; and (2) they were nearly 3.5% more likely to serve in the military between 1999 and 2013. The earnings losses are due not only to reduced labor force participation (0.14 p.p.), but also to lower income, conditional on working (0.6 p.p.). These results survive numerous robustness tests. Moreover, similar but smaller effects are detected among daughters. In the remainder of the paper, we set out to better understand why randomly increasing the risk of individuals’ wartime service would translate into such differential outcomes for their sons 40 years later. We show that there are differences that may be contributing to these effects—namely, reduced labor market experience and slightly lower educational attainment—and we conclude by investigating the various channels that may give rise to our findings.

The reduction in sons’ earnings appears to derive from diminished human capital development owing to any number of already documented (or as yet uncovered) consequences of risk of Vietnam-era service—such as suppressed earnings or increased mental health issues—affecting fathers during their sons’ formative years. This finding highlights the strong role that environmental factors play in determining children’s long-term outcomes. The size of our effect is considerable, and, given both the average age at which we observe them in 2013 (generally past the “overtaking age” of Mincer (1974)) and the consistently increasing effects by age we document, these earnings differentials will likely extend over their full earnings trajectory. It remains to be seen whether the persistent differences for these families will in turn hinder the earnings outcomes of future generations.

These findings also offer new causal evidence that an occupation can be transmitted across generations, even in a setting in which one might suspect transmission to be low (e.g., randomly assigned risk of service in an unpopular war). Our estimates suggest that for every 385 men at risk of conscription—or for every 50 Vietnam-era servicemen induced by the draft—we can expect one extra voluntary enlistee down the road. All else equal and assuming no spillovers, had all of the fathers in just these two cohorts been induced to serve, we would anticipate over 45,000 voluntary male enlistees owing to conscription alone. (For comparison, there were 166,000 sons who served in the military from these cohorts.) Moreover, since conscription compels people into service in spite of better outside options, the 2 p.p. increase in the likelihood a son enlists that we detect might represent a lower-bound estimate of transmission in other settings.

Most generally, family appears to have played important roles in both the transmission of occupational preferences (or in the acquisition of occupation-specific human capital) and the development of long-term human capital in our setting, such that we might expect the historically determined differences we detect to persist for some time (Altonji and Blank, 1999). Thus, our results speak more broadly to socioeconomic mobility and different mechanisms by which inequities may persist across generations. Future work should investigate the key manipulable forces that underlie our results as well as the generalizability of our estimates of occupational transmission to other settings.

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**Table 1.** *Effect of draft eligibility on attrition and selection*

|  |  |  |
| --- | --- | --- |
|  | Coefficient (SE) on draft eligibility |  |
| Panel A: Survival and Filing in 1996 |  |  |  |
| Outcome | (1)Counts by DOB | (2)Ln(Counts by DOB) | (3)Alive in 1996 | (4)File in 1996 | (5)Claim dependent in 1996 |
|  | -20.37(27.04) | -0.0034(0.0046) | -0.00002(0.00011) | 0.0003(0.0008) | 0.0011(0.0007) |
| Panel B: Dependents in 1996 |  |
| Outcome |  (6)Number claimed | (7)Any son | (8)Number of sons | (9)Any daughter | (10)Number of daughters |
|  | 0.0011(0.0016) | 0.0008(0.0006) | 0.0002(0.0010) | 0.0010(0.0063) | 0.0009(0.0010) |

Notes: The table presents estimates from regressions of various sample attrition and selection outcomes on a dummy variable indicating whether the individual was Vietnam draft eligible (based on his date of birth) and a dummy variable for the individual’s year of birth. The sample is limited to all men born between 1951 and 1952 with a Social Security number (SSN) (or Taxpayer Identification Number). Columns (1) and (2) examine whether there is an imbalance in the number of men with a valid SSN, with each cell aggregated to the date of birth (DOB) level. All other columns relate to 1996 because the link of fathers to their children is performed using 1996 tax returns (the first year this information is available). Standard errors are clustered at the father date of birth level, except for the aggregated count outcomes, where only robust standard errors are reported. *N* = 4,303,632 except in columns (1) and (2), where *N* = 731. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Table 2.** *Effect of father’s draft eligibility on son’s 2013 earnings and work decisions*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Category |  (1)Overall |  (2)Civilian |  (3)Military |
| Panel A: Earnings |  |  |  |
| Reduced form | -257.21(79.44)\*\*\* | -295.03(88.05)\*\*\* | 37.82(9.85)\*\*\* |
| Wald estimate | -2032.42 | -2231.25 | 298.86 |
| Control group mean | 36,449.49 | 35,579.65 | 885.33 |
| Panel B: Any job (earnings > 0) |  |  |  |
| Reduced form | -0.0014(0.0007)\*\* | -0.0021(0.0007)\*\*\* | 0.0014(0.0003)\*\*\* |
| Wald estimate | -0.0110 | -0.0170 | 0.0115 |
| Control group mean | 0.8251 | 0.8063 | 0.0320 |
| Panel C: Ln(earnings) |  |  |  |
| Reduced form | -0.0060(0.0024)\*\* | -0.0078(0.0024)\*\*\* | -0.0019(0.0097) |
| Wald estimate | -0.0475 | -0.0617 | -0.0151 |
| Control group mean | 40,334.42 | 44,112.33 | 27,564.17 |

Notes: The table presents estimates from regressions of son earnings and job outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. Control group means are derived from sons of non-draft-eligible fathers. Panel C control group means are constructed by taking the mean of all positive values before the log transformation. The Wald estimate is illustrative, scaling the draft eligibility estimate by the effect of draft eligibility on military enlistment among men born between 1951 and 1952. Standard errors are clustered at the father date of birth. Earnings are in 2013 dollars. *N* = 2,153,234 except in panel C, where the sample sizes in columns (1)–(3) are 1,775,718, 1,734,772, and 69,797, respectively. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Table 3.** *Effect of father’s draft eligibility on son’s military service (1999–2013)*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Outcome | (1)Served in the military | (2)Years of military service |
| Reduced form | 0.0026 (0.0004)\*\*\*0.02090.0765 | 0.0158(0.0032)\*\*\*0.12450.4878 |
|  |
| Wald estimateControl group mean |

Notes: The table presents estimates from regressions of son military service on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. Control group means are derived from sons of non-draft-eligible fathers. The Wald estimate is illustrative, scaling the draft-eligibility estimates by the effect of draft eligibility on military enlistment among men born between 1951 and 1952. Standard errors are clustered at the father date of birth level. *N* = 2,153,234. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Table 4.** *Effect of father’s draft eligibility on the nature of son’s military service (1999–2013)*

|  |  |  |
| --- | --- | --- |
|  | Coefficient (SE) on draft eligibility |  |
| Outcome |  (1)Activeduty | (2)Reserveduty | (3)Not during wartime (1999–2000) | (4)During wartime(2001–13) |
| Reduced form | 0.0025(0.0003)\*\*\* | 0.0011(0.0002)\*\*\* | 0.0013 (0.0002)\*\*\* | 0.0024(0.0004)\*\*\* |
| Wald estimate | 0.0194 | 0.0086 | 0.0100 | 0.0190 |
| Control group mean | 0.0470 | 0.0262 | 0.0259 | 0.0725 |

Notes: The table presents estimates from regressions of son military service on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. Control group means are derived from sons of non-draft-eligible fathers. The Wald estimate is illustrative, scaling the draft eligibility estimates by the effect of draft eligibility on military enlistment among men born between 1951 and 1952. Standard errors are clustered at the father date of birth level. *N* = 2,153,234. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Table 5.** *Effect of father’s draft eligibility on daughter’s outcomes*

|  |  |  |
| --- | --- | --- |
|  | Coefficient (SE) on draft eligibility |  |
| Outcome | (1)Earnings | (2)Any job | (3)Ln(earnings) |  (4)Any military service | (5)Years of military service |
| Reduced form | -93.00(50.29)\* | 0.0005(0.0007) | -0.0055(0.0024)\*\* | 0.0006(0.0002)\*\*\* | 0.0044(0.0014)\*\*\* |
| Wald estimate | -734.84 | 0.0041 | -0.0435 | 0.0044 | 0.0349 |
| Control group mean | 26,023.46 | 0.7768 | 33,498.72 | 0.0154 | 0.0890 |

Notes: The table presents estimates from regressions of daughter outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The sample comprises female dependents claimed on 1996 tax returns by men born between 1951 and 1952. Control group means are derived from daughters of non-draft-eligible fathers. The column (3) mean is constructed by taking the mean of all positive values before the log transformation. The Wald estimate is illustrative, scaling the draft eligibility estimate by the effect of draft eligibility on military enlistment among men born between 1951 and 1952. Earnings are in 2013 dollars. Standard errors are clustered at the father date of birth level. *N* = 2,071,417 except in column (3), where *N* = 1,609,418. \*\*\* denotes *p*< 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Table 6.** *Effect of father’s draft eligibility on son’s work experience and college attendance*

|  |  |  |
| --- | --- | --- |
|  | Coefficient (SE) on draft eligibility |  |
| Outcome | (1)Years of prior work exp. (1999–2012) | (2)Years of prior civilian work exp. (1999–2012) | (3)Years of postsecondary (1999–2013) | (4)Any undergrad. school (1999–2013) | (5)Any graduate school (1999–2013) |
| Reduced form | -0.0220(0.0093)\*\* | -0.0291 (0.0096)\*\*\* | 0.0001(0.0058) | 0.0002(0.0009) | -0.0012(0.0006)\* |
| Wald estimate | -0.1735 | -0.2302 | 0.0010 | 0.0020 | -0.0094 |
| Control group mean | 9.8327 | 9.5806 | 3.415 | 0.7021 | 0.1430 |

Notes: The table presents estimates from regressions of other son outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. Years of experience are calculated as the number years the son received either a Form W-2 or 1099-MISC (for nonemployee compensation), while college attendance is calculated as the number of years the son received a Form 1098-T (or whether one was received for undergraduate or graduate school). Control group means are derived from sons of non-draft-eligible fathers. The Wald estimate is illustrative, scaling the draft-eligibility estimate by the effect of draft eligibility on military enlistment among men born between 1951 and 1952. Standard errors are clustered at the father date of birth level. *N* = 2,153,234. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10. \*\*\* denotes *p* < 0.01.

**Figure 1a and Figure 1b: Effect of father’s draft eligibility on son’s earnings (1a) and military service (1b) at various ages (1999–2013)**





Notes: Figure 1a (1b) presents the point estimates and 95% confidence interval (CI) from regressions of earnings (military service) on father’s draft eligibility and father’s year of birth. Each regression uses all observations between 1999 and 2013 when the son was a given age; the regressions are therefore unbalanced. Sample sizes range from 277,780 to 1,795,169. Standard errors are clustered at the father date of birth level.

**Appendix 1: Effect of Draft Eligibility on Vietnam-Era Military Service of 1951–52 Cohorts**

To aid in the interpretation of our reduced-form results on the effect of a father’s draft eligibility on his son’s outcomes, we wish to identify the effect of draft eligibility on draft cohort military service. To do so, we use information made available on Josh Angrist’s website to infer probabilistic transitions from draft lottery numbers (in bins of five) to military service. Specifically, we combine sample code (Angrist1990\_Table2DMDC.do) and two data sets used in Angrist (1990).[[31]](#footnote-31) The data are derived from Defense Manpower Data Center Administrative Records and the Social Security Administration Continuous Work History Sample (dmdcdat.dta and cwhsa.dta, respectively) and contain service and at-risk-of-draft counts organized into 73 lottery number cells per year, with five birth dates in each cell. (Angrist decomposes his counts by race, but, since we do not observe race in our data, we aggregate the service and draft figures across race.) Each cell is associated with a draft-eligibility indicator, using the thresholds available in his paper, and a service rate. We then estimate that

*servicec = β0 + β1 \* eligiblec + εs*

over 73 lottery number cells *c*, separately for each year, and then overall (by stacking these panels and adding a cohort fixed effect). For the 1951 and 1952 cohorts, we estimate that the draft induces service by 11.7 p.p. and 13.7 p.p., respectively, and then by 12.7 p.p. overall (Appendix Table 2). (About 9% of the at-risk cohorts who were not drafted served, so drafted cohorts were over 2.3 times as likely to serve as their peers.) In the text, we scale the reduced-form effects of father’s draft eligibility by these estimates to derive the illustrative effects scaled by father’s service. We do not observe the military service of fathers from the 1970s in tax data; therefore, to the extent the effect on service is different for fathers in our sample relative to men overall (as these data measure), these estimates may not be scaled appropriately. For example, if fathers in our sample were more (less) likely to enlist in response to draft eligibility, then the estimates would be too high (low).

**Appendix 2: Extended Results on the Nature of Sons’ Service Outcomes**

In Appendix Table 11, we separately examine whether treated sons are more likely to serve in particular military branches (i.e., the Army, Navy, and Air Force) and work in the civilian defense sector (i.e., the Department of Veterans Affairs (VA) and the Department of Defense (DOD)). Draft-eligible fathers induce sons’ service within each category we consider, ranging from 0.05 p.p. in the Navy to 0.14 p.p. in the Army (top panel, three left columns). To some extent, this range reflects the participation rates we see in our untreated sample, and, when we take these into account, we see the story is a bit more nuanced. While control group participation rates in the Air Force and Navy are about equal (around 1.5%), having a draft-eligible father has more than double the effect on a son’s Air Force enlistment (relative to the Navy). In fact, the point estimate on Air Force enlistment is much closer to that on Army enlistment, even though the Army enlistment rate in the untreated sample is 3.4%. In other words, the estimated increase on Air Force enlistment is 8.63% of baseline, compared with around 4.13% in the Army and 3.33% in the Navy. Future research in this area should link the son’s service area to his father’s to formally test hypotheses regarding a tradition of service within these branches.

To investigate this narrative a bit further, we estimate the effect on work at the VA and the DOD. While these are technically civilian occupations, the nature of the work is connected to the national defense sector. Consistent with our prior findings, we estimate that treated sons are also a bit more likely (0.03 p.p., or 3.12%) to hold a civilian military position in the VA or the Department of Defense than untreated sons (top panel, right column). It seems that treated sons are generally more attached to national defense work, and this attachment holds beyond participation in wars or even enlistment in the military more generally.

**Appendix 3: Mechanisms**

In this appendix, we explore possible mechanisms that underlie our two main findings. While the randomized nature of the Vietnam draft lottery is well suited to determining causal effects of draft eligibility, it is less suited to conclusively determining the mechanisms that drive these effects. As a result, these explorations should be considered suggestive in nature. We begin by discussing mechanisms that may underlie our earnings result and then turn to those that may explain our military service result.

We consider the following mechanisms to be an exhaustive (but not mutually exclusive) list of why sons of draft-eligible fathers tend to earn less than their peers: (1) inherited traits from lower-skilled mothers made them genetically less able; (2) their increased enlistment rate lowered their earnings; (3) different household environments as children led them to have lower reservation wages or prefer lower-paying sectors; or (4) lower parental inputs reduced their general human capital. We investigate each in turn.

First, we investigate whether the effect is driven by genetic factors. While draft eligibility could not have influenced the genetic endowments of fathers, draft-eligible men may have matched, on average, to lower-skilled women. Given the heritability of skills, these lower skills could then be passed on to their children. To probe this narrative, we examine the earnings of the mothers of draft-eligible sons (Appendix Table 15). First, we test and find no evidence of imbalance in our ability to link mothers to treated and untreated sons (i.e., whether the father files jointly in 1996), as shown in the first column. In the remaining columns, we examine whether mothers of draft-eligible sons tend to have worse labor market outcomes than mothers of control sons in 1999 (the first year we can observe information returns). Across mothers’ earnings, log earnings, and labor force participation outcomes, we find no evidence this is so and can rule out even small differences between groups.[[32]](#footnote-32) Since from these findings, genetics seems to be an unlikely explanation, we presume that the earnings effect we detect is driven by environmental factors. The rest of the analysis seeks to uncover which factors.

 Second, we assess the extent to which military service alone can explain the earnings losses we recover.[[33]](#footnote-33) For instance, military enlistment may have generated some of the reduced civilian work experience and delays in attending college we found earlier. To explore the extent to which increased military participation is driving our result, we ask how negative the effect of service must be to generate our earnings estimate (by scaling our earnings result by our military service result). We find that the implied magnitude necessary to explain an average $257 reduction in earnings per son is implausibly high: each son induced to enlist must have suffered a $98,926.92 setback in his 2013 earnings, a value several times the mean earnings of sons of non-draft-eligible fathers ($36,449.49). Still, we probe this finding a bit further by comparing earnings among twins, when one twin enlists and the other does not. Assuming that we can hold genes and environment constant, the difference in earnings should yield the expected earnings effects from military service alone. While these assumptions are unlikely to fully hold and treatment heterogeneity may limit the comparability of different estimates, this exercise suggests we can rule out a decrease of earnings greater than $4,000 (i.e., the bottom of the 95% confidence interval), which is well below the nearly $100,000 that our estimates imply. Altogether, it seems extremely unlikely the military service effect could explain more than a fraction of the earnings response. Finally, while it remains possible that our earnings losses could result from treated sons being less likely, on the whole, to invest in their human capital because of an intention to later join the military (regardless of whether they ultimately enlist), which in turn lowers earnings, this explanation would still imply quite a large effect of this decision and/or a large effect on an intention to join the military.

Third, treated sons may have lower reservation wages or preferences for lower-paying (nonmilitary) work, perhaps because, as noted in findings from prior literature, fathers suffered some initial earnings losses in the decades after Vietnam.[[34]](#footnote-34) (In addition, draft-eligible men in the 1970s were more likely to work (while earning less), though that effect dissipates over time.) While these earnings differentials were erased by the 1990s, those earlier years may have been formative in shaping economic attitudes for sons, such that sons would later find themselves in lower-paying industries or generally be willing to accept less compensation for their work. When we consider our results more broadly, they are not fully consistent with a lower (or higher) reservation wage narrative, because we find that treated sons are less likely to work (which, all else being equal, would imply higher reservation wages) and earn less, conditional on working (which, all else being equal, would imply lower reservation wages). To explore whether treated sons seem to prefer lower-paying jobs, we estimate the intensive 2013 earnings effect of draft eligibility with and without six-digit North American Industry Classification System, or NAICS, industry controls for their highest-paying job (Appendix Table 16).[[35]](#footnote-35) We find that the earnings effect is insignificantly lowered by less than 20%. This estimate, if anything, may overstate the difference, given that (1) treated sons may sort by industry, even if, in the absence of this mechanism, they have lower earnings potential given that certain industries are associated with lower pay; and (2) the estimate mechanically includes any fixed differences in earnings in the military. To account for any bias introduced by treated sons who otherwise earn less sorting into certain industries, we separately calculate industry-simulated earnings. First, we recover mean earnings among non-draft-eligible sons by industry. We then regress this measure on draft eligibility and fail to find a significant relationship. While we cannot conclusively rule out this mechanism (e.g., there could be sorting across occupation, which we cannot measure with tax data and which will not perfectly overlap with detailed industry), the available data we have yields little evidence in support of the hypothesis that preferences for lower-paying jobs are generating our effect.

By elimination, we conclude that our earnings losses are likely driven in large part by lower potential earnings—reflecting differences in cumulative human capital—resulting from reduced parental inputs (and, further, an effect of parents that operates independently of the other mechanisms we considered). This setback in earnings could not have been the result of father absenteeism due to Vietnam-era service, as nearly all dependents claimed in 1996 would not have been born yet. However, prior literature has evinced several channels that may have contributed to our result: two-stage least squares earnings losses of 15% in the 1970s and 10% in the 1980s, increased behavioral health disorders, and greater reliance on disability benefits.[[36]](#footnote-36) Indeed, our results by son’s age suggest that the children born earlier experience, if anything, larger decreases in earnings.[[37]](#footnote-37) In addition, we can observe that the draft induced some fathers to remain in the military long enough to earn a pension (an increase of approximately 1 p.p., as shown in Appendix Table 17), which requires at least 20 years of military service; thus, a portion of the effect could be related to differential outcomes for sons of career military fathers, who either move many times throughout their lives or grow up without their father around.[[38]](#footnote-38) No matter the explanation, the mere fact that risk of conscription that occurred well before many of these children were born generated real differences in their earnings is worthy of further investigation. Future work should do more to unpack how and why the draft induced shocks to parental inputs that would persist so profoundly into the next generation.

 We turn now to potential mechanisms underlying our enlistment estimates. We examine two candidate channels: (1) a transmission of occupational preferences and investment, perhaps due to a transmission of a culture of military service; and (2) relatively higher returns to military service due to lower-paying civilian labor market opportunities. Because of our earnings results (as well as prior literature documenting lower earnings among draft-eligible men in the 1970s and 1980s) and what is conventionally presumed as a driver of enlistment decisions (Hosek and Peterson, 1985; Kilburn and Klerman, 1999; Kleykamp, 2006), we view the second explanation as the default channel, so we explore whether it is operating alone to produce our result or whether the first can explain any of the effect we detect.

First, recall that the relative effect of a father’s draft eligibility on earnings is -0.7%, while for military enlistment, it is 3.4%. If the story were fully one of fewer labor market opportunities, these findings would suggest that the elasticity between military service and earnings opportunities must be quite large (presumably still brought about by the father’s conscription, albeit indirectly).[[39]](#footnote-39) Nonetheless, to explore whether lower opportunities are likely to explain much of the effect, we undertake some suggestive exercises to produce a range of (naive) elasticity estimates and assess whether they could explain our results (Appendix Table 18). To begin, we attempt to directly estimate the relationship between opportunities and enlistment, where, for the purposes of this exercise, we use family income as a proxy for opportunities. Among non-draft-eligible sons, we regress military enlistment on log 1996 family income and recover an estimate of -0.006.[[40]](#footnote-40) Assuming this estimate is unbiased, the magnitude indicates that the decrease in earnings opportunities brought about by having a draft-eligible father (namely, the reduced form effect of draft-eligibility) must be similar to a 43.3% decrease in 1996 family income, which seems implausibly high.[[41]](#footnote-41)

Still, family income in a single year may not be a great proxy of opportunity, given that there are transitory income shocks that could bias the estimate toward zero. To construct a measure of family income across all of childhood, we use the Statistics of Income 1987–96 Family Panel, which contains family income beginning in 1987, to fill the gap in coverage introduced by observing only the universe of returns from 1996 to 2013. Using the average family income of sons between ages 0 and 18, we recover a coefficient of negative 0.012, implying a reduction in opportunities equivalent to a 22% drop in family income, which again seems implausibly large.[[42]](#footnote-42)

To generate a final elasticity estimate, we focus on the 2013 earnings of the non-draft-eligible sons in our sample as a proxy for “latent” earnings potential and ask, to what extent are lower earnings associated with enlistment behavior? For the purposes of this exercise, we assume that (endogenous) military service has no effect on earnings. In a regression of military service on 2013 earnings, we find that a $257 decrease in earnings (our reduced-form estimate on the earnings response) is associated with a -0.00003 increase in military service. (Recall that our reduced-form estimate on military enlistment is 0.0026.) This estimate is orders of magnitude too low to explain the military result; indeed, this exercise suggests that the difference in “latent” earnings must be closer to $26,396 to explain the effect (or that the influence of military service among the compliers must be an implausibly large *positive* number),[[43]](#footnote-43) which again suggests that labor market opportunities are unlikely to fully describe enlistment in our setting. However, these back-of-the-envelope exercises likely suffer from omitted variable bias (among other issues). For example, the earnings opportunity channel may be understated if preferences for enlisting in the military are positively correlated with family or own income. Still, while we cannot determine the size or direction of the omitted variable bias, there would likely have to be quite substantial downward bias in these elasticities for the first channel to fully explain our military result.

Second, the set of results in our analyses of heterogeneity by family income is not, prima facie, consistent with the first channel. Recall that we documented an earnings decrease but no enlistment response among those at the bottom of the family-income distribution, and an enlistment increase but no earnings response for those at the very top of the distribution. Moreover, the point estimates for the insignificant results are wrong signed, and we can separately reject the null hypothesis that these two earnings estimates and two enlistment estimates are the same. As a result, this array of results suggests that the earnings effect operates at least somewhat independently of the enlistment effect.

Third, if sons of draft-eligible fathers indeed have a stronger preference to enlist in the military, we might expect that during times of economic distress—when economic opportunities are more likely to drive enlistment decisions—the gap in participation between the two groups shrinks. We test for this expectation by investigating whether the effect of draft eligibility on service is weaker during the Great Recession. As we show in Appendix Table 19, we find evidence consistent with this prediction. Because a father’s draft eligibility is less important in determining enlistment outcomes when overall job opportunities are fewer, treated sons’ increased military attachment does not appear to be the result of lower economic opportunity alone.[[44]](#footnote-44)

 As a final piece of suggestive evidence, we examine the extent to which we observe persistence by branch of military service. While we do not observe the military service records of fathers in our sample, we take advantage of which branches tended to rely on the draft for servicemen. While many draft-eligible men voluntarily enlisted in the Navy and Air Force—as those branches were perceived to be less dangerous—the draft itself conscripted men into the Army and, to a lesser extent, the Marines. However, in the 1951 and 1952 cohorts, no individuals were drafted into the Marines. (While earlier cohorts could be drafted into the Marines, the United States had stopped conscripting men into that particular branch by February 1970 (Shulimsob, Blasiol, Smith, and Dawson, 1997).) Therefore, we examine whether there is a difference in a son’s proclivity to enlist in the Marines according to whether his father was at risk of being drafted into the Marines. In other words, we compare the 1949 and 1950 cohorts—for which the draft did lead to larger enlistment behavior of the fathers as well as their children, as documented in the appendix—with the 1951 and 1952 cohorts. As shown in Appendix Table 20, we find that the sons of older cohorts are slightly (0.0005 p.p.) more likely to enlist in the Marines, which is consistent with a transmission of occupation.[[45]](#footnote-45)

Thus, we conclude from this evidence that, in addition to whatever reduced labor market opportunities the sons of draft-eligible fathers experienced relative to their peers, there appears to be a separately operating transmission of occupation that can explain some of our military service results.

 Finally, we discuss two subjects that may affect the interpretation of our results. First, we evaluate the extent to which the transmission of occupation may be skills based versus preference based. Certain eligibility criteria must be met to join the U.S. military: mainly, enlistees must meet certain mental aptitude and physical fitness requirements (and, in some cases, have no criminal record). Sons of draft-eligible fathers may therefore have better information about or otherwise be more likely to meet these requirements, or they may possess skills that are relatively higher valued by the military because of their father’s risk of draft service. While we cannot quantify the extent to which this factor drives the transmission in service we observe relative to preferences, a few pieces of evidence suggest it may not be important in the relationship. First, given the overall lower earnings found among sons of draft-eligible fathers, all else equal, mental aptitude is unlikely to be higher among sons of draft-eligible fathers. Second, conditional on military service, military earnings are no different between the two groups (though a higher fraction of the treatment group does serve, introducing a potential selection bias).

Second, we consider the extent to which the U.S. military might actively recruit the children of veterans (COVs) to a greater extent than other children. While the incentives of recruiters are limited to filling their enlistment quotas and there is no evidence that the military formally targets COVs in their recruitment, it may indirectly expend additional resources to target COVs for reasons of self-selection. For example, those in high school Reserve Officers’ Training Corps (i.e., “ROTC”) programs are more heavily recruited (and COVs are likely overrepresented in these programs), and there may be a heightened military recruitment presence in areas with more military families. But in these cases, the military is responding to higher initial interest or (presumably not entirely erroneous) expectations of higher interest among COVs. As a result, to the extent this recruitment operates and influences enlistment, the magnitude of the military service result—but not the sign—may be upwardly biased if it is interpreted as being entirely driven by the higher supply of military service among draft-eligible sons.

**Appendix Table 1.** *Effect of father’s draft eligibility on 1996 family outcomes*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Outcome | (1)Total income | (2)Ln(Total income) | (3)Filing married | (4)Ln(Average Zip code income) |
| Reduced form | -171.92(154.11) | -0.0022 (0.0022) | -0.00002(0.00056) | -0.0018(0.0009)\* |
| Control group mean | 69,600.85 | 70,032.58 | 0.9040 | 39,644.55 |

Notes: The table presents estimates from regressions of family outcomes reported on Form 1040 on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. Earnings are in 2013 dollars. Control group means are derived from sons of non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. *N* = 2,153,234. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 2.** *Effect of Vietnam draft eligibility on Vietnam-era military service*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Father’s year of birth | (1)1951 | (2)1952 | (3)1951–52 |
|  | 0.117(0.004)\*\*\* |  | 0.137(0.005)\*\*\* | 0.127(0.003)\*\*\* |
| Control group mean | 0.0687 |  | 0.1033 | 0.0870 |
| *F*-statistic | 938.197 |  | 927.812 | 1641.060 |

Notes: The table presents estimates from regressions of Vietnam-era military service on a dummy variable indicating whether the individual was Vietnam draft eligible (based on his date of birth) and, in column (3), a dummy variable for the individual’s year of birth. The sample is derived from Defense Manpower Data Center Administrative Records information on accessions, from 1970 to 1973, among men born between 1951 and 1952 and is available online (<http://economics.mit.edu/faculty/angrist/data1/data/angrist90>), aggregated by the individual’s lottery number in sequential bins of five. The specification pools whites and nonwhites, weighting them by their respective proportion in the data. See Appendix 1 for more details. Control group means are derived from non-draft-eligible men. Robust standard errors are reported. *N* = 73 in columns (1) and (2) and 146 in column (3). \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 3.** *Primary outcomes for sons and daughters pooled*

|  |  |
| --- | --- |
|  | Coefficient (SE) on father military service |
| Outcome | (1)Earnings | (2)Any work | (3)Ln(earnings) | (4)Military service | (5)Years of service |
| Reduced form | -180.63(53.10)\*\*\* | -0.0005(0.0006) | -0.0058(0.0019)\*\*\* | 0.0016(0.0023)\*\*\* | 0.0102(0.0018)\*\*\*  |
| Control group mean | 31,270.45 | 0.80144 | 39,017.79 | 0.0465 | 0.2924 |

Notes: The table presents estimates from regressions of pooled son and daughter outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The sample comprises all dependents claimed on 1996 tax returns by men born between 1951 and 1952. Earnings are in 2013 dollars. Control group means are derived from children of non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. *N* = 4,224,651 except in column (3), where *N* = 3,385,136. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 4.** *Effect of draft eligibility by gender*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Outcome | (1)Earnings | (2)Any work | (3)Ln(earnings) | (4)Military service | (5)Years of service |
| Coefficient (SE) on draft eligibilityXson | -164.49(79.01)\*\* | -0.0019(0.0008)\*\* | -0.0005(0.0030) | 0.0021(0.0005)\*\*\* | 0.0113(0.0034)\*\*\* |

Notes: The table presents estimates from regressions of pooled son and daughter outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth), a dummy variable for the father’s year of birth, a dummy variable for whether the child was male, and the interaction with prior variables. The interaction of the son dummy and draft eligibility is reported. The sample comprises all dependents claimed on 1996 tax returns by men born between 1951 and 1952. Earnings are in 2013 dollars. Standard errors are clustered at the father date of birth level. *N* = 4,224,651 except in column (3), where *N* = 3,385,136. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 5.** *Effect of draft eligibility on son’s 2013 earnings and work decisions: Alternative models, samples, and specifications*

|  |  |  |
| --- | --- | --- |
|  | Coefficient (SE) on draft eligibility |  |
|  | (1)Father citizen | (2)Father noncitizen | (3)Include duplicate dependents | (4)Additional father controls | (5)Additional father+son controls | (6)Alt winsorized (A and C) or probit (B) |
| Panel A: Earnings |  |  |  |  |  |  |
| Reduced form | -267.80(78.37)\*\*\* | -22.96(248.22) | -238.67(77.24)\*\*\* | -237.93(68.81)\*\*\* | -234.48(63.06)\*\*\* | -265.54(112.69)\*\* |
| Control group mean | 37,039.20 | 26,103.71 | 36,173.88 | 36,449.49 | 36,449.49 | 38,631.77 |
| Panel B: Any job |  |  |  |  |  |  |
| Reduced form | -0.0013(0.0007)\*\* | -0.0013(0.0034) | -0.0013(0.0007)\* | -0.0016(0.0006)\*\* | -0.0019(0.0006)\*\*\* | -0.014(0.0007)\*\* |
| Control group mean | 0.8101 | 0.6498 | 0.8240 | 0.8251 | 0.8251 | 0.8251 |
| *Panel C: Ln(earnings)* |  |
| Reduced form | -0.0063(0.0024)\*\* | 0.0014(0.0108) | -0.0055(0.0023)\*\* | -0.0052(0.0021)\*\* | -0.0038(0.0019)\*\* | -0.0059(0.0024)\*\* |
| Control group mean | 40,641.52 | 36,611.61 | 43,902.41 | 40,334.42 | 40,334.42 | 46,822.03 |

Notes: The table presents estimates from regressions of son earnings and job outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth for various models, samples, and specifications. Column (1) includes only the sons of fathers who are U.S. citizens, while column (2) includes the sons of noncitizen fathers. Column (3) includes, along with the main sample, dependents who were claimed by more than one tax filer in 1996. Column (4) uses the main sample but includes controls for father month of birth interacted with year of birth along with state of birth. Column (5) adds fixed effects for the son’s exact age. Column (6) presents earnings estimates, where the variable is winsorized at the 99.99th percentile and the “any job” estimate using a probit specification, reporting marginal effects. A regression with raw earnings as the dependent variable yields an effect on earnings of negative 645.43 with a standard error of 319.37, and a regression with log earnings as the dependent variable results in an effect of negative 0.0060 with a standard error of 0.0024, both of which are significant at the 5 percent level. Earnings are in 2013 dollars. Control group means are derived from sons of non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. In panels A and B from left to right, *N* = 2,037,254, 115,980, 2,269,362, 2,153,234, 2,153,234, and 2,153,234. In panel C, *N* = 1,697,957, 77,761, 1,869,013, 1,775,718, 1,775,718, and 1,775,718. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 6.** *Effect of draft eligibility on son’s military service (1999–2013):*

*Alternative models, samples, and specifications*

|  |  |  |
| --- | --- | --- |
|  | Coefficient (SE) on draft eligibility |  |
|  | (1)Father citizen | (2)Father noncitizen | (3)Include duplicate dependents | (4)Additional father controls | (5)Additional father+son controls | (6)Probit (A) or Poisson (B) |
| Panel A: Served in military |  |  |  |  |  |
| Reduced form | 0.0029(0.0004)\*\*\* | -0.0014(0.0013) | 0.0026(0.0004)\*\*\* | 0.0026(0.0004)\*\*\* | 0.0026(0.0004)\*\*\* | 0.0026(0.0004)\*\*\* |
| Control group mean | 0.0785 | 0.0407 | 0.0771 | 0.0765 | 0.0765 | 0.0765 |
| Panel B: Years of service |  |
| Reduced form | 0.0175(0.0033)\*\*\* | -0.032(0.0090) | 0.0150(0.0031)\*\*\* |  0.0155(0.0032)\*\*\* | 0.0159(0.0032)\*\*\* | 0.0340(0.0054)\*\*\* |
| Control group mean | 0.3002 | 0.1550 | 0.4907 | 0.4878 | 0.4878 | 0.4878 |

Notes: The table presents estimates from regressions of son military outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth for various models, samples, and specifications. Column (1) includes only the sons of fathers who are U.S. citizens, while column (2) includes the songs of noncitizen fathers. Column (3) includes, along with the main sample, dependents who were claimed by more than one tax filer in 1996. Column (4) uses the main sample but includes controls for father month of birth interacted with year of birth along with state of birth. Column (5) adds fixed effects for the son’s exact age. Column (6) uses a probit model, marginal effects reported for panel A, and a Poisson model for panel B. Control group means are derived from sons of non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. From left to right, *N* = 2,037,254, 115,980, 2,269,362, 2,153,234, 2,153,234, and 2,153,234. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 7.** *Two-Sample IV for primary outcomes*

|  |  |
| --- | --- |
|  | Coefficient (SE) on father’s military service |
| Outcome | (1)Earnings | (2)Any work | (3)Ln(earnings) | (4)Military service | (5)Years of service |
| Instrument(s) |  |  |  |  |  |
| Eligibility | -2,034.60(626.98)\*\*\* | -0.0110(0.0054)\*\* | -0.0475(0.0189)\*\* | 0.0209(0.0033)\*\*\* | 0.1245(0.0254)\*\*\*  |
|  |  |  |  |  |  |
| EligibilityXyear | -2,066.59(629.66)\*\*\* | -0.0114(0.0054)\*\* | -0.0486(0.0190)\*\* | 0.0208(0.0033)\*\*\* | 0.1215(0.0248)\*\*\*  |
|  |  |  |  |  |  |
| Intervals | -2,039.096(630.22)\*\*\* | -0.0084(0.0054) | -0.0544(0.0192)\*\*\* | 0.0199(0.0033)\*\*\* | 0.1171(0.0257)\*\*\*  |
|  |  |  |  |  |  |
| IntervalsXyear | -1,944.29(621.38)\*\*\* | -0.0101(0.0053)\* | -0.0482(0.0188)\*\*\* | 0.0203(0.0032)\*\*\* | 0.1215(0.0248)\*\*\*  |
| Control group mean | 36,449.49 | 0.8251 | 40,334.42 | 0.0765 | 0.4878 |

Notes: The table presents two-sample instrumental variable (IV) estimates of son outcomes, where the second stage uses fitted values for military service from a first-stage estimate from the Defense Manpower Data Center of Vietnam-era military service on a dummy variable indicating whether the father was Vietnam draft eligible (and other instruments) and a dummy variable for the father’s year of birth. See Appendix Table 2 for more information on the first-stage data. The first row uses draft eligibility as an instrument, the second row uses draft eligibility and draft eligibility interacted with draft year (which is the same as father year of birth) as instruments, the third row uses five draft lottery group bins as instruments following prior literature to exploit within-eligibility changes in the probability of enlistment (the bins were for the following draft numbers: 1–95, 96–125, 126–160, 161–195, and 196–230), and the fourth row uses the five draft lottery bins and the bins each interacted with draft year as instruments. Standard errors from the second stage are clustered on the father date of birth level. When standard errors are instead block bootstrapped on the father date of birth level or the five-day bins level to which the first-stage data are aggregated, they are smaller than the ones presented above. *N* = 2,153,234 except in column (3), where *N* = 1,775,718. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 8.** *Effect of draft eligibility on son’s earnings and military service by father’s year of birth*

|  |  |  |
| --- | --- | --- |
|  | Coefficient (SE) on draft eligibility |  |
| Father’s year of birth | (1) 1948 | (2)1949 | (3)1950 | (4)1951 | (5)1952 | (6)1953 |
| Panel A: 2013 earnings |  |  |  |  |  |  |
| Reduced form | -136.57 (113.79) | 29.59(116.83) | -224.24(100.20)\*\* | -186.56 (95.05)\*\* | -298.51(99.92)\*\*\* | -82.78(91.36) |
| Control group mean | 42,792.41 | 41,101.82 | 39,412.64 | 37,495.91 | 35,591.52 | 33,327.12 |
| Panel B: Any military service (1999–2013) |  |  |  |  |  |
| Reduced form | 0.0005 (0.0006) | 0.0021 (0.0006)\*\*\* | 0.0018(0.0006)\*\*\* | 0.0023 (0.0006)\*\*\* | 0.0029(0.0006)\*\*\* | 0.0016(0.0007)\*\* |
| Control group mean | 0.0711 | 0.0716 | 0.0721 | 0.0753 | 0.0774 | 0.0793 |
| Effect of draft-eligibility on cohort military service (from AC 2011) | 0.0549 | 0.0710 | 0.1276 | 0.1330 | 0.1593 | 0.0305 |

Notes: The table presents estimates from regressions of son outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and month of birth and state of birth fixed effects for sons of fathers born in different years for the years 1948–53. The effect of draft-eligibility on military service by cohort from Angrist and Chen (2011) is reproduced at the bottom of the table. To make the reproduced results as comparable as possible with our reduced form, we use the same controls as they do—namely, the month of birth and state of birth of fathers. Month of birth is a particularly important control variable for the cohorts subject to the 1969 draft lottery (1948-1950 ) because of errors in randomizing draft numbers across birthdates, especially across birth month. The father state of birth variable used here is reconstructed based on the first three digits of the father’s Social Security number, which can be used to infer the state of issuance but is not the same as the state of birth and could theoretically be endogenous (though practically speaking has little influence on the results). Month of birth controls are especially important for the 1948–50 birth cohorts because of randomization errors in the lottery. The samples comprise male dependents claimed on 1996 tax returns by men in each respective cohort. Earnings are in 2013 dollars. Control group means are derived from sons of non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. *N* = 811,813, 882,106, 943,609, 1,036,356, 1,116,825, and 1,164,146 from left to right. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 9.** *Effect of draft eligibility by son’s birth year*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Outcome | (1)Earnings | (2)Any work | (3)Ln(earnings) | (4)Military service | (5)Years of service |
| Coefficient (SE) on draft eligibility X son birth year | 17.79(10.98) | 0.00019(0.00013) | 0.0002(0.0004) | -0.00010(0.00008) | -0.0015 (0.0006)\*\* |
| Control group mean | 36,449.49 | 0.8251 | 40,334.42 | 0.0765 | 0.4878 |

Notes: The table presents estimates from regressions of son earnings and job outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and dummy variables for the father’s year of birth, son’s year of birth, and son’s year of birth interacted with draft eligibility. The interaction is presented. If the dependent variable is divided by the control group mean for each year of birth before running the regression, insignificant effects are found. The sample comprises male dependents claimed on their 1996 tax return by men born between 1951 and 1952. Control group means are derived from sons of non-draft-eligible fathers. Panel C control group means are constructed by taking the mean of all positive values before the log transformation. Standard errors are clustered at the father date of birth level. Earnings are in 2013 dollars. *N* = 2,153,234 except in column (3), where *N* = 1,775,718. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 10.** *Effect of draft eligibility by father-son linkage type*

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| Outcome | (1)Earnings | (2)Any work | (3)Ln(earnings) | (4)Military service | (5)Years of service |
| Coefficient (SE) on draft eligibility X SSA link | -4.48(33.32) | -0.0002(0.0006) | -0.0003(0.0021) | -0.0003(0.0003) | -0.0001(0.0021) |
|  Control group mean | 25,983.40 | 0.8272 | 26,392.99 | 0.0691 | 0.3651 |

Notes: The table presents estimates from regressions of outcomes for sons linked through 1996 tax returns pooled with outcomes for sons linked through Kidlink (derived from Social Security card applications) on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth), a dummy variable for the father’s year of birth, a dummy variable for whether the linkage is through Kidlink, and the interaction of Kidlink with the prior variables. The interaction between Kidlink and draft eligibility is reported. The sample comprises males born beginning in 1983 to fathers born between 1951 and 1952 according to each data set; most sons appear in both data sets. Control group means are derived from sons of non-draft-eligible fathers. Earnings are in 2013 dollars. Standard errors are clustered at the father date of birth level. *N* = 2,153,197 except in column (3), where *N* = 1,780,158. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 11.** *Effect of father draft eligibility on other son military outcomes (1999–2013)*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Panel A: Branch | (1) | (2) | (3) | (4) |
|  | Air Force | Army | Navy | Civilian defense (DOD/VA) |
| Reduced form | 0.0012(0.0002)\*\*\* | 0.0014(0.0003)\*\*\* | 0.0005(0.0002)\*\*\* | 0.00034(0.00017)\*\* |
| Control group mean | 0.0139 | 0.0339 | 0.0150 | 0.0109 |
| Panel B: Other outcomes | (5)Years of military service intensive margin | (6)Any son in military | (7)In military (one son in household) | (8)In military (> one son household) |
| Reduced form | -0.0139(0.0224) | 0.0031(0.0005)\*\*\* | 0.0032(0.0006)\*\*\* | 0.0021(0.0006)\*\*\* |
| Control group mean | 6.38 | 0.0991 | 0.0763 | 0.0763 |

Notes: The table presents estimates from regressions of different son military service outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. Marines and Coast Guard outcomes cannot be examined individually for disclosure purposes. Column (5) includes only sons who were in the military, while columns (6)–(8) are on the father level instead of the son level, whereby column (7) limits the sample to families with one male dependent and column (8) limits the sample to families with more than one male dependent. Control group means are derived from sons of non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. *N* = 2,153,234 in panel A, and *N* = 166,210, 1,539,527, 1,031,343, and 508,184 from left to right in panel B. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* <0.10.

**Appendix Table 12.** *Effect of draft eligibility on son outcomes by father income in 1996*

|  |  |  |
| --- | --- | --- |
|  | Coefficient (SE) on draft eligibility |  |
| Family income quantile | (1)*p* < (10) | (2)10 < *p* < 25 | (3)25 < *p* < 50 | (4)50 < *p* < 75 | (5)75 < *p* < 90 | (6)*p* > 90 |
| Panel A: 2013 earnings |  |  |  |  |  |
| Reduced form | -325.76(164.91)\*\* | -51.97(132.75) | -289.92 (108.485)\*\*\* | -418.22(129.01)\*\*\* | -425.36(195.37)\*\* | 472.43(287.26) |
| Control group mean | 22,063.31 | 26,421.35 | 32,687.46 | 39,875.30 | 45,527.23 | 51,902.63 |
| Panel B: Military service |  |  |  |  |  |
| Reduced form | -0.0014(0.0012) | 0.0017(0.0010)\* | 0.0040(0.0009)\*\*\* | 0.0037(0.0009)\*\*\* | 0.0015(0.0010) | 0.0032(0.0010)\*\*\* |
| Control group mean | 0.0624 | 0.0802 | 0.0902 | 0.0842 | 0.0690 | 0.0422 |
| Panel C: Pr(quantile) |  |  |  |  |  |
| Reduced form | 0.0005(0.0007) | -0.00003(0.00071) | 0.0005(0.0009) | 0.00053(0.0008) | -0.0004(0.0008) | -0.0011(0.0007) |

Notes: The table presents estimates from regressions by parent-income quantiles of son earnings and military outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. Panel C tests and confirms that the sons of draft-eligible fathers are not more or less likely to be in any of the family-income quantiles. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952 across different family-income quantiles. Control group means are derived from sons of non-draft-eligible fathers. Earnings are in 2013 dollars. Standard errors are clustered at the father date of birth level. From left to right in panel A and B, *N* = 215,321, 322,957, 538,269, 538,272, 322,951, and 215,303, while *N* = 2,153,197 in panel C. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

A**ppendix Table 13.** *Effect of draft eligibility on the distribution of son earnings in 2013*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Earnings percentile | (1)Earn > *p*(10) | (2)Earn > *p*(25) | (3)Earn > *p*(50) | (4)Earn > *p*(75) | (5)Earn > *p*(90) |
| Reduced form | -0.0014(0.0007)\*\* | -0.0014(0.0008)\* | -0.0038(0.0009)\*\*\* | -0.0027(0.0008)\*\*\* | -0.0017(0.0005)\*\*\* |
| Earnings cutoff | 0 | 4,501 | 27,514 | 52,642 | 83,612 |

Notes: The table presents estimates from regressions of whether the son is in particular 2013 earnings quantiles on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The earnings cutoff for each quantile is presented below the regression results. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952 across different family-income quantiles. Earnings are in 2013 dollars. Standard errors are clustered at the father date of birth level. *N* = 2,153,197 in panel C. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 14.** *Effect of father draft eligibility on other son outcomes*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Panel A: Other college and health |
| Outcome | (1)College attendance in 2013 | (2)Log $postsecondary quality (1999–2013) | (3)Alive in 2013 | (4)Any disability income in 2013 |
| Reduced form | 0.0004(0.0006) | -0.0008(0.0005) | -0.00001(0.00023) | -0.00002(0.00019) |
| Control group mean | 0.1518 | 38882.21 | 0.0225 | 0.0153 |
| Panel C: Transition into adulthood |  |  |  |
| Outcome | (5)Any UI income in 2013 | (6)Number of children fathered as teenager | (7)Total number of children  | (8)College attendance at age 19 |
| Reduced form | -0.00004(0.00038) | 0.0006(0.0002)\*\*\* | -0.0026(0.0018) | -0.0024(0.0011)\*\* |
| Control group mean | 0.0617 | 0.0159 | 0.6749 | 0.6011 |

Notes: The table presents estimates from regressions of other military outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. Information on college attendance is derived from Form 1098-T, and postsecondary quality is measured as the average 2013 earnings of individuals who attended each school (excluding the cohorts in the analysis), is the weighted average for sons who attended more than one institution, and excludes those who attended no institution. The measure of whether the son is alive is derived from the Social Security Death Master File, while disability income is derived from Form 1099-SSA, unemployment income (UI) is derived from Form 1099-G, and fertility is derived from Kidlink. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. Control group means are derived from sons of non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. *N* = 2,153,234 except in column (2), where *N* = 1,547,905, and in column (8), where *N* = 1,476,340. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 15.** *Effect of draft eligibility on 1999 mother earnings*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Outcome | (1)Mother on 1996 return | (2)Earnings  | (3)Any work | (4)Ln(earnings) |
| Reduced form | 0.0001(0.0006) | -16.33(79.41) | 0.0004(0.0010) | 0.0007(0.0027) |
| Control group mean | 0.9024 | 35,353.18 | 0.7369 | 47,974.22 |

Notes: The table presents estimates from regressions of whether a spouse appears on the 1996 tax return on which the male dependent is claimed and, if so, her 1999 earnings and work outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. The sample in column (1) comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952, while, in columns (2)–(4), the sample comprises the returns where a spouse is also claimed. Control group means are derived from values associated with non-draft-eligible fathers. Earnings are in 2013 dollars. Standard errors are clustered at the father date of birth level. *N* = 2,153,234 in column (1), *N* = 1,943,353 in columns (2)–(3), and *N* = 1,432,537 in column (4). \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 16.** *Effect of draft eligibility on son industry choice in 2013*

|  |
| --- |
|  Coefficient (SE) on draft eligibility |
| Outcome | (1)Nonzero earnings | (2)Nonzero earnings (w/ 6-digit industry control) | (3)Simulated nonzero earnings based on industry |
| Reduced form | -247.23(84.26)\*\*\* | -201.23(73.36)\*\*\* | -43.19(28.72) |
| Control group mean | 44,196.17 | 44,196.17 | 44,196.17 |

Notes: The table presents estimates from regressions of nonzero earnings and simulated nonzero earnings based on the earnings of sons of non-draft-eligible fathers in each son’s 6-digit North American Industry Classification System (NAICS) industry on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth), a dummy variable for the father’s year of birth, and, in column (2), fixed effects for 6-digit NAICS industry. NAICS codes for firms are available in the Internal Revenue Service Business Master File, prepared by the Internal Revenue Service, and individuals are assigned their industry based on the industry in which they had the highest earnings in 2013. Earnings are in 2013 dollars. The sample comprises male dependents with nonzero earnings claimed on 1996 tax returns by men born between 1951 and 1952. Control group means are derived from sons of non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. *N* = 1,768,485. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 17.** *Effect of draft eligibility on father receiving a military pension (1999–2013)*

|  |  |
| --- | --- |
|  | Coefficient (SE) on draft eligibility |
| Reduced form | (1)0.0130 (0.0004)\*\*\* |
| Control group mean | 0.0329 |

Notes: The table presents estimates from a regression of father military pension receipt on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth) and a dummy variable for the father’s year of birth. In order to qualify for a military pension, an individual must have served in the military for at least 20 years. Military pension receipt is derived from whether the father received a Form 1099-R from the military anytime between 1999 and 2013. The sample comprises men born between 1951 and 1952 who claimed male dependents on their 1996 tax returns. Control group means are derived from non-draft-eligible fathers. Standard errors are clustered at the father date of birth level. *N* = 2,153,234. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 18.** *Association between son military service and family/own income*

|  |  |  |  |
| --- | --- | --- | --- |
| Independent variable | (1)Ln(1996 Family income) | (2)Ln(Family income in childhood) | (3)2013 own earnings/257 |
| Coefficient(SE) | -0.0006(0.0001)\*\*\* | -0.0120(0.0053)\*\* | -0.00003(0.000001)\*\*\* |

Notes: The table presents estimates from regressions of military service (1999–2013) on various family-income and own-earnings measures, along with controls for son and father age among sons with non-draft-eligible fathers. Column (1) uses family income reported on the 1996 tax return, column (2) uses average family income reported on the tax return while the son was between 0 and 18 years of age, and column (3) uses the 2013 earnings of the son scaled by the reduced-form main earnings effect (for interpretation purposes). The sample in column (1) and (3) comprises male dependents claimed on their 1996 tax return by men born between 1951 and 1952 who received a draft-ineligible lottery number (i.e., the control group), while the sample in column (2) comprises all male dependents that appear in the Statistics of Income 1987(–present) Family Panel that were born between 1987 and 1994 (such that a measure of family income is available for them throughout childhood and they are at least 19 years of age in 2013). Earnings are in 2013 dollars. Standard errors are clustered at the family level. *N* = 1,509,903 in columns (1) and (3), and *N* = 5,782 in column (2). \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 19.** *Effect of draft eligibility on son military service in Great Recession versus other years*

|  |  |
| --- | --- |
|  | 1. Military service
 |
| Coefficient (SE) on draft eligibility X Great Recession | -0.0099(0.0050)\*\* |

Notes: The table presents estimates from a regression of annual son military service (1999–2013) on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth), a dummy variable for the father’s year of birth, a dummy variable for whether the Great Recession transpired during the year (2007–09), and the interaction of the Great Recession dummy variable with draft eligibility, the latter of which is presented in the table. In order for the interpretation to be relative, the dependent variable is scaled by non-draft-eligible son service for each year; the mean among sons of non-draft-eligible fathers is therefore 1. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. Standard errors are clustered at the father date of birth level. *N* = 34,451,744. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

**Appendix Table 20.** *Effect of draft eligibility on son Marines or Coast Guard service by father draft*

|  |  |
| --- | --- |
|  | 1. Military service
 |
| Coefficient (SE) on draft eligibility X 1970 draft | 0.0005(0.0002)\*\* |

Notes: The table presents estimates from a regression of military service in the Marines or Coast Guard (1999–2013) on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth), a dummy variable for the father’s year of birth, a dummy variable for whether the father was susceptible to being drafted into the Marines (i.e., whether he was from the 1948–50 birth cohorts and therefore in the 1969 draft lottery), and the interaction of the susceptibility variable with draft eligibility, the latter of which is presented in the table. The mean among sons of non-draft-eligible fathers is 0.0112. The sample comprises male dependents claimed on 1996 tax returns by men born between 1948 and 1952. Standard errors are clustered at the father date of birth level. *N* = 4,867,456. \*\*\* denotes *p* < 0.01; \*\* denotes *p* < 0.05; \* denotes *p* < 0.10.

1. Email: sarena.f.goodman@frb.gov and adam.isen@treasury.gov. This paper does not necessarily reflect the views of the Federal Reserve Board or the U.S. Treasury. We thank seminar participants at the 2015 Office of Tax Analysis Research Conference for helpful comments and suggestions. We also thank Mark Duggan, John Hamilton, Katherine Meckel, and Jesse Rothstein for helpful discussions. All errors are our own. [↑](#footnote-ref-1)
2. According to the National Vietnam Veterans Readjustment Study (Kulka and Schlenger, 1988), 15 percent of Vietnam veterans were suffering from posttraumatic stress disorder (PTSD) in 1988, and 31 percent had experienced PTSD cumulatively. The evidence for early- and later-life physical health effects and later-life mental health effects for draft-eligible men is inconclusive (see also Dobkin and Shabani, 2009). [↑](#footnote-ref-2)
3. We separately present and discuss estimates for daughters as well. [↑](#footnote-ref-3)
4. The enlistment rate and average time spent in the military among sons whose fathers were not draft eligible (i.e., the control group) were 7.65% and 0.49 year over this period, respectively, so the effects we detect each represent an increase of over 3%. [↑](#footnote-ref-4)
5. We note that the exclusion restriction (i.e., that draft eligibility influences sons’ outcomes only through military service of the fathers) may not necessarily hold. Moreover, not only could some avoidance behaviors influence the effects we detect, but many outcomes that result from serving in the military, rather than service itself, could in part determine them as well. [↑](#footnote-ref-5)
6. The rationale for our focus on sons is twofold. First, military occupations are male dominated; even today, men compose over 80 percent of the U.S. military, so there is likely more scope for the transmission of military service among sons. In addition, prior literature on intergenerational transmission of earnings and occupation argues that daughters’ earnings may not be a valid indicator of their labor market success (Oreopoulos, Page, and Stevens, 2008). Whether this argument remains as true today, our results can be best understood within the context of these earlier findings. [↑](#footnote-ref-6)
7. See, for example, Angrist (1990); Dobkin and Shabani (2009); Angrist and Chen (2011); Angrist, Chen, and Song, (2011); Angrist, Chen, and Frandsen (2010); Duggan, Rosenheck, and Singleton (2010); Autor, Duggan, and Lyle (2011); Coile, Duggan, and Guo (2015); and Autor, Duggan, Greenberg, and Lyle (2015). [↑](#footnote-ref-7)
8. Parental inputs have traditionally been linked to children’s outcomes (e.g., Becker and Tomes, 1976, 1979, and 1986; Becker, 1981; Cunha, Heckman, and Schennach, 2010). There is evidence that parenting plays a key role in explaining differences in outcomes across racial and socioeconomic status groups (e.g., Brooks-Gunn, Klebanov, and Duncan, 1996), as well as by gender (Bertrand and Pan, 2013). Other related papers have linked economic shocks during development to children’s achievement and attainment (Ananat et al., 2011; Hilger, 2014). [↑](#footnote-ref-8)
9. Kleykamp (2006), analyzing data from Texas high school graduates in 2002, studies how familiarity with the military, as well as educational goals and socioeconomic factors, correlates with the decision to join. Campante and Yanagizawa-Drott (2015), using census data to link together fathers and adult sons who live in the same household, exploit variation in a father’s age relative to the timing of U.S. wars to quantify how a father’s wartime service affects his son’s military service. They recover a large participation effect during wars but a large negative effect on military service in nonwar eras, yielding a zero net participation overall. [↑](#footnote-ref-9)
10. Dal Bó, Dal Bó, and Snyder (2009) find that, in the case of elected office, there is a strong causal relationship between parental electoral wins and their offspring later holding office, which they attribute to self-perpetuating political power. Hellerstein and Morill (2011) compare, as women became more likely to work, trends in the likelihood that daughters assume their father’s occupation with trends in the likelihood that they assume their father-in-law’s and estimate that at least 13% of the increase in the former was driven by occupation-specific human capital transmission. [↑](#footnote-ref-10)
11. The DM-1 file is the master database used by the Internal Revenue Service that links data from multiple sources, including the Social Security Administration, to capture the full universe of prospective tax filers and their dependents, i.e., everyone with a Taxpayer Identification Number, which in most cases is a Social Security number. While some prior papers on the Vietnam draft focused more on white men, we pool races (as the tax data do not contain race). [↑](#footnote-ref-11)
12. In addition, we retain 1996 income and other information from the returns, which we use to investigate heterogeneity by income in the appendix. [↑](#footnote-ref-12)
13. These data are available on Joshua Angrist’s website at http://economics.mit.edu/faculty/angrist/data1/data/angrist90. Tax data do not record the past military service of fathers. [↑](#footnote-ref-13)
14. We exclude from our main analysis sample any children that were claimed by more than one tax filer. We confirmed that a duplicate claim is uncorrelated with draft eligibility status. A robustness check retaining these “duplicate children” is available in the appendix. [↑](#footnote-ref-14)
15. In our baseline regressions, there is one control variable, a cohort fixed effect (i.e., whether the father was born in 1951 or not), which in theory could pose problems in constructing a Wald estimate. But in practice, this fixed effect has no influence on the estimates; as we show in the appendix, our results are extremely similar if we construct instrumental variable (IV) estimates based on two-sample IV (Angrist and Krueger, 1992a) in which the second stage uses fitted values for military service from a first-stage estimate from the Defense Manpower Data Center data with a cohort effect. [↑](#footnote-ref-15)
16. To suggestively examine this issue, we exploit natural variation across dates of birth in the propensity of appearing in our sample. We ask to what extent differences in the propensity to show up in the sample relate to differences in the first stage. To do so, for each year, we regress the number of times the father appears in the sample on a linear control for exact age (since exact age will mechanically predict whether you claimed a child in 1996), take the residuals, and take the average of the residuals on the binned date-of-birth level to which the first-stage data are aggregated (one bin for every five draft lottery numbers). We then regress the proportion that served in the military on draft eligibility, the averaged residuals, and their interaction. We find a negative but insignificant interaction. This result suggests that the first stage we use may, if anything, be underscaling our Wald estimates, although the estimate is statistically inconclusive, and the exercise suffers from other concerns (though we do separately confirm that the averaged residuals are uncorrelated with draft eligibility). [↑](#footnote-ref-16)
17. To reduce the influence of outliers, we winsorize the earnings measure at the 99th percentile of draft-ineligible sons but show in the appendix that the earnings results are extremely similar if we winsorize at the 99.99th percentile and are even larger when raw earnings are used. Further, draft eligibility is insignificantly (and negatively) correlated with being in the top percentile. [↑](#footnote-ref-17)
18. In line with our definition stated earlier, we consider any year in which a son has positive military earnings a year in which he served in the military. [↑](#footnote-ref-18)
19. In the appendix, we also consider probit and Poisson specifications designed to capture the limited number of values each of these measures can take. Results do not materially differ from those we describe in the main text. [↑](#footnote-ref-19)
20. Because of confidentiality concerns, we report results only for employment categories associated with more than one EIN. [↑](#footnote-ref-20)
21. Campante and Yanagizawa-Drott (2015), in the census years in which they can observe both wartime and nonwartime service, estimate father service leads to a 5.4 p.p. increase (relative to a mean enlistment rate of 10.0 p.p.) in the likelihood a son serves during wartime, which is fully offset by a 5.3 p.p. decrease (relative to a mean enlistment rate of 5.8 p.p.) during peacetime, and subsequently rule out occupational choice in favor of a strong culture of war as the key mechanism underlying transmission. [↑](#footnote-ref-21)
22. This ignores any possible differential selection bias between sons and daughters. [↑](#footnote-ref-22)
23. When we include an interaction term for sons when pooling all children, sons and daughters experience statistically distinguishable changes in all outcomes but log earnings (Appendix Table 4). This result is consistent with literature finding sons are especially sensitive to changes in their household environment (e.g., Autor and Wasserman, 2013). However, relative to their sample means, all of the sons and daughters estimates are statistically indistinguishable, except labor force participation. [↑](#footnote-ref-23)
24. Prior studies of the Vietnam-era draft leveraged the uncertainty surrounding the true draft threshold for variation (Angrist, Chen, and Frandsen, 2010). Because lower draft numbers translated into higher draft risk more generally, military participation has been shown to nonlinearly decrease with respect to draft number. These five bins were for the following draft numbers: 1–95, 96–125, 126–160, 161–195, and 196–230. The estimates from this exercise are consistent with an interpretation of our main estimates as the byproducts of service rather than avoidance, provided that the increased probability of enlistment associated with lower draft numbers does not systematically relate to increased draft avoidance. [↑](#footnote-ref-24)
25. Using 1987 as the first year will slightly understate the proportion missing, since children born before the father was, on average, 17.5 years old would not be claimed on a 1987 return unless the child was enrolled in higher education. These <1% Statistics of Income samples are far too small to meaningfully estimate the effect of draft eligibility on sons’ earnings and military service. [↑](#footnote-ref-25)
26. Kidlink is also more likely to include noncustodial biological children compared with tax filings that would include custodial nonbiological children. [↑](#footnote-ref-26)
27. A limitation in interpreting these results is that we do not have a correspondence between draft eligibility and Vietnam-era service (or other outcomes) for men from different income groups. Relative to prior U.S. war drafts, the Vietnam War draft in particular drew from the middle of the income distribution, reportedly because high-income draftees were afforded new avenues to avoid service or being sent to war (i.e., conscientious objector status or deferments for educational, occupational, or medical reasons) and lower-income draftees were disproportionately rejected on account of poor medical conditions, low aptitude, or criminal records (Card and Lemieux, 2001; Segal and Segal, 2004). [↑](#footnote-ref-27)
28. To validate this analysis, we regress the likelihood of being in a particular income quartile on draft eligibility, and the coefficients are all indistinguishable from zero (Appendix Table 12, bottom panel). [↑](#footnote-ref-28)
29. In the appendix, we provide some discussion about the extent to which transmission of preferences versus skills is operating, but we remain agnostic in the conclusion we draw from our results. [↑](#footnote-ref-29)
30. In the appendix, we also consider that the military may have higher demand for the children of veterans (COVs)—manifested through, for example, informal heightened recruiting of COVs—reaching the conclusion that these efforts would likely be driven by higher initial interest among these children. [↑](#footnote-ref-30)
31. We note that other data sets (e.g., 2000 census data and Social Security Administration data) have yielded somewhat different estimates of the effect of draft eligibility on draft cohort military service (Angrist, Chen, and Song (2011)). [↑](#footnote-ref-31)
32. Given that we find no effect on having a job, the null result on overall earnings is unlikely to be driven by an income effect and lower skill pushing in different directions, whereby lower-skilled mothers work more to make up for the lower earnings of the father in the 1970s and 1980s. [↑](#footnote-ref-32)
33. While parental inputs, broadly defined, would be inclusive of this mechanism, we are interested here in whether the effect is driven *exclusively* by military service of the son (and, in turn, an intergenerational transmission of military service). [↑](#footnote-ref-33)
34. A father being in a specific line of work could also raise the relative return to that line of work through (investment in) job-specific capital. [↑](#footnote-ref-34)
35. We note the limitation that the sample is selected due to an extensive margin work response. [↑](#footnote-ref-35)
36. A high incidence of drug abuse has also been documented among Vietnam serviceman (Robins, Davis, and Goodwin 1974). We note that the samples from which some of these estimates were constructed are not exactly the same as the population from which we draw. For example, the earnings losses cited above are drawn from a sample of white men from these cohorts; for our sample, we do not impose a race restriction but instead limit it to male dependents claimed in 1996 by men from these cohorts. [↑](#footnote-ref-36)
37. Also, when we hold constant the age at which we observe the son and interact year of birth with draft eligibility, we generally find larger effects for those born earlier. [↑](#footnote-ref-37)
38. While the small increase in father career military service is unlikely to explain a significant part of the earnings, this finding bears insight into our main estimates in two ways. First, the sons of career military fathers may be more likely to relocate frequently, spend less time with their fathers, or some combination of the two. Children’s academic achievement scores suffer when fathers are deployed (Engel, Gallagher, Lyle, 2010; Lyle, 2006), which could generate down-the-road earnings losses. In addition, continual shocks to the environment during formative periods of childhood and adolescence, brought about by regularly relocating, might be linked to less favorable labor market outcomes down the road. Second, heterogeneity may be underlying the transmission of service we estimate among our draftees. In other words, some drafted fathers were compelled to serve well beyond their required tours, and these fathers may be driving some of the service result. Indeed, naive correlations between career military fathers and either son earnings or military service in the control group suggest that the military service result (but not the earnings result) could in large part be explained by the draft inducing a long-term career in the military. [↑](#footnote-ref-38)
39. If an enlistment decision was based purely on economic returns, then a comparison of the estimates would understate how large the elasticity must be (i.e., if not for the military service path, earnings of sons of draft-eligible fathers would be even lower). [↑](#footnote-ref-39)
40. There may be nonlinearity in the relationship. Moreover, the estimate cannot be interpreted causally, since there may be omitted variables correlated with income that influence enlistment. [↑](#footnote-ref-40)
41. For comparison, prior work documented (reduced form) male earnings reductions due to draft eligibility of approximately 1.8 percent on account of the Vietnam draft in the 1980s, the decade during which most of our sample was alive (this comparison is illustrative and does not imply that decreases in earnings opportunities are necessarily either exclusively or partially due to earnings reductions from the 1980s). [↑](#footnote-ref-41)
42. Interestingly, if childhood family income is split between years 0–9 and 10–18 and treated as separate regressors, they statistically differ, and only the latter is negative (whereas the earnings losses documented in prior literature were larger when the children were younger). [↑](#footnote-ref-42)
43. This final elasticity exercise uses level earnings instead of log earnings so that the magnitude can be compared with our main effect on earnings. [↑](#footnote-ref-43)
44. However, we cannot rule out that during the Great Recession the prospects of the otherwise more affluent control group deteriorated more so than the son of draft-eligible fathers. [↑](#footnote-ref-44)
45. Because of disclosure issues, we have to group the Marines with the Coast Guard, although enlistment behavior in the much larger Marines is driving the results. [↑](#footnote-ref-45)