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

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Abstract

We study how randomized variation from the Vietnam draft lottery affects the next generation's labor market. Using the universe of federal tax returns, we link fathers from draft cohorts to their sons and offer two primary findings. First, sons of men called by the lottery 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 demonstrate that manipulating parents' circumstances can alter children's later-life outcomes and, more specifically, are consistent with two separately operating channels: (1) parental inputs as important determinants of human capital development and (2) intergenerational transmission of occupation.

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I. Introduction

Parents play an influential role in their children's later-life economic outcomes. Still, in understanding how, it is difficult to disentangle parents' genetics from the environment they provide. Thus, a fundamental question is whether manipulating parental circumstances alone can alter children's outcomes.

The Vietnam draft lottery provides a unique setting to examine this question: the experiences of men randomly called by the Vietnam draft lottery have differed from those of their peers in important ways. Early seminal work linked the lottery to large increases in military service and subsequent earnings losses (Angrist, 1990).[2] Related research has documented swollen disability rolls among men called by the draft lottery, reflecting a combination of negative health outcomes and increased generosity of the program (Angrist, Chen, and Frandsen, 2010).[3] These differential experiences among otherwise-comparable men likely generated meaningful differences for their households in the decades following the war, which, consequently, could have affected their children's labor market in the long run.

In this paper, we investigate whether and how Vietnam-era conscription spilled over to future generations. Specifically, we ask two questions

(1) did a father's risk of Vietnam draft service affect his son's later-life earnings?

(2) did this risk increase the likelihood his son voluntarily enlists in the military?

and anchor our findings within several lines of research.[4] We first estimate that sons of men called by the draft lottery fare worse in the labor market. Compared with their peers, they have lower labor force participation and lower earnings, both unconditionally and conditional on

[2] Subsequent analyses found that the gap in earnings disappeared over the long run (Angrist and Chen, 2011; Angrist, Chen, and Song, 2011).

[3] 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. Dobkin and Shabani (2009) find some evidence of early life mental health problems among men called by the draft lottery. Other work in economics investigating the health and disability of servicemen from this period more generally (Duggan, Rosenheck, and Singleton, 2010; Autor, Duggan, and Lyle, 2011) has reached similar conclusions.

[4] We separately present and discuss estimates for daughters as well.

participation. This finding indicates that the early negative earnings consequences of the Vietnam draft persisted into the next generation. We also find that these sons are more likely to enlist in the military and are generally more wedded to defense work, which, all else equal, suggests a larger military today reduces the costs of building a military tomorrow. In addition, we offer supplemental evidence that our results contribute to the growing literatures on (1) how adverse shocks to household conditions, and specifically parental inputs,[5] can reduce children's later-life outcomes and (2) the intergenerational transmission of occupational choice.

To generate these findings, 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 called by the draft lottery with those of sons whose fathers were not. Such a comparison presents a number of data challenges (e.g., identifying men who were called by the draft lottery, linking such men to their sons, observing sons' labor markets), which we overcome using the universe of U.S. federal tax data. 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 called by the lottery (i.e., draft eligible). Before we endeavor to compare sons of these two groups, we first demonstrate that there is no evidence of survival bias or other sample selection bias that would make them incomparable. We additionally find no evidence of differential fertility in our data. 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 begin by investigating important dimensions of a son's labor market that could be affected by his father's draft eligibility. Specifically, we examine sons' outcomes in 2013, when their average age was 31 years. We estimate that a father's draft eligibility, on average, reduces his son's earnings, by $268 (0.72% of the mean), and labor force participation, by 0.14

[5] Throughout the paper, we define parental inputs broadly to include the quantity and quality of parental time and resources that influence children as well as any indirect effects of these inputs, e.g., schools, neighborhoods, peer groups. We treat attitudes, preferences, or job specific skills that are transmitted either directly or indirectly via parents as separate mechanisms.

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 0.02 year longer, than their peers (both over 3% of the mean). 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 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.

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.[6] For instance, our earnings estimates suggest that a father's military service lowers his son's annual earnings by about $2,000 (equivalent to a reduction of more than 5%). In addition, his 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.

[6] These estimates are suggestive in nature for a number of reasons. First, the tax data do not record military service prior to 1999, so we rely on a separate dataset to estimate service by date of birth among draft lottery cohorts. Second, the exclusion restriction (i.e., that draft eligibility influences sons' outcomes only through their fathers' military service) may not necessarily hold if, for example, men took action to avoid the draft (e.g., college attendance) that separately influenced the outcomes we consider. We also note that outcomes that result from serving in the military in this context, rather than service itself, could have affected sons' labor markets as well.

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 of draft-eligible fathers 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.[7] Among daughters, the estimated effects on earnings is almost 40% as large, the log earnings estimate is similar to the result for sons, and the labor force participation effect is indistinguishable from zero. The effect on their enlistment behavior 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.

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 potentially 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 develop skills or preferences for lower-paying sectors; or (4) reduced parental inputs lowered their general human capital. We test and find no evidence for skill differences between mothers; therefore, we surmise that the earnings effect is driven by environmental factors brought about by parents' circumstances (i.e., mechanisms (2)-(4) above). We then use patterns in the data to argue our result is most consistent with reduced parental inputs. We also consider two candidate channels that could explain increased military participation among draft-eligible sons: (1) a transmission

[7] The rationale for our focus on sons is twofold. First, military occupations are male dominated, and, in fact, certain parts of the military only allow males to serve; 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).

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 transmission of occupational skills or preferences—perhaps due to a culture of military service in 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.[8] By focusing on the children of men subject to the draft, our results demonstrate that even though some observed labor market and other setbacks among those called by the draft lottery 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. These consequences cannot be overlooked in an accounting of the costs of the Vietnam War and suggest potential intergenerational costs should be considered in the context of other wars (e.g., Blimes and Stiglitz, 2008).

Second, our findings point to a causal role linking a parent's circumstances to his child's earnings trajectories. Thus, we provide evidence that adverse shocks to such circumstances can reduce children's later-life outcomes, contributing to an ongoing debate on whether and how manipulable household conditions 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 et al., 2014; Black et al., 2014; Dahl, Kostøl, Mogstad, 2014; Persson and Rossin-Slater, 2014; Black et al., 2015; Cesarini et al., 2015; Chetty and Hendren, 2015; Manoli and Turner, 2015, Rossin-Slater and Wust, 2015).[9]

[8] See, for example, Angrist (1990); Dobkin and Shabani (2009); Angrist, Chen, and Frandsen (2010); Duggan, Rosenheck, and Singleton (2010); Angrist and Chen (2011); Angrist, Chen, and Song, (2011); Autor, Duggan, and Lyle (2011); Lindo and Stoecker (2014); Coile, Duggan, and Guo (2015); and Autor, Duggan, Greenberg, and Lyle (2015).

[9] 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,

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 direct evidence of familial transmission of military service (Kleykamp, 2006; Campante and Yanagizawa-Drott, 2015).[10] The large enlistment increases we detect demonstrate that inducing one generation into military service may make it easier to recruit subsequent generations for the military, which bears implications for the persistence of conflict. Moreover, our findings imply a quite generalized increase in attachment to the military.

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 evolution of socioeconomic mobility over time and space (Gee, Gerald, and Turner, 2013; Chetty, Hendren, Kline, and Saez, 2014; Chetty, Hendren, Kline, Saez, and Turner, 2014; Mitnik, Bryant, Weber, and Grusky, 2015). Our paper helps inform the extent to which tax policy can address this phenomenon 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 occupations transmit across generations (Dal Bó, Dal Bó, and Snyder, 2009;

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).

[10] 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 correlate 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 wartime service. They recover a large increase in son wartime military service (although a net zero effect on son military service across war and non-war eras).

Hellerstein and Morill, 2011).[11] (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.

II. 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[12] 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.[13] 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. Note that because we rely on tax filings to identify father-child 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

[11] 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.

[12] 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).

[13] In the appendix, we also look at the other Vietnam draft lotteries, namely for cohorts born in 1948-1950 and 1953, to examine robustness across the different draft lotteries. Randomization errors in the 1948–50 cohorts draft lottery (Feinberg, 1971), and the lack of a binding draft for the 1953 cohort lead us to exclude them from our main analysis.

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). Finally, we discuss and explore at the end of Section 3 the implications of being unable to observe some sons born too early to be claimed in 1996.

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 enlistee to the specific years he served, which allows us to delineate 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 observe postsecondary education pursued over our full analysis 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., draft numbers) and, to relate draft eligibility to military participation, a Defense Manpower extract. 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] 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 82% of sons in the control sample (77% of daughters) worked in 2013, and their average income (including zeros for nonparticipants) was $37,082.86 ($26,700.66 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 unconditional 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.

Before we turn to our main exercise, we confirm that use of the Vietnam draft lottery does not introduce any sample selection issues across survival—or because we rely on tax records—tax-filing and 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 lottery generated comparable numbers of draft eligible and non-draft eligible 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 selection bias induced by differential rates of filing a return.[15] 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,

[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.

[15] This result is consistent with prior work finding no evidence that variation from the Vietnam draft lottery introduces survival bias. We explore this issue further using data from the National Archives Defense Casualty Analysis System and aggregate the number of Vietnam-era servicemen fatalities to the date of birth level. We find that draft eligibility leads to an insignificant 0.18 increase in fatalities (with a standard error of 0.11). Few men induced into military service by draft-eligibility in the 1951 and 1952 cohorts were exposed to combat.

and the number of dependents of each sex. Across all of these tests, we can rule out even small amounts of bias, lending credence to the validity of our empirical design. Later, when exploring mechanisms underlying our results, we show there is no evidence that draft-eligible men are differentially selected on the basis of labor market measures into having dependents in our sample.

Finally, in Appendix Table 1, we investigate differences by draft eligibility among fathers in reported total income in 1996; whether they filed as married; the average income in their zip code; and, using the estimates from Chetty and Hendren (2015), the percentage gain in income from spending an additional year of childhood in their respective county. Any observed differences here do not pose validity concerns with our design but will help us contextualize our results.[16] We find evidence that draft-eligible fathers are slightly more likely to live in worse neighborhoods but no other meaningful differences.[17] Our results are consistent with Angrist and Chen (2012), who find no difference in earnings or marriage rates between draft-eligible and non-draft-eligible men by the time they were sampled in the 2000 census.

III. Main Results: Sons' Earnings and Military Service

We would like to estimate how draft eligibility influenced the next generation's labor market. Following Angrist (1990), we exploit the draft lottery, which was randomized within cohorts over birth dates and generated an exogenous shock to the probability of military service among otherwise-comparable men. Our main estimates are thus reduced form effects of a father being called by the draft lottery 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

[16] Imbalance on filing as married could still raise the prospect of selection issues. It could also complicate the later comparison of mothers we undertake. However, we can rule out even small differences in the proportion filing as married.

[17] Draft-eligible fathers are slightly more likely to live in lower-income zip codes (0.2%, representing about $70) and downward mobility counties (representing a 0.0019% decrease in income per year of county childhood exposure).

derive illustrative Wald estimates of the effect of service.[18] 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.

A. Father's Military Service Outcomes

In Appendix 1, we derive a correspondence between father's draft eligibility and military service, and we estimate that eligibility increased the likelihood of 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 is designed to capture 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 men in our sample (i.e., those who filed and claimed a dependent in 1996) than for the overall population.[19]

Moreover, for these estimates to represent the causal effect of service, it must be the case that a son's earnings (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,

[18] 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.

[19] 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.

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, all else equal, would likely lead to higher earnings (which would work against the direction of our result). 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 lottery 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.

B. 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

$$y_{s,c} = \beta_0 + \beta_1 * eligible_{s,c} + \gamma_c + \varepsilon_{s,c} \quad (1)$$

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,[20] (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, γ_c, 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, β_1 is an estimate of the causal

[20] To reduce the influence of outliers, we winsorize the earnings measure at the 99th percentile of control group sons but show in the appendix that the earnings results are similar if raw earnings are used (slightly more negative, less precise, and still significant at 5%). Further, draft eligibility is insignificantly correlated with being in the top percentile.

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 $267.91 less than untreated sons (i.e., sons whose fathers were not draft eligible), lowering their income by 0.72% of the mean (Table 2, left column). This figure reflects two negative labor market outcomes for these men: first, they were 0.14 p.p. less likely to work in 2013, and, second, conditional on working, their log earnings were 0.6% lower than untreated sons. The level earnings and log-transformed results are significant at 1% ($p = 0.001$ and $p = 0.010$, respectively); the participation result is significant at 5% ($p = 0.046$).

The scaled estimates suggest that having a father who *served* lowered a son's earnings in young adulthood by more than $2,000 (a 5.71% reduction), labor market participation by 1.4 p.p., and conditional earnings by 4.96%. 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 ($34.88) and participation (0.15 p.p.), but these increases are fully eclipsed in our aggregate measures by large decreases on the civilian side of the labor market ($309.18 and 0.24 p.p., respectively). Further, conditional on working in the sector, civilian earnings are a statistically significant 0.74% lower among treated sons, but the military earnings differential is indistinguishable from zero. We examine military participation further in the next table.

C. 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.

Sons of draft-eligible fathers are 0.26 p.p. more likely to serve in the military than untreated sons (Table 3, left column). Moreover, sons of draft-eligible fathers serve for about 0.0158 year longer (equivalent to about 6 days) than untreated sons (Table 3, right column). Results are statistically significant at 1% ($p < 0.001$).[21]

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 duration of his son's service by 0.1245 year. Putting these numbers in context, about 8% of untreated sons enlist, and their 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%.

We next explore 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).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)). 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 (Appendix Table 3). We estimate that treated sons are also a bit more

[21] We also consider variants of this specification in Appendix Table 3, which we describe in more detail in Appendix 2. When we examine the effect on years of military service, conditional on serving, estimates are indistinguishable from zero. When 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, our results do not materially change.

likely (0.03 p.p.) to hold a civilian military position in the VA or DOD than untreated sons. Thus, it appears that treated sons are generally more attached to national defense work.

We also investigate 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, noting the caveat that many sons in our sample were too young to serve in the nonwar period. We estimate a 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). Relative to the average enlistment rate over each period, the peacetime estimate actually represents a larger percent increase than the wartime estimate.[22]

D. Heterogeneity

We investigate heterogeneity in effects by parental income (measured in1996) and sons' earnings (measured in 2013).[23] First, we probe in Appendix Table 4 the extent to which particular parent-income groups drive our estimates.[24] 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.[25] When we flip the question around and probe whether sons are differentially likely to earn above different earnings percentiles (Appendix

[22] Campante and Yanagizawa-Drott (2015) estimate father service leads to a 6.7 p.p. increase in the likelihood a son serves during wartime, which is fully offset by a 7.6 p.p. decrease during peacetime, and consequently rule out occupational choice in favor of a strong culture of war service as the key mechanism underlying their transmission. Our results hold across war and peace, suggesting more scope in our setting for occupational choice to transmit across generations (perhaps due to a broad culture of service).

[23] 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).

[24] 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 4, bottom panel).

[25] The lack of an earnings effect at the top of the distribution is consistent with wealthy families providing insulation from negative shocks. We do not take this as evidence of strict avoidance at the top of the income distribution because of the large increase in son's military service that we also detect among this group.

Table 5), 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.

E. Labor Force and Military Outcomes among Daughters

Table 5 re-estimates our main outcomes among daughters. We obtain an earnings effect for daughters of $106.32 ($p = 0.043$), which is almost 40% the size of the estimate for sons. In addition, the labor force participation effect is indistinguishable from zero. When we consider log earnings, which omits nonparticipants, the estimate for daughters (0.5% lower, $p = 0.047$) 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 0.5 to 0.6%.[26]

Turning to the service results, fathers' draft eligibility, on average, increases daughters' military outcomes—participation by less than 1 p.p. and duration by 0.0044 year. 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 6, we present the results from pooling together the sons and daughters

[26] Differences between sons and daughters in selection into the workforce may affect the interpretation of the log estimates.

samples. Unsurprisingly, the results are extremely significant with the exception of labor force participation.[27]

F. Robustness

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 8 and 9, 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 rather than noncitizens (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 raw earnings. Across all of these robustness checks, our estimates are extremely similar to those presented in the main text.

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 age, exploiting 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

[27] 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 7). 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.

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.[28]

Turning to Appendix Table 10, 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));[29] 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.

In Appendix Table 11, 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 more binding the draft (i.e. the larger the effect of draft-eligibility on the cohort's military service), the more positive the effect on son military service and the more negative the effect on son earnings. Altogether, this array of results lends credence to our main conclusions.

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[28] These figures are unbalanced and could therefore be loading treatment heterogeneity by both son year of birth and the age at which we examine them. We find similar patterns when we re-estimate the effects separately by age while holding constant the sons we examine.

[29] 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 (or other mechanisms).

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 1996, which, for example, includes sons born during the draft lottery years we examine. 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.[30] To explore this issue further, we test in Appendix Table 12 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 only sometimes significant. Note that this set of results would suggest that the effects we detect likely represent lower bound estimates of the overall effects. 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.

Another 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, and produce an estimate of how many children might be missing. The exercise indicates that approximately only 5% are missing. In addition, in Appendix Table 13, 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.[31] Across all outcomes, the difference is

[30] Using 1987 as the first year will slightly understate the proportion missing, since some children born before the father was, on average, 17.5 years old would not have been eligible to be claimed on returns from 1987 or later. These less than 0.1% Statistics of Income samples are far too small to meaningfully estimate the effect of draft eligibility on sons' earnings and military service.

[31] Kidlink is also more likely to include noncustodial biological children compared with tax filings that would include custodial nonbiological children.

always statistically and economically insignificant, suggesting nonfilers are not affecting the generalizability of our results.

IV. 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. Differences in these outcomes could help explain why a father being called by the Vietnam draft lottery, on average, translates into lower earnings and increased enlistment for his son.[32]

First, we investigate whether a father's draft eligibility reduced his son's 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 a 0.022 year decrease in work experience, suggesting that his service induces about a 0.171 year decrease (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 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

[32] Note that the underlying mechanisms (parental circumstances) of our main results are likely generating these intermediate outcomes as well.

education among their children if there is an intergenerational transmission of education. However, the negative earnings effect documented earlier would make a decrease 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, while perhaps helping to explain some of the differences in earnings we estimate for 2013, raises additional questions regarding how the additional time outside the labor market was spent.

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.[33] Finally, we find no evidence that treated sons are more or less likely to receive unemployment insurance payments in 2013.

Then, in the bottom row, we examine differences in family structure. 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, treated 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 other words, these results suggest that, at early stages of young adulthood, differential rates of human capital investment may be contributing to the later-life differences we observe.

V. Discussion of Mechanisms and Interpretation

In Appendix 3, we explore possible mechanisms that explain our main results. The purpose is to identify which mechanisms, of those available, are most consistent with the data. Many of the analyses we undertake are descriptive in nature; thus, we cannot determine with certainty which hypotheses are correct. This section summarizes our analyses and corresponding findings from the appendix.

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 potentially lower-skilled mothers made them genetically less able;[34] (2) their increased enlistment rate lowered their earnings; (3) different household environments as children led them to have lower reservation wages or develop skills or preferences for lower-paying (non-military) sectors; or (4) lower

[33]VA disability compensation and Supplemental Security Income are generally not taxable and therefore cannot be observed in the data. Especially given the higher enlistment rate among draft-eligible sons, we cannot rule out that they may have slightly higher overall rates of disability.

[34] Given the random nature of the draft lottery and the sample balance documented in Table 1, it is unlikely for there to be genetic differences between fathers. As mentioned earlier, we also find no evidence that draft-eligible men were differentially selected on the basis of labor market measures into having dependents in our sample (Appendix Table 15). More discussion of this issue can be found in the Appendix.

parental inputs, defined broadly, reduced their general human capital.[35] Note that (non-genetic) parental circumstances could drive (2)-(4). To the extent possible, we attempt to analyze each explanation in isolation so that (4) is the effect of parents exclusive of any transmission of preferences and/or job-specific human capital that underlie (2) or (3).

We reach several conclusions. First, we do not find any evidence consistent with a genetic explanation: we can rule out even small differences in mothers' earnings, labor force participation, and disability rates by draft eligibility (Table 7).[36,37] As a result, we surmise that the earnings effect is driven by environmental factors brought about by parents' circumstances, rather than biological determinants. Second, the increased enlistment rate cannot explain much of the lost earnings. At the extreme, if the entire reduction in earnings was driven by the enlistment increase we detect, then military service, on average, would need to reduce annual earnings by an implausibly large amount, on the order of $100,000, which is close to 300% of the mean earnings of control sons. Third, neither a lower reservation wage nor a proclivity toward lower-paying sectors appears to be driving earnings down. In Table 2, we had found reduced labor force participation and earnings conditional on working, which are inconsistent with lower (or higher) reservation wages alone. In Appendix Table 16, we do not find much evidence that treated sons differentially sort into lower-paying industries. By elimination, parental inputs appear to drive a large portion of our earnings result. While the children we study were too young to be affected by the draft directly (very few children claimed in 1996 could have been born before the Vietnam War was over), prior literature has documented various setbacks among

[35] As noted earlier, we take parental inputs to mean the quantity and quality of parental time and resources that influence children as well as any indirect effects of these inputs.
[36] We also find mothers are observationally equivalent in other measures available from the tax data (i.e., there are no differences in the proportion still alive by 2013, the proportion that are citizens, or age).
[37] The lack of evidence of assortative matching in our setting is, in itself, interesting. We might expect, given their relatively low income in the decades following the war, that draft-eligible fathers would match with lower-skilled mothers. Still, at least one paper has concluded that the additional education received by returning (WWII) veterans sorted men into higher-skilled wives (Larsen et al., 2015).

draft-eligible men that may have impaired their sons' human capital development.[38] Several of our findings offer indirect evidence consistent with this narrative. For instance, prior work documented lower earnings in the decades after the war that faded over time, so that the children born earliest within our setting likely grew up with the fewest resources; indeed, we estimate larger decreases among sons born earliest (Appendix Table 12).[39] In addition, draft-eligible fathers may have spent less time with their children, as suggested by their higher labor force participation rates later in life (Appendix Table 17) and increase in career military service (Appendix Table 18).[40] Last, treated sons resided in lower-mobility counties in 1996, pointing to small indirect neighborhood effects (Appendix Tables 1). And while our data do not allow us to examine it directly, we cannot rule out important differences in the quality of parenting, as suggested by—or even potentially brought about by—the higher incidence of behavioral and physical health disorders, and enrollment in disability among draft-eligible men (Appendix Table 18 as well as prior work).

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,[41] and (2) fewer (or lower-paying) civilian labor market opportunities raise the relative returns to military service. Enlistment decisions tend to be inversely related to socioeconomic background (Hosek and Peterson, 1985; Kilburn and Klerman, 1999; Kleykamp, 2006); because prior literature has documented lower earnings

[38] The samples on which these estimates are based are not exactly the same as the one used in this paper. If the effects of draft eligibility were, in fact, heterogeneous, our estimates may not be directly comparable to those from earlier studies.

[39] The difference in earnings by year of birth is robust to dividing earnings by the control group mean or standard deviation for each year of birth before running the regression. When we separately look by the different ages at which we can observe the son and interact year of birth with draft eligibility, we find larger and sometimes significant effects for those born earlier.

[40] Fathers may have worked more given the income effect (from lower prior earnings), and men in the military may have spent less time with their children if they relocated frequently or were stationed overseas and sometimes were away from their families.

[41] 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. 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—and surmise that these efforts likely would be driven by higher proclivity among these children to enlist.

among draft-eligible men in the 1970s and 1980s and we show sons in our sample have lower earnings, our focus is on whether the second 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 induces voluntary enlistment in the next, which, in and of itself, is a novel result.

First, 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. Second, 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 untreated sons. 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 19). 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 estimate for the relative effect of draft eligibility on military service during the Great Recession is statistically significantly lower than during other years in our sample (Appendix Table 20). Finally, the extent to which different military branches drafted servicemen was uneven across cohorts. We exploit this variation, investigating whether a son's branch of service systematically varies with his father's likely branch, and find some support for a transmission of branch in the data (Appendix Table 21). We take the sum of this evidence to imply that, in addition to whatever

reduced labor market opportunities that sons of draft-eligible 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.

VI. Conclusion

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 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 less education—and we conclude by investigating the various channels that may give rise to our findings. Finally, and more broadly, our results demonstrate that policies that affect one generation can have effects on subsequent generations.

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 the Vietnam-era draft lottery affecting fathers during their sons' formative years. This finding underscores the strong role that manipulable parental 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.[42] 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 expect transmission to be low (e.g., randomly assigned risk of service in an unpopular war). Our estimates suggest, assuming no spillovers, had all men from these two cohorts been called by the lottery, we would anticipate 12,770 additional voluntary enlistees down the road. If we can also presume the assumptions underlying our Wald estimate hold, had the lottery induced all men in these two cohorts to serve, all else equal, we would expect over 100,000 future enlistees owing to conscription alone. Moreover, since conscription compels people into service in spite of better outside options, the increase in the likelihood a son enlists that we detect might represent a lower-bound estimate of the transmission of military service.

More 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 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.

[42] Of course, prior studies have concluded that earnings differences between Vietnam draft-eligible men and their peers in young adulthood fully eroded over time. While it remains to be seen how sons of draft-eligible fathers fare in the longer run, given the wealth of evidence that earnings in young adulthood are fairly predictive of lifetime earnings, it seems most likely the source of the early earnings differences for draft cohorts were context-specific and narrowed at least partially from the benefits and services offered to conscriptees after service.

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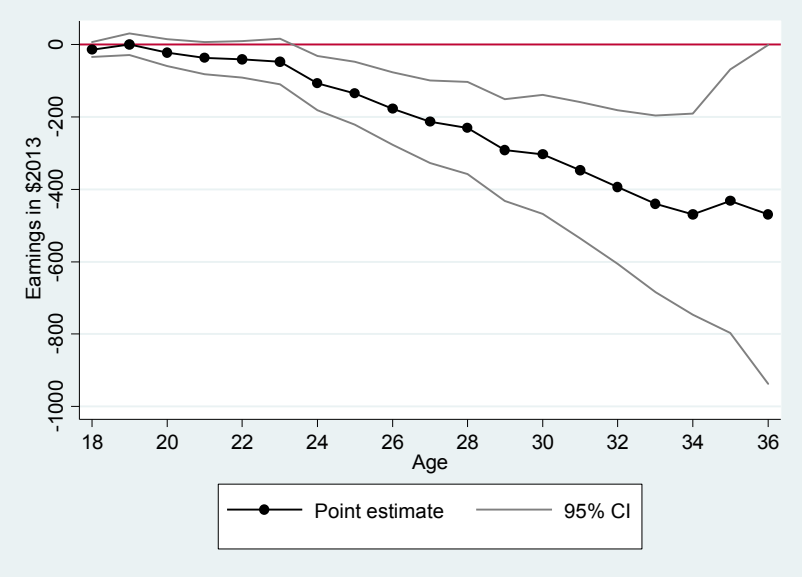
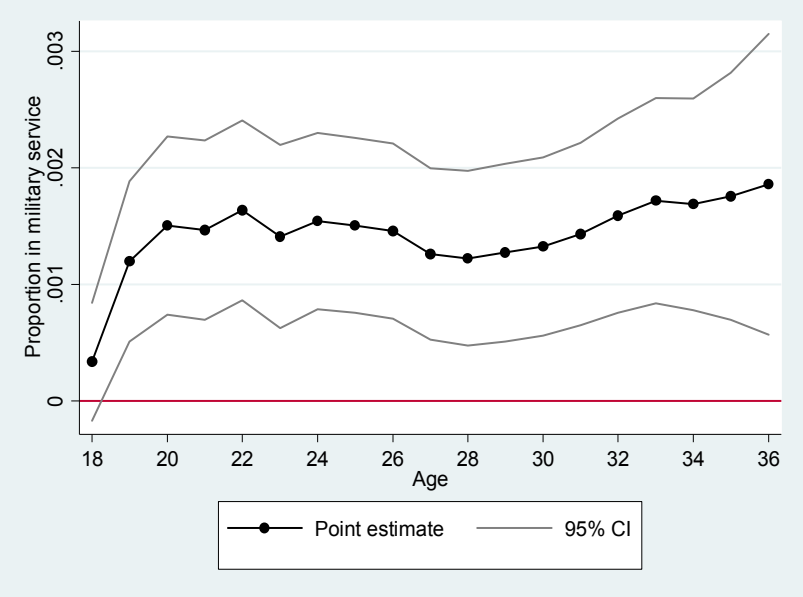
Figure 1a and Figure 1b: Effect of father's draft eligibility on son's earnings (1a) and military service (1b) at various ages (1999–2013)





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 data point represents a separate regression that uses all observations between 1999 and 2013 when the son was a given age; the 18 regressions per figure are therefore unbalanced. Sample sizes range from 277,780 to 1,795,169. We find similar patterns when we re-estimate the effects separately by age while holding constant the sons we examine. Standard errors are clustered at the father date of birth level.

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
Draft eligibility	-20.37	-0.0034	-0.00002	0.0003	0.0011
	(27.04)	(0.0046)	(0.00011)	(0.0008)	(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
Draft eligibility	0.0011	0.0008	0.0002	0.0010	0.0009
	(0.0016)	(0.0006)	(0.0010)	(0.0063)	(0.0010)

Notes: The table presents estimates from 10 separate 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		
	(1)	(2)	(3)
Category	Overall	Civilian	Military
Panel A: Earnings			
Draft eligibility	-267.91	-309.18	34.88
	(81.94)***	(82.34)***	(7.84)***
Wald estimate	-2116.95	-2443.07	275.63
Control group mean	37,082.86	36,151.98	767.47
Panel B: Any job (earnings > 0)			
Draft eligibility	-0.0014	-0.0024	0.0015
	(0.0007)**	(0.0007)***	(0.0003)***
Wald estimate	-0.0109	-0.0187	0.0117
Control group mean	0.8150	0.7940	0.0316
Panel C: Ln(earnings)			
Draft eligibility	-0.0063	-0.0074	-0.0035
	(0.0024)***	(0.0024)***	(0.0082)
Wald estimate	-0.0496	-0.0587	-0.0273
Control group mean	45,501.22	45,533.97	24,249.29

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 1,754,035, 1,708,183, and 69,000, 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	
	(1)	(2)
Outcome	Served in the military	Years of military service
Draft eligibility	0.0026	0.0158
	(0.0004)***	(0.0032)***
Wald estimate	0.0209	0.1245
Control group mean	0.0765	0.4878

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)*

Outcome	Coefficient (SE) on draft eligibility			
	(1) Active duty	(2) Reserve duty	(3) Not during wartime (1999–2000)	(4) During wartime (2001–13)
Draft eligibility	0.0025	0.0011	0.0013	0.0024
	(0.0003)***	(0.0002)***	(0.0002)***	(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
Draft eligibility	-106.32	0.0003	-0.0049	0.0006	0.0044
	(52.40)**	(0.0007)	(0.0025)**	(0.0002)***	(0.0014)***
Wald estimate	-840.15	0.0027	-0.0385	0.0044	0.0349
Control group mean	26,700.66	0.7690	34,722.96	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 $N = 1{,}593{,}008$. *** 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)
Draft eligibility	-0.0216	-0.0291	0.0001	0.0002	-0.0012
	(0.0096)**	(0.0097)***	(0.0058)	(0.0009)	(0.0006)*
Wald estimate	-0.1705	-0.2299	0.0010	0.0020	-0.0094
Control group mean	9.7456	9.4859	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 $p < 0.01$.

Table 7. *Effect of draft eligibility on mother labor force measures (1999)*

	Coefficient (SE) on draft eligibility				
Outcome	(1) Mother on 1996 return	(2) Earnings	(3) Any work	(4) Ln(earnings)	(5) Any disability insurance
Draft eligibility	0.0001	-19.18	0.0001	0.0005	0.0002
	(0.0006)	(59.66)	(0.0010)	(0.0028)	(0.0002)
Wald estimate	0.0005	-151.57	0.0007	0.0040	0.0014
Control group mean	0.9024	25,739.57	0.7244	35,533.57	0.0134

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)–(5), 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 (5), and N = 1,407,872 in column (4). *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

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).[43] 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

$$service_c = \beta_0 + \beta_1 * eligible_c + \varepsilon_s$$

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 draft eligible served, so draft eligible 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).

[43]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)).

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

In Appendix Table 3, we consider other military outcomes of interest. First, 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)) (top panel, left 3 columns). 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.[44]

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.

[44] On the other hand, there is a small negative correlation between draft-eligibility and a variable constructed as whether the son served in either the Coast Guard or the Marines. Because of confidentiality concerns, we report branch-specific results only for employment categories associated with more than one EIN (whereas the Coast Guard and the Marines are each represented by only one EIN).

Finally, we consider variants on our main specification that capture slightly distinct concepts (bottom panel). 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.

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 could underlie our earnings result and then turn to those that could explain our enlistment result.

We consider the following mechanisms to be a non-mutually exclusive list of why sons of draft-eligible fathers may earn less than their peers: (1) inherited traits from potentially 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 develop skills or preferences for lower-paying sectors; or (4) lower parental inputs reduced their general human capital.[45] We investigate each in turn.

First, we investigate whether the effect is driven by genetic factors. Before turning to mothers, we begin by discussing the possibility that, even though draft lottery numbers were randomly assigned across men, the genetic endowments of *fathers* may differ. At first blush, this seems unlikely given that we found no evidence that draft-eligible men were more or less likely to appear in our sample (Table 1). But in theory, there could be non-random selection on the basis of which men show up as fathers in our sample, which could pose a problem if, for instance, draft-eligible men who are predisposed to be lower skilled are more likely to claim children in 1996, and this increased likelihood is exactly offset by higher-skilled draft-eligible men being less likely to claim children that year. To investigate this further, we examine in Appendix Table 15 whether the difference in 1999 labor market measures between draft-eligible and non-draft-eligible men varies by whether they appear in our father sample. The interaction

[45] There may be other explanatory factors that we have not listed that could also explain this result.

between draft-eligibility and dependents claimed is insignificant across outcomes, which is inconsistent with genetic differences between fathers in our sample.[46]

Turning to mothers, 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 earnings and other labor market measures of the mothers of draft-eligible sons (Table 7). First, we reproduce the earlier evidence of balance 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, labor force participation, and disability insurance receipt measures, we find no evidence this is so and can rule out even small differences between the groups.[47] 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.[48] For instance, military enlistment may have generated some of the reduced civilian work experience 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 $268 reduction in earnings per son is implausibly high: each son induced to enlist must have suffered a $103,042.31 setback in his 2013 earnings, a value several times the mean earnings of sons of non-draft-eligible fathers

[46] Differences of this form would not necessarily pose a threat to our analyses, as they could also reflect heterogeneity in the effect of draft eligibility.

[47] 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. Additionally, given the extremely precise null results on mothers, it is unlikely that different effects are exactly offsetting each other.

[48] While parental circumstances, 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).

($37,082.86). 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 will not 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 $5,000 (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 general 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 the intention to join the military.

Third, treated sons may have lower reservation wages or may have developed specific skills or preferences for lower-paying (nonmilitary) work, perhaps because, as noted in findings from prior literature, fathers suffered earnings losses in the decades after Vietnam. (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 or skillsets 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).[49] To explore whether the earnings result is driven by treated sons having a proclivity towards lower-paying jobs, we estimate the intensive 2013 earnings effect of draft eligibility

[49] On their own, differences in reservation wages could therefore at best explain only a portion of the result.

with and without six-digit North American Industry Classification System (NAICS) industry controls for their highest-paying job (Appendix Table 16).[50] We find that the earnings effect is insignificantly lowered by less than 15%. To account for any bias introduced by treated sons who otherwise earn less sorting into specific industries, we separately calculate industry-simulated earnings. First, we calculate mean earnings among non-draft-eligible sons by industry. We then regress this measure on draft eligibility and fail to find a significant relationship. These estimates, if anything, may overstate the difference, given that (1) treated sons may work in certain industries in the absence of this mechanism if they have lower earnings potential (given that certain industries pay less); and (2) the estimate mechanically includes any fixed differences in earnings in the military. Nonetheless, we cannot conclusively rule out this mechanism (e.g., there could be sorting across occupation that cannot be measured with tax data and will not perfectly overlap with detailed industry); still, such a mechanism would ultimately indicate that an environmental factor brought on by parents' circumstances is driving our results. Altogether, the data we have yield little evidence in support of the hypothesis that preferences for or skillsets linked to 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. As discussed earlier, we take parental inputs to mean the quantity and quality of parental time and resources that influence children as well as any indirect effects of these inputs, as opposed to effects of parents on preferences or job-specific skills that underlie mechanisms (2)-(3). This setback in earnings could not have been the result of the draft directly (including father absenteeism due to Vietnam 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.[51] The two-stage least squares earnings losses of 15% in the 1970s

[50] We note the limitation that the sample is selected due to an extensive margin work response.
[51] The higher levels of education among draft-eligible men (Angrist and Chen, 2011) are likely working against our finding.

and 10% in the 1980s from early work are consistent with a resource channel.[52] Further, the fadeout in the earnings gap suggests that the children born earliest grew up with the fewest resources, and indeed, our results by son's age indicate that the children born earlier experienced larger decreases in earnings (Appendix Table 12).[53] In addition, draft-eligible fathers may have spent less time with their children. Several of our findings offer indirect evidence consistent with this narrative. We can observe that draft-eligible fathers were slightly more likely to work in 1999 (Appendix Table 17), perhaps due to lower levels of wealth accumulated earlier in life, and that extra time working could have come at the expense of time with their children.[54] We also find 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 18), which requires at least 20 years of military service. A military career may be indicative of less time spent with children if fathers had to relocate frequently and/or were stationed overseas and did not bring their families with them. (In related work, children's academic achievement has been shown to suffer when fathers are deployed ((Engel, Gallagher, Lyle, 2010; Lyle, 2006)).[55] And while our data do not allow us to examine it directly nor are we aware of any studies with such a finding in this

[52] We note that the samples from prior work 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. Though as noted before, we do not find any evidence that men are differentially selected into our sample on the basis of 1999 labor market characteristics. Still, if the effects of draft eligibility are heterogeneous, our estimates may not be directly comparable to those from earlier studies.

[53] The difference in earnings by year of birth is robust to dividing earnings by the control group mean or standard deviation for each year of birth before running the regression. When we separately look by the different ages at which we can observe the son and interact year of birth with draft eligibility, we find larger and sometimes significant effects for those born earlier.

[54] There is no measure for hours worked in the tax data. Angrist and Chen (2011) find no difference in working or hours worked, though the confidence intervals are consistent with small differences (and the samples do not perfectly overlap).

[55] 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 several additional ways. First, the possible frequent relocation of children may have detrimental effects on them and generated down-the-road earnings losses. 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.

context, we cannot rule out important differences in the quality of parenting, as suggested by—or even potentially brought about by—the higher incidence of behavioral and physical health disorders, and enrollment in disability among draft-eligible men (Appendix Table 18 as well as prior work). Also suggestive, prior work (Lindo and Stoecker, 2014) found some evidence that draft eligibility led to increases in later life incarceration for violent crime (while decreasing incarceration for nonviolent crime, with no overall effect on incarceration).[56] Finally, the results from Appendix 1 point to indirect effects contributing to the lower earnings we detect. Namely, we find evidence of small neighborhood effects, though they appear to only explain a fraction of the earnings reduction.[57] 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.[58] Future work should do more to unpack how and why the draft induced shocks to parental circumstances 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.

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[56] While not directly about draft-eligible fathers, a high incidence of drug abuse has also been documented among Vietnam serviceman (Robins, Davis, and Goodwin 1974).

[57] The negative effect on the percentage gain in income from spending one more year of childhood in the county in which we observe each son in 1996 (from Chetty and Hendren 2015) multiplied by 18 would generate a $14 reduction. Neighborhood effects could be attenuated given that we cannot *a priori* identify the most relevant level of neighborhood that matters (which may operate through peers, schools, crime, labor demand, etc.). Controlling for county or zip code fixed effects in our main specification lowers the earnings estimate by about 10 or 20 percent, respectively.

[58] Our data are also inconsistent with some other channels. For example, we find no evidence of any differences in family structure (Appendix Table 1).

First, 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.

Second, 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 risk of conscription, albeit indirectly).[59] Nonetheless, to explore whether lower opportunities are likely to explain much of the effect, we undertake some suggestive exercises to produce a range of (naïve) elasticity estimates and assess whether they could explain our results (Appendix Table 19). 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.[60] 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.[61]

[59] 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).

[60] There may be nonlinearity in the relationship. Moreover, the estimate cannot be interpreted causally, since there are possible omitted factors correlated with income that influence enlistment.

[61] 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).

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 1987–1996 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.[62]

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 $268 decrease in earnings (our estimate on the earnings response) is associated with a 0.00002 increase in military service. (Recall that our 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 $34,840 to explain the effect (or that the influence of military service among the compliers must be an implausibly large *positive* number),[63] 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

[62] 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).

[63] This final elasticity exercise uses level earnings instead of log earnings so that the magnitude can be compared with our main effect on earnings.

substantial downward bias in these elasticities for the first channel to fully explain our military result.

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 20, 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.[64]

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 in 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 21, we find that

[64] 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.

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.[65]

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 higher likelihood of military 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

[65] 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.

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)	(5) Causal County Effect (C&H)
Draft eligibility	-171.92	-0.0022	-0.00002	-0.0018	-0.0019
	(154.11)	(0.0022)	(0.00056)	(0.0009)*	(0.0007)**
Control group mean	69,600.85	70,032.58	0.9040	39,644.55	-0.0269

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. "Causal county effect" represents the percentage gain in individual income at age 26 from spending one more year of childhood in one's county according to estimates from Chetty and Hendren (2015), averaged between their measurement of families at the 25th and 75th percentiles. 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		
	(1)	(2)	(3)
Father's year of birth	1951	1952	1951–52
Draft eligibility	0.117	0.137	0.127
	(0.004)***	(0.005)***	(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. *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)
Draft eligibility	0.0012	0.0014	0.0005	0.00034
	(0.0002)***	(0.0003)***	(0.0002)***	(0.00017)**
Control group mean	0.0139	0.0339	0.0150	0.0109
Panel B: Other outcomes	(5)	(6)	(7)	(8)
	Years of military service intensive margin	Any son in military	In military (one son in household)	In military (> one son household)
Draft eligibility	-0.0139	0.0031	0.0032	0.0021
	(0.0224)	(0.0005)***	(0.0006)***	(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 4. *Effect of draft eligibility on son outcomes by 1996 family income*

	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: Earnings (2013)						
Draft eligibility	-344.01	-17.33	-310.54	-425.12	-428.95	415.60
	(163.31)**	(134.10)	(110.10)***	(133.98)***	(200.86)**	(296.12)
Control group mean	27,707.90	34,144.79	32,687.46	40,669.26	46,032.07	49,610.16
Panel B: Military service (1999-2013)						
Draft eligibility	-0.0014	0.0017	0.0040	0.0037	0.0015	0.0032
	(0.0012)	(0.0010)*	(0.0009)***	(0.0009)***	(0.0010)	(0.0010)***
Control group mean	0.0624	0.0802	0.0902	0.0842	0.0690	0.0422
Panel C: Pr(quantile)						
Draft eligibility	0.0005	-0.00003	0.0005	0.00053	-0.0004	-0.0011
	(0.0007)	(0.00071)	(0.0009)	(0.0008)	(0.0008)	(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 $p < 0.10$.

Appendix Table 5. *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 > $p(90)$
Draft eligibility	-0.0014	-0.0014	-0.0037	-0.0028	-0.0018
	(0.0007)**	(0.0008)*	(0.0009)***	(0.0008)***	(0.0006)***
Earnings cutoff	0	3,743	27,545	54,056	86,636

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 6. *Primary outcomes for sons and daughters pooled*

	Coefficient (SE) on father military service				
	(1)	(2)	(3)	(4)	(5)
Outcome	Earnings	Any work	Ln(earnings)	Military service	Years of service
Draft eligibility	-188.64	-0.0005	-0.0056	0.0016	0.0102
	(56.05)***	(0.0006)	(0.0019)***	(0.0023)***	(0.0018)***
Control group mean	31,994.57	0.7924	40,375.26	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 $N = 3{,}347{,}043$. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 7. *Effect of draft eligibility by gender*

	(1)	(2)	(3)	(4)	(5)
Outcome	Earnings	Any work	Ln(earnings)	Military service	Years of service
Coefficient (SE) on draft	-161.58	-0.0017	-0.0014	0.0021	0.0113
eligibilityXson	(80.67)**	(0.0008)**	(0.0030)	(0.0005)***	(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 $N = 3{,}347{,}043$. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 8. *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) Un-winsorized (A and C) or probit (B)
Panel A: Earnings						
Draft eligibility	-280.19	2.44	-250.90	-245.15	-241.59	-281.43
	(81.31)***	(252.58)	(79.87)***	(70.50)***	(64.55)***	(127.41)**
Control group mean	37,688.43	26,458.83	36,797.26	37,082.86	37,082.86	38,976.01
Panel B: Any job						
Draft eligibility	-0.0014	-0.0006	-0.0013	-0.0016	-0.0019	-0.014
	(0.0007)**	(0.0034)	(0.0007)*	(0.0007)**	(0.0006)***	(0.0007)**
Control group mean	0.8235	0.6652	0.8140	0.8150	0.8150	0.8150
Panel C: Ln(earnings)						
Draft eligibility	-0.0066	0.0012	-0.0057	-0.0053	-0.0036	-0.0061
	(0.0025)***	(0.0110)	(0.0024)**	(0.0022)**	(0.0018)**	(0.0025)**
Control group mean	45,764.81	39,776.17	45,204.51	45,501.22	45,501.22	47,824.13

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 raw earnings estimates (i.e. earnings is not winsorized) and the "any job" estimate using a probit specification, reporting marginal effects. 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 = 1,676,922, 77,113, 1,846,494, 1,754,035, 1,754,035, and 1,754,035. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 9. *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						
Draft eligibility	0.0029	-0.0014	0.0026	0.0026	0.0026	0.0026
	(0.0004)***	(0.0013)	(0.0004)***	(0.0004)***	(0.0004)***	(0.0004)***
Control group mean	0.0785	0.0407	0.0771	0.0765	0.0765	0.0765
Panel B: Years of service						
Draft eligibility	0.0175	-0.032	0.0150	0.0155	0.0159	0.0158
	(0.0033)***	(0.0090)	(0.0031)***	(0.0032)***	(0.0032)***	(0.0032)***
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 for panel A and a Poisson model for panel B, marginal effects reported. 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 10. *Two-Sample IV for primary outcomes*

	Coefficient (SE) on father's military service				
	(1)	(2)	(3)	(4)	(5)
Outcome	Earnings	Any work	Ln(earnings)	Military service	Years of service
Instrument(s)					
Eligibility	-2,116.97	-0.0109	-0.0496	0.0209	0.1245
	(647.50)***	(0.0055)**	(0.0193)***	(0.0033)***	(0.0254)***
EligibilityXyear	-2,138.95	-0.0112	-0.0502	0.0208	0.1215
	(629.66)***	(0.0054)**	(0.0194)***	(0.0033)***	(0.0248)***
Intervals	-2,151.86	-0.0092	-0.0532	0.0199	0.1171
	(650.61)***	(0.0054)*	(0.0195)***	(0.0033)***	(0.0257)***
IntervalsXyear	-2,014.65	-0.0104	-0.0475	0.0203	0.1215
	(640.13)***	(0.0053)*	(0.0192)**	(0.0032)***	(0.0248)***
Control group mean	37,082.86	0.8150	45,501.22	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 =1,754,035. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 11. *Effect of draft eligibility on son's earnings and military service by father's year of birth (including other draft lotteries)*

	Coefficient (SE) on draft eligibility					
	(1)	(2)	(3)	(4)	(5)	(6)
Father's year of birth	1948	1949	1950	1951	1952	1953
Panel A: 2013 earnings						
Draft eligibility	-177.41	37.78	-274.87	-210.11	-293.08	-81.52
	(115.80	(116.62)	(101.32)**	(97.85)**	(101.90)***	(89.25)
Control group mean	41,980.30	40,226.12	38,583.96	38,230.08	36,196.76	32,497.18
Panel B: Any military service (1999–2013)						
Draft eligibility	0.0005	0.0021	0.0018	0.0023	0.0029	0.0016
	(0.0006)	(0.0006)***	(0.0006)***	(0.0006)***	(0.0006)***	(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 (specifically, the weighted average of the point estimates for whites and nonwhites). 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). 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 12. *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	30.27	0.00020	0.0001	-0.0002	-0.0020
eligibility X son birth year	(10.33)***	(0.00013)	(0.0004)	(0.00007)**	(0.0005)***
Control group mean	37,082.86	0.8150	45,501.22	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 the interaction of son's year of birth with the prior variables. The interaction of son's year of birth with draft eligibility is presented. If the dependent variable is divided by the control group mean or standard deviation for each year of birth before running the regression, all but the earnings result is insignificant (though the signs all stay the same). 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 = 1,754,035. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 13. *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	2.011	-0.0002	0.0005	-0.0003	-0.0001
eligibility X SSA link	(33.76)	(0.0006)	(0.0021)	(0.0003)	(0.0021)
Control group mean	26,189.37	0.8134	32,197.04	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 =2,223,221 except in column (3), where N =1,820,463. *** 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				
	(1)	(2)	(3)	(4)
Outcome	College attendance in 2013	Log $postsecondary quality (1999–2013)	Alive in 2013	Any disability income in 2013
Draft eligibility	0.0004	-0.0008	-0.00001	-0.00002
	(0.0006)	(0.0005)	(0.00023)	(0.00019)
Control group mean	0.1518	38882.21	0.9775	0.0153
Panel C: Transition into adulthood				
	(5)	(6)	(7)	
Outcome	Any UI income in 2013	Number of children fathered as teenager	Total number of children	
Draft eligibility	-0.00004	0.0006	-0.0026	
	(0.00038)	(0.0002)***	(0.0018)	
Control group mean	0.0617	0.0159	0.6749	

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 (through 2013). 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 $N = 1{,}547{,}905$. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 15. *Effect of draft eligibility by 1996 dependent claiming on own labor force measures for all draft cohort men (1999)*

	Coefficient (SE) on draft eligibility			
Outcome	(1) Earnings	(2) Any work	(3) Ln(earnings)	(4) Any disability insurance
Draft eligibility X dependents claimed	-78.97 (57.88)	0.0001 (0.0005)	-0.0013 (0.0012)	-0.0001 (0.0002)
Control group mean	46,963.67	0.7099	66,153.55	0333

Notes: The table presents estimates from regressions of male 1999 earnings and work outcomes on a dummy variable indicating whether the father was Vietnam draft eligible (based on his date of birth), a dummy variable for the man's year of birth, a variable indicating the number of dependents, if any, claimed in 1996 (i.e., number of times each man appears in the sample as a father), and the interaction with prior variables. The interaction of dependent claiming and draft eligibility is reported. The sample comprises all males born between 1951 and 1952. Control group means are derived from values associated with non-draft-eligible males. Earnings are in 2013 dollars. Standard errors are clustered at the date of birth level. $N = 4{,}303{,}632$ in columns (1)-(2) and (4), and $N = 3{,}057{,}344$ in column (3). *** 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
Draft eligibility	-247.23 (84.26)***	-213.29 (77.54)***	-44.33 (29.91)
Control group mean	45,496.74	45,496.74	45,496.74

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. $N = 1{,}743{,}056$. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 17. *Effect of draft eligibility on 1999 father labor force measures*

	Coefficient (SE) on draft eligibility		
	(1)	(2)	(3)
Outcome	Earnings	Any work	Ln(earnings)
Draft eligibility	92.92	0.0021	0.0003
	(165.49)	(0.0008)***	(0.0025)
Control group mean	67,474.59	0.8216	82,123.08

Notes: The table presents estimates from regressions of a father's 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 comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. 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 columns (1)-(2) and $N = 1{,}770{,}364$ in columns (3). *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 18. *Effect of draft eligibility on father receiving a military pension and disability insurance (1999–2013)*

	Coefficient (SE) on draft eligibility	
	(1)	(2)
Draft eligibility	Military Pension	Disability
	0.0130	0.0013
	(0.0004)***	(0.0006)**
Control group mean	0.0329	0.1196

Notes: The table presents estimates from a regression of father military pension receipt and disability insurance 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 at least one Form 1099-R from the military anytime between 1999 and 2013. The disability estimate likely understates the degree to which disability receipt was higher, as draft-eligible men were also more likely to receive veterans disability compensation (Angrist, Chen, and Frandsen, 2011). 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 19. *Association between son military service and family/son income*

	(1)	(2)	(3)
Independent variable	Ln(1996 Family income)	Ln(Family income in childhood)	2013 own earnings/268
Coefficient	-0.0006	-0.0120	-0.00002
(SE)	(0.0001)***	(0.0053)**	(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). We scale earnings in (3) by 268 because it is our draft eligibility earnings estimate. 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 20. *Effect of draft eligibility on son military service in Great Recession versus other years*

	(1) Military service
Coefficient (SE) on draft	-0.0099
eligibility X Great Recession	(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 21. *Effect of draft eligibility on son Marines or Coast Guard service by father draft*

	(1) Military service
Coefficient (SE) on draft eligibility X 1969 draft lottery	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 $p < 0.10$.