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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

Can shocks to one generation propagate to the next? To answer this question, we study how the Vietnam draft lottery affected the next generation's labor market. Using the universe of U.S. federal tax returns, we link fathers from draft cohorts to their sons' outcomes and find that sons of fathers randomly called by the draft 1) have lower earnings and labor force participation than their peers, and 2) are more likely to volunteer for military service. These findings highlight the strong role family plays in human capital development and occupational choice. More generally, our results provide sound evidence that malleable aspects of a parent's life course can influence children's labor market outcomes and that policies that only directly alter the circumstances of one generation can have important long-run effects on the next.

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I. Introduction

Can shocks to one generation propagate to the next? Recent breakthroughs in the data landscape, allowing researchers to link administrative records over time, have markedly advanced two major areas of study within economics that relate to this question. First, new studies estimating the degree of intergenerational mobility in the United States have found little change in the correlation between parents' economic circumstances and their children's over time (Gee, Gerald, and Turner, 2013; Chetty et al., 2014; Chetty et al., 2014; Mitnik et al., 2015). While some of this transmission likely reflects innate factors and immutable aspects of the household environment, there may be other causal channels that can be influenced by policy. Second, other recent studies have been able to tie early-life interventions to significant changes in children's long-run outcomes (e.g., Chetty et al., 2012; Chetty, Hendren, and Katz, 2015), opening up the possibility that well-placed public policy could increase opportunity.

This study bridges the innovations made in these two areas, leveraging randomized variation from the Vietnam draft lottery within the universe of U.S. federal tax returns to examine the intergenerational effects of a considerable shock to parents' circumstances. The analysis reveals that sons of men who were called by the draft have lower labor force participation and earnings, and are more likely to enlist in the military.[2] Thus, we establish that malleable aspects of a parent's life course can influence children's earnings and occupational choices and that policies that only directly alter one generation's circumstances (isolated from genetic factors) can have important long-run effects on the next.[3] Further, we provide evidence pointing to two distinct causal mechanisms that have largely eluded prior quasi-experimental studies: (1) parental inputs as a key determinant of human capital development and (2) the transmission of occupational preferences or occupation-specific skills from fathers to children.[4]

[2] We separately present and discuss estimates for daughters as well, which are directionally similar but smaller.
[3] Important to this conclusion, when we examine the mechanisms underlying our results, we test and find no evidence of skill differences between mothers – i.e., no evidence for assortative matching, which would otherwise be consistent with genetic factors that operate through the mother.
[4] 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.

The Vietnam draft lottery provides a uniquely promising setting for this line of inquiry. First, rarely, if ever, has a randomized policy been implemented on such a large scale, yielding both convincing variation and statistical power. Second, most men had not yet had children by the time the lotteries we examine were conducted, so the children we study were not directly exposed to the draft.[5] Thus, relative to an intervention geared towards families, or even a shock to parents that occurs while children are present, the variation in parents' circumstances that stems from the draft is well isolated. Third, it is well established that, by restricting the set of opportunities available to those with selected birthdates, the experiences of men called by the Vietnam draft lottery differed from those of their peers. Early seminal work linked the lottery to large increases in military service and subsequent earnings losses (Angrist, 1990). Later studies found that the gap in earnings dissipated over the long run (potentially due to service-induced increases in educational attainment) but some evidence of modest differences in health and other outcomes (see Appendix 1).[6]

The analysis identifies the sons of fathers from two key cohorts and compares sons whose fathers were randomly called by the draft lottery to those whose were not, according to the father's exact date of birth.[7] Focusing on 2013 labor market outcomes, when a son's average age was 31 years old, we find that a father's draft eligibility reduced his son's earnings by $268 (0.7% of the mean) and labor force participation by 0.14 percentage point (p.p.). Then, examining military service over the full period we can observe, we find that sons of draft-eligible fathers were 0.26 p.p. more likely to enlist than their peers (3.4% of the mean). To aid in the interpretation of these estimates, which represent the effects of draft *eligibility*, we also derive a

[5] We primarily examine the 1970 and 1971 draft lotteries, which, unlike prior stages of the draft, pertained only to 19-year-old men. We estimate that, within our design and data linkage, we can observe (the youngest) two-thirds of the children born to men in these cohorts. Our estimates are derived from comparisons among those children.

[6] For example, Dobkin and Shabani (2009) find some evidence of early life mental health problems among men called by the draft lottery, and Angrist et al. (2010) find an increased presence of federal transfer income, presumably driven by VA disability compensation. The samples in prior work are similar to ours but not exactly the same, which we describe more fully in the appendix. To the extent possible, we corroborate key findings from the prior literature within our sample; specifically, we show that there are no observable differences in 1996 earnings (or related outcomes) between fathers who were and were not called by the draft and that fathers who were called are slightly more likely to receive (non-VA) disability insurance.

[7] Within our sample, there is no evidence of survival bias or other sample selection bias that could invalidate such comparisons. There is also no evidence of differential fertility.

suggestive scaling factor to translate them into the effects of draft *service*.[8] The scaled estimates imply that a father's draft service reduced his son's 2013 earnings by about $2,000 (6%) and increased his son's enlistment by 2.09 p.p. (25%).

Finally, we leverage the extraordinary detail of the tax data to examine a range of additional outcomes of interest and to evaluate the mechanisms underlying our main results. We find that sons of draft-eligible fathers had less work experience and, while we do not detect differences in undergraduate attendance, there is evidence of reduced human capital along several other dimensions we consider (e.g., college quality, educational attainment, incarceration, teenage fatherhood). We provide evidence that genetics, differential enlistment rates, and other work-related preferences cannot explain the earnings effect and that, instead, it is most consistent with reduced parental inputs lowering sons' general human capital. And, though the opportunity cost of military service may be lower for draft-eligible sons, we demonstrate that a transmission of occupational skills or preferences from one generation to the next explains at least some of the enlistment effect.

The chief new insight from our study is that a shock to one generation can propagate to the next in a number of economically relevant ways.[9,10] In addition, our findings advance several inter-related literatures in economics. First, we contribute an important result—namely, that

[8] The scaling factor is an estimate of the extent to which draft eligibility induced military service among the men from the draft cohorts we consider. The scaled estimates are suggestive in nature for a number of reasons. First, we rely on a separate dataset to estimate service by date of birth among draft lottery cohorts. Second, the exclusion restriction may not necessarily hold if men took action to avoid the draft that separately influenced the outcomes we consider. (For the cohorts we examine, such avoidance was minimal (Card and Lemieux, 2001). We also develop a test that indicates avoidance behavior does not appear to be driving our results.)

[9] Our design credibly purges variation in parents' circumstances of the direct effects that household shocks can have on children who are present at the time. Secondary analysis within a recent working paper supports this conclusion. Examining low-dose nuclear exposure in utero, Black et al. (2014) show some evidence that the effects they find on the first generation persisted into the next – i.e., in some specifications, children whose parents were exposed in particularly sensitive birth months had lower cognitive scores. They do not investigate the role of assortative matching nor do they examine later life outcomes.

[10] Holmlund, Lindahl, and Plug (2011) review the existing literature on the intergenerational transmission of education, which has produced a very mixed set of findings, and conclude that the different approaches in the literature likely violate key assumptions required for identification. One recent paper (Bingley et al., 2016) improves on this concern by exploiting randomized variation from being drafted in Denmark and finds insignificant effects of education transmission. However, given the limited sample size, the confidence interval cannot rule out large effects (several times as large as the OLS regressions). We note that even though the GI bill has been shown to increase education among draft-eligible men, our results point to, on net, *lower* human capital among their sons.

shocks to parents' circumstances can influence children's earnings—to the literature that examines whether household conditions affect children's outcomes.[11] Within that literature, there are two general approaches used for identification. The first exploits the quasi-random assignment of adoptees across different types of households (e.g., Sacerdote, 2007; Fagereng, Mogstad, and Ronning 2015). Estimates from these studies are most directly interpretable as the effects of growing up in different types of households, but they cannot isolate malleable aspects of parents from those that are immutable or from other features of the household; moreover, the approach has not, as of yet, delivered significant estimates of the effects of parents on adult earnings. A second, more common approach that better relates to our strategy derives quasi-experimental variation from shocks to families (as opposed to parents before their children were born)—such as from changes in parental income or job loss—and has delivered mixed short- and long-run estimates for a range of outcomes and contexts.[12] The major empirical challenge of this approach is that the shock must be uncorrelated with other factors that could influence children's outcomes, which can be difficult to validate. While a randomly assigned shock naturally overcomes this challenge, recent studies that rely on random variation have found little to no

[11] A related strand of literature focuses on the long-term effects of interventions. These studies examine policy interventions that not only have direct effects on children or families but are in fact targeted to them, such as those involving early education (e.g., Chetty et al., 2012; Heckman et al., 2010) and neighborhoods (Chetty, Hendren, and Katz, 2015). It is notable that we find large effects in a non-targeted intervention context. Relatedly, Chetty and Hendren (2016) leverage within-family variation in U.S. neighborhoods during childhood and also detect significant long-term effects. Another strand of literature examines the long-term effects of early health and environmental shocks (e.g. Almond, 2006; Isen, Rossin-Slater, and Walker, 2016).

[12] Examples of frontier studies in this area include those that derive variation from household resources (Akee et al., 2010; Milligan and Stabile, 2011; Dahl and Lochner, 2012; Manoli and Turner, 2015; Aizer et al., 2016; Cesarini et al., 2016; Bleakley and Ferrie, 2016). The majority of these papers rely on a difference-in-differences strategy and examine only short-term outcomes (generally finding effects), though Aizer et al. (2016) estimate an effect of an early form of welfare on long-run mortality (and find some evidence that earnings differences may explain the result). Cesarini et al. (2016) and Bleakley and Ferrie (2016) instead rely on randomization and find no effect on either short- or long-term outcomes. Related work has examined the effect of parental job loss. While some studies have found large effects (e.g., Oreopoulos, Page, and Stevens, 2008), recent work by Hilger (2016) calls into question prior approaches and argues that job loss has little to no long-term effects. Two related literatures examine the effects of stress during pregnancy in Sweden – Persson and Rossin-Slater (forthcoming) find an effect on mental health, while Black, Devereux, and Salvanes (2016) find no evidence of an effect on adult earnings – and the effects of two different non-U.S. parental leave policies – Dustmann and Schönberg (2012) find no effect on adult earnings, while Carneiro, Løken, and Salvanes (2015) find an effect.

effect (Bleakley and Ferrie, 2016; Cesarini et al., 2016). Ours is the first study in this area that leverages a random shock and detects an effect.

Second, while the occupational choices of parents and their children are undoubtedly intertwined, our findings offer new credible causal evidence of occupational transmission. As professions typically are not randomly assigned, the existing support for a causal relationship is limited (Dal Bó, Dal Bó, and Snyder, 2009; Hellerstein and Morill, 2011; Campante and Yanagizawa-Drott, 2015).[13] (More generally, there is also little, if any, causal evidence linking a parent's occupation to a child's later-life income.) In our setting, we find that children whose fathers were randomly called by the draft are more likely to enlist in the military themselves, clearly indicating that children's occupations can be directly influenced by their fathers'.

Third, our results reveal material intergenerational consequences of the draft lottery, adding a new dimension to our understanding of the legacy of the Vietnam War and suggesting that potential effects on future generations should be considered in the context of other wars (e.g., Blimes and Stiglitz, 2008).[14] Even though prior evidence suggests that permanent setbacks experienced among those called by the draft lottery were limited, we find that the next generation's earnings were ultimately penalized, perhaps because the early setbacks observed among draft-eligible men occurred over a formative period for their children. In addition, we find increased military enlistment among the children of draft-eligible men, suggesting that the draft may have indirectly made it easier to recruit subsequent generations for the military.

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[13] 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. Campante and Yanagizawa-Drott (2015), using census data to link together fathers and adult sons who live in the same household, exploit variation in a father's age relative to the timing of U.S. wars to quantify how a father's wartime service affects his son's military service. They recover a large increase in son wartime military service (although a net zero effect on son military service across war and non-war eras). Johnson et al. (2016) examine military service transmission within the Minnesota Twin study and find directionally similar effects (albeit an order of magnitude larger).

[14] 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).

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

United States 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 and assign them their respective draft number and eligibility based on their exact dates of birth.[15] Those subject to the lotteries of 1970 and 1971 faced the prospect of being drafted in 1971 and 1972, respectively.[16] 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 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 (approximately one-third) born too early to be claimed in 1996.

To examine children's 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

[15] 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), which will not bias our results, but could mask some heterogeneity by race.

[16] In the appendix, we also look at the other national 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.

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), about 10 of which represent a military employer. Thus, we can 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).[17] We observe disability insurance receipt from Form 1099-SSA, military pension receipt from Form 1099-R, 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. 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

[17] We supplement the information available on Form 1098-T with the universe of postsecondary enrollment, grant, and completion records from the Department of Education's National Student Loan Data System (NSLDS), the central database for recipients of federal student aid. There is no statistical difference in the likelihood the sons within our sample differentially match to these data (p-value is 0.982).

fathers have multiple children, there are 2,178,651 unique fathers in our sample. Comparing to Census tabulations, these fathers constitute approximately 60% of men from these cohorts.

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. (We also find no evidence of survival bias when specifically examining Vietnam-era service fatalities.[18]) Next, we confirm that there is no differential probability that a man on each side of the draft appears in our sample (i.e., claims at least one dependent). We then test for selection in the sex of the dependents, the number of dependents, and the number of dependents of each sex. Across all of these tests, we can rule out even small amounts of bias, 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 (2016), 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. We find evidence that draft-eligible fathers are slightly more likely to live in worse

[18] Namely, we explore this issue further by using data from the National Archives Defense Casualty Analysis System and aggregating 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. This result is consistent with prior work finding no evidence that variation from the Vietnam draft lottery introduces survival bias in the long-run (Conley and Heerwig, 2012).

neighborhoods but no other meaningful differences.[19] 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 being called by the Vietnam draft lottery influenced the next generation's labor market. As described in Appendix 1, there were meaningful consequences for men called by the draft lottery (including large increases in military service, earnings reductions in the 1970s and 1980s, increases in college attainment due to the GI bill, and increases in disability), which we hypothesize could have led to important differences in their children's outcomes. As such, our estimates should be interpreted as the average net effect of draft eligibility on children's outcomes. Following Angrist (1990), we exploit the arbitrary nature of 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.

Four annual rounds of the draft lottery were held from 1969 to 1972 (the final years of U.S. involvement), which randomly determined the order that men were called to report for induction into the military, according to their birthdates. (A randomized process was implemented in response to criticism of prior draft inequities.) This order was set by drawing balls marked with a specified date from a glass container and assigning each birthday a draft priority number based on the order it was drawn. Then, men were called for induction until a need threshold, which varied by year, was reached. Men that were subsequently drafted were required to serve for approximately 2 years.[20]

Our analysis primarily focuses on the second and third drawings (due to randomization errors in the first lottery and lack of a binding draft for the last lottery), which were held in July 1970 and August 1971, pertained to 19 year old men (i.e., those born in 1951 and 1952), and called numbers up to 125 and 95, respectively. Our main estimates derive from comparing the outcomes of children whose fathers were below the cutoff in the relevant year to those whose

[19] 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).

[20] Per the Universal Military Training and Service Act of 1951, "each person inducted into the Armed Forces... shall serve on active training and service for a period of twenty-four consecutive months."

fathers were above it, and thus represent the effects of having a father who was called by the draft (i.e., intent-to-treat estimates).

One natural issue that arises is how draft avoidance behavior factors into our design. For the cohorts we consider, avoidance through educational deferments was relatively minimal (Card and Lemieux, 2001), and other types of deferment, including for marital and paternity reasons, were no longer available (Bitler and Schmidt, 2012).[21] (Note that avoidance through educational deferments might lead to differential human capital investment by draft eligibility, which, all else equal, would likely lead sons of draft-eligible fathers to have *higher* earnings and thus would work against the direction of our result.) Nonetheless, because the draft lottery was randomized, draft avoidance poses no threat to our primary strategy or to the validity of our main estimates, but rather in the interpretation of our findings as the sole byproduct of a father's service.

That said, to aid in the interpretation of our main estimates, we sometimes offer illustrative Wald estimates of the effect of service, which we derive using a separate Defense Manpower Data Center data set on military service by birth date among men from these same cohorts to estimate a "first-stage."[22] These estimates are merely illustrative for two reasons. First, because the first stage estimates are derived 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., the 60% who filed and claimed a dependent in 1996) than for the overall population. Second, for the Wald estimates to be valid, it must be the case that a son's earnings (or other outcomes) are correlated with his father's Vietnam draft eligibility only through its effect on his father's service in the military, an assumption that would be violated if measures were taken to avoid the draft that affected children's outcomes.[23] Later, we describe a crude examination of avoidance behavior that we can

[21] Specifically, per Executive Order 11527, paternity deferments were no longer granted for children conceived on or after April 23, 1970, and marital deferments ended in the mid-1960s. The first lottery drawing for our analysis sample took place July 1, 1970.

[22] 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.

[23] Studies that found elevated college attendance among draft-eligible men concluded these differences were driven by service rather than avoidance; thus, the scaled estimates are not biased by these education effects.

perform within our setting—exploiting differences in the cutoff in the current and preceding year—and show that such behavior does not appear to be driving our results, consistent with the institutional features of the lotteries we examine as well as conclusions from prior work.

We first briefly describe this first stage 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 2, 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 estimates of draft eligibility on son outcomes, we also scale each of our main 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). 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,[24] (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). We use data from 2013, the most recent year available, to focus on a measure of earnings that is most representative of lifetime earnings. Our specification pools the sons across the two draft cohorts

[24] 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.

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 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 $267.91 less than untreated sons (i.e., sons whose fathers were not draft eligible), lowering their income by 0.72% of the mean (Table 2, left column). This figure reflects two negative labor market outcomes for these men: first, they were 0.14 p.p. less likely to work in 2013, and, second, conditional on working, their log earnings were 0.6% lower than untreated sons. (Given the former, we note the latter is a selected outcome.) The level earnings and log-transformed results are significant at 1% ($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 military participation between 1999 and 2013 or (2) an integer-valued variable, 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% ($p < 0.001$).[25]

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 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.

[25] We also consider variants of this specification in Appendix Table 3, described in more detail in Appendix 3. 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.

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.[26]

D. Heterogeneity

We investigate heterogeneity in effects by parental income (measured in 1996) and sons' earnings (measured in 2013).[27] First, we probe in Appendix Table 4 the extent to which particular parent-income groups drive our estimates. (As parents' income may be affected by father's draft eligibility, the bottom panel of the table displays estimated effects on being in a particular income quartile according to eligibility. The coefficients are all statistically indistinguishable from zero, suggesting comparisons within parent-income groups are valid.) 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.[28] When we flip the question around and probe whether sons are differentially likely to earn above different earnings percentiles (Appendix Table 5), we find that treated sons are worse off no

[26] 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).

[27] A limitation in interpreting these results is that we do not have a correspondence between draft eligibility and Vietnam-era service (or other outcomes) for men from different income groups. Relative to prior U.S. war drafts, the Vietnam War draft in particular drew from the middle of the income distribution, reportedly because high-income draftees were afforded new avenues to avoid service or being sent to war (i.e., conscientious objector status or prior enrollment in college) 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).

[28] 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 that the finding is exclusively driven by much smaller effects on father's service at the top of the income distribution because of the large increase in son's military service that we also detect among this group.

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.[29] 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%.[30]

Turning to service, fathers' draft eligibility, on average, increases daughters' military outcomes—participation by less than 1 p.p. and duration by 0.0044 year. Both coefficients are statistically significant at 1% ($p = 0.003$). While these estimates are substantially smaller than those we recovered among sons, this difference appears to reflect differences in military participation by sex, as 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 samples. Unsurprisingly, the results are extremely significant (with the exception of labor force participation).[31]

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[29] The rationale for our focus on sons thus far 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).

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

[31] 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

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 split our sample and show our results are driven by the sons of current citizens rather than noncitizens (which is expected given that almost all current noncitizens would have immigrated to the U.S. after the draft). In column (3), we add duplicate dependents (i.e., sons also claimed by other fathers), whom we had excluded from our main analysis sample.[32] In column (4), we take our main sample and 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 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

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.

[32] We also confirmed that a duplicate claim is uncorrelated with draft eligibility status.

adulthood and is relatively constant across ages. It is clear from these figures that the results are not specific to any age.[33]

Turning to Appendix Table 10a, 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));[34] 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, with the binned specifications providing some support for the interpretation of our main estimates as the byproducts of service, rather than avoidance (under the assumption that the increased probability of enlistment associated with lower draft numbers does not systematically relate to increased draft avoidance).

To explore avoidance more directly, we exploit two features of the lotteries: 1) the draft eligibility cutoff in each year was lower than the cutoff in the year immediately preceding it and 2) the cutoff was not announced in advance. Thus, those with numbers low enough to have been called in the prior year but not in the current year may have undertaken preemptive avoidance actions even though they were ultimately not at risk of being drafted. To that end, we test whether having a father with a number within this range affects our main outcomes and the estimates are not significant (Appendix Table 10b). While this exercise is not conclusive, the results suggest that avoidance behavior is not driving our results.

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

[33] 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.

[34] 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.

(1953 cohort, though no one was ultimately drafted). 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.

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.[35] 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 not always 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

[35] 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.

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.[36] Across all outcomes, the difference is always statistically and economically insignificant, suggesting nonfilers are not affecting the generalizability of our results.

IV. Other Son Outcomes of Interest

In this section, we attempt to round out our understanding of how sons were affected by their fathers' draft eligibility. We follow the same estimation framework as before and investigate whether we can observe systematic differences in other outcomes, related to human capital development, that we can observe in our data. Any differences we detect may also yield insight into our main findings and point to the particular mechanisms underlying them, which we examine more thoroughly in the next section.

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 (Table 6, panel A). 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) (row 1). These estimates are a bit higher when we consider civilian work experience alone (row 2). 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 next panel 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 year of college, which they

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

attributed to GI Bill benefits. As a result, all else equal, we would expect an increase in education among their children if there is an intergenerational transmission of education and/or if sons themselves are eligible for GI Bill benefits that correspondingly induce them to pursue more education than they otherwise would have. However, the negative earnings effect documented earlier would make a decrease unsurprising. To investigate these outcomes, we first derive three measures to capture sons' attendance patterns: (1) integer-valued years of postsecondary attendance, representing the number of years that at least one Form 1098-T was filed; (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 and second outcomes, so the lower prior work experience among treated sons does not appear to be a byproduct of more time spent in school. We do, however, estimate a marginally statistically significant decrease in graduate training. This finding helps to explain some of the differences in earnings we estimate for 2013, but raises additional questions regarding how the additional time outside the labor market was spent.

In the bottom three rows of Panel B, we probe several additional questions related to postsecondary schooling, and we see that postsecondary attendance patterns are more nuanced than the broader outcomes suggest: treated sons attend lower-quality postsecondary schools (as measured by the log average 2013 earnings of the other attendees of each school, weighted by the time spent at each school), are more likely to receive GI Bill benefits (which we measure using the universe of Department of Education annual grant and lending activity), and, are generally less likely to graduate from their program of study (which we measure using information reported by schools to the Department of Education). The quality and completion results help explain why labor outcomes are lower even though attendance is not much different (and suggests they are less productive while enrolled in school), and the increased receipt of GI Bill benefits could help explain the about-even levels of college we observe, if they compel treated sons (who are more likely to be eligible for such benefits) to pursue more schooling than they otherwise would have.

In panel C, we 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. We note the limitation that these are not the most comprehensive measures of health for individuals in this age range.[37]

Then, in panel D, we examine differences in circumstances that capture time spent outside the labor force in 2013 (which could mechanically lower labor market outcomes). There is no difference in whether treated sons are in school (row 11), but, using additional information on incarceration, treated sons are marginally more likely to have been in prison (row 12).

Finally, in panel E, we use the Kidlink database to explore the fatherhood outcomes of the sons in our sample. While we find that treated sons have statistically the same number of children as untreated sons, 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 lower earnings down the road among this group.

In sum, our findings of reduced labor market experience, worse education (in terms of completion, quality, and graduate school), higher levels of incarceration, and increased teenage fatherhood, while generally modest in size, suggest that, at early stages of young adulthood, differential rates of human capital investment may be contributing to the later-life differences we observe. Further, this set of findings points to the existence of possible cognitive and non-cognitive skill gaps between treated and untreated sons.

V. Discussion of Mechanisms and Interpretation

In Appendix 4, we explore in detail the possible explanations for our results. While the children we study (i.e., those claimed in 1996) were generally too young to have been alive during the draft lottery, or their father's mandatory service if drafted, there are several channels, stemming from how the lottery affected their fathers, that could be operating. We conduct a

[37] 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 also have slightly higher overall rates of disability.

number of additional empirical exercises that leverage the full breadth of our data but are more suggestive in nature to assess the viability of each explanation. This section summarizes that analysis and highlights the most relevant results from the corresponding appendix tables.

We first investigate the 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;[38] (2) their increased enlistment rate lowered their earnings; (3) different household environments as children led them to develop different preferences towards work more generally or skills/ preferences for (non-military) lower-paying sectors; or (4) lower parental inputs, defined broadly, reduced their general human capital.[39] 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 to support a genetic explanation for our findings: we can rule out even small differences in mothers' earnings, labor force participation, and disability rates by draft eligibility (Table 7).[40,41] As a result, we surmise that we are isolating non-genetic contributions to intergenerational mobility and that the earnings effect is driven by malleable aspects of parents that changed as a result of the draft. Thus, we have established that, absent genetic mediation, shocks can propagate across generations.

Second, the increased enlistment rate cannot explain much of the earnings gap. At the extreme, if the entire reduction in earnings was driven by the enlistment increase we detect, then

[38] 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 14). More discussion of this issue can be found in the Appendix.

[39] Work linking parental inputs to children's outcomes extends to other literatures (e.g., Becker and Tomes, 1976 and 1979; Becker, 1981; Cunha, Heckman, and Schennach, 2010). 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.

[40] These finding are also consistent with mothers being no worse on non-genetic dimensions as well. 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).

[41] 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).

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 both reduced labor force participation and earnings conditional on working, which would not be consistent with a simple reservation wage story. In Appendix Table 15, we do not find much evidence that treated sons differentially sort into lower-paying or shorter-workweek industries (or even the same firm as their fathers), nor into lower-paying government or social service jobs (perhaps via a more-general public service bent that was transmitted within their households). Altogether, our data yield little evidence in support of the hypothesis that preferences for or skillsets linked to lower-paying jobs are generating our effects. (In the appendix, we also examine possible differences in other preferences related to work, such as attitudes toward family or risk, and we do not find evidence consistent with such differences.)

Finally, while, by elimination, parental inputs appear to be driving a large portion of our earnings result—which is indeed *prima facie* consistent with many of the reduced human capital and broader measures of disadvantage we observed in Table 6—we are also able to offer evidence consistent with this channel. As we define parental inputs rather broadly, there is considerable scope for the investments parents make to differ by draft eligibility. For ease of exposition, we describe first the evidence on resources and time at home, and then turn to evidence of any indirect effects of parents' circumstances that we can observe (e.g., location).

First, prior literature has documented earnings reductions among draft-eligible men during time periods that may have been formative for the children we study and impaired their human capital development.[42] In addition, later work documented that the lower earnings in the decades after the war faded over time, so that children born earliest within our setting likely grew up with the fewest resources. Consistent with this narrative, we estimate larger decreases among

[42] 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.

sons born earliest (Appendix Table 12).[43] 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 16) and increase in career military service (Appendix Table 17).[44] Last, treated sons resided in lower-mobility counties in 1996, pointing to small indirect neighborhood effects (Appendix Table 1). And while our data do not allow us to examine it directly, we cannot rule out differences in the quality of parenting, for which a broad read of the findings on father disability, health more generally, and incarceration could provide support (Appendix Table 17 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,[45] and (2) 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 documented lower earnings 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)

[43] 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.

[44] 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.

[45] 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.

enlistment increase but an earnings reduction. Second, the effect of a father's draft eligibility on son 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 particularly 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 18). 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 19). 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 20). 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.[46]

VI. Conclusion

The effects of the Vietnam draft indeed persisted into the next generation. In this study, we have uncovered two facts regarding the sons of draft-eligible fathers: (1) they have lower earnings, and (2) they are more likely to serve in the military. These results survive numerous

[46] Focusing on a different type of transmission, Dahl, Kostøl, and Mogstad (2014) supports this conclusion. They find evidence that parental disability receipt increases the likelihood children receive disability and attribute the effect to information about, or attitudes towards, disability insurance.

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 education as well as increased incarceration and teenage fatherhood—and we conclude by investigating the various channels that may give rise to our findings.

The reduction in sons' earnings appears to derive from diminished human capital development, a byproduct of how the draft lottery affected their fathers. (Incidentally, it also suggests that any positive intergenerational effects of education are limited or at least are not large enough to offset more-negative effects of the draft.) This finding underscores the strong role that parents—and, particularly, the circumstances of theirs that can be manipulated by policy—play in determining children's long-term outcomes. More broadly, our findings offer conclusive evidence that policies that only directly alter the circumstances of one generation can have important long-run effects on the next.

The size of our earnings effect is considerable: if we accept the assumptions underlying the scaled estimates, our results point to a 6 percent reduction in earnings due to military service. To put the magnitude of this effect in the context of prior literature that detects earnings effects, it is about equal to an extra 3 to 4 years of a one standard deviation higher-quality teacher (Chetty, Friedman, and Rockoff, 2014) and almost as large as the effect on median earnings of the Perry Preschool targeted intervention (Schweinhart et al., 2005).[47] Further, since the average age of our sample in 2013 is generally past the "overtaking age" of Mincer (1974) and, if anything, the size of our estimate increases with age, these earnings differentials will likely extend through the lifecycle and may very well continue to be visible in future generations.

[47] The large confidence intervals and often insignificant point estimates on mean earnings from the Perry Preschool experiment limits the ability to meaningfully compare those effects, though the mean estimates are about twice as large (Heckman et al., 2010). As another comparison, our estimates are similar to the point estimate (though the estimate is insignificant) of being randomized into a highly educated and smaller household relative to an average household (Sacerdote, 2007).

Our 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 (i.e., randomly assigned conscription for an unpopular war). Our estimates suggest, assuming no spillovers and all else equal, that had all men from these two cohorts been called by the lottery, we would expect over 100,000 future enlistees owing to conscription alone.[48]

More generally, our results speak to the correlation in disadvantage across generations, and, in particular, demonstrate that reduced opportunities in one generation can hinder the next. Our findings show that family plays an important role in both the transmission of occupation and the development of long-term human capital, each of which can help explain why historically determined differences persist over time (Altonji and Blank, 1999). Investigating the key manipulable forces that underlie our results as well as the generalizability of our estimates of occupational transmission to other settings are potential subjects for future work. Further, subsequent work should examine the extent to which other policies, particularly those that *increase* opportunities instead of *reduce* them, can have positive effects on future generations.

[48] These concerns are especially crucial for today's military, which relies on voluntary enlistment and must continually replenish its ranks (Segal and Segal, 2004).

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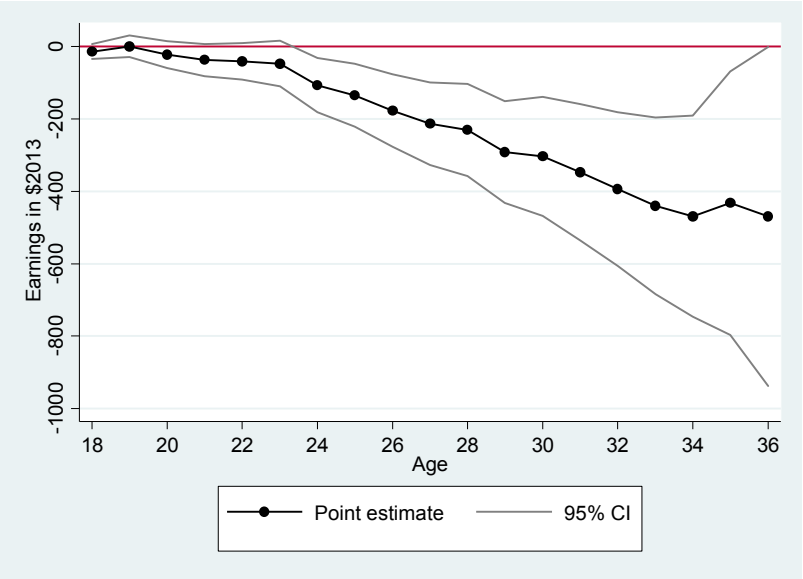
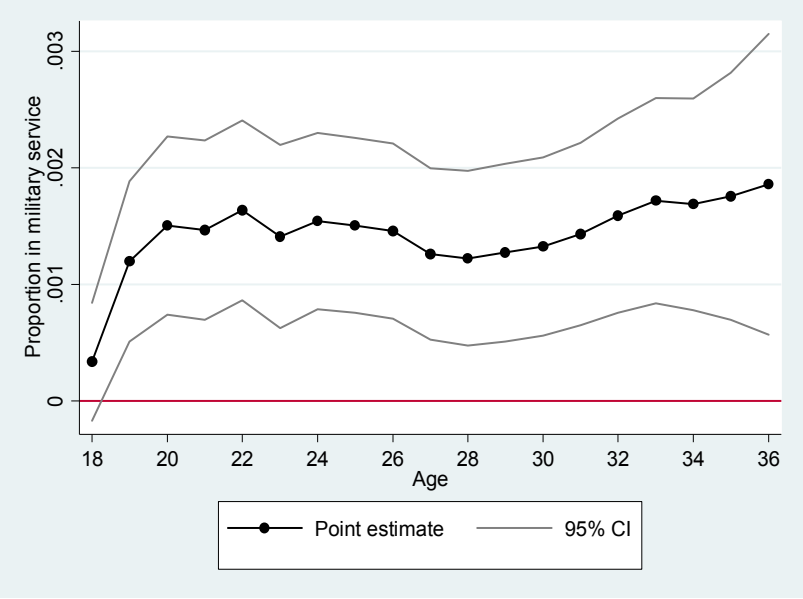
Figure 1a and Figure 1b: Effect of father's draft eligibility on son's earnings (1a) and military service (1b) at various ages (1999–2013)





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. 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)*

	Coefficient (SE) on draft eligibility			
	(1)	(2)	(3)	(4)
Outcome	Active duty	Reserve duty	Not during wartime (1999–2000)	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 other son outcomes*

Outcome		Coefficient (SE) on draft eligibility	Control group mean
Panel A: Prior Labor Market Experience (1999-2012)			
Years worked	(1)	-0.0216	9.7456
		(0.0096)**	
Civilian years worked	(2)	-0.0291	9.4859
		(0.0097)***	
Panel B: Postsecondary (1999-2013)			
Years of schooling	(3)	0.0001	0.001
		(0.0058)	
Any undergraduate?	(4)	0.0002	0.7021
		(0.0009)	
Any graduate?	(5)	-0.0012	0.143
		(0.0006)*	
Log $postsecondary quality (cond. on enrollment)?	(6)	-0.0012	58,701.45
		(0.0006)**	
VA schooling benefits (cond. on enrollment)?	(7)	0.0016	0.0378
		(0.0005)***	
Program completion (cond. on enrollment)?	(8)	-0.0019	0.5913
		(0.0011)*	
Panel C: Health (2013)			
Alive?	(9)	-0.00001	0.9775
		(0.00023)	
Any disability income?	(10)	-0.00002	0.0153
		(0.00019)	
Panel D: Labor Force Alternatives (2013)			
In school?	(11)	0.0004	0.1518
		(0.0006)	
Incarcerated?	(12)	0.00031	0.0129
		(0.00017)*	
Panel E: Fertility (1999-2013)			
Total number of children	(13)	-0.0026	0.6749
		(0.0018)	
Children fathered as teenager	(14)	0.0006	0.0159
		(0.0002)***	

Notes: Please see Appendix 5 for table notes.

Table 7. *Effect of father's 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 (including all subsequent pages)

Appendix 1: Prior Estimates of Effects of Vietnam Draft Lottery on Draft Cohorts

Authors (Year)	Data Source(s)	Main Outcomes	Period	Summary of Findings
Angrist (1990)	SSA records	Earnings	early 1980s	White veterans earned about 15 percent less per year than nonveterans.
Card and Lemieux (2001)	Current Population Survey, Census	Educational enrollment (postsecondary)	1955-1980 (enrollment)	Differences in enrollment were at or near zero for 1951 and 1952 draft lottery cohorts (but higher earlier when educational deferments were more available).
Dobkin and Shabani (2009)	NHIS	Health	1997-2004	Some evidence of early mental health effects but differences are generally statistically insignificant
Angrist, Chen, and Frandsen (2010)	Census	Employment and Disability	1999/2000	Increase in federal transfer income (likely VA disability benefits). Differences in work-related disability status are statistically insignificant but small effect on non-work-related disability for whites
Angrist and Chen (2011)	Census	Education, Labor Market	1999/2000	White veterans attended about 0.33 more years of schooling. Differences in broad labor market are statistically insignificant.
Angrist, Chen, and Song (2011)	SSA records with additional military service info	Earnings	1970s, 1980s, 1990s, 2000s	White veterans earned about 15 percent less per year in the 1970s and about 10 percent less in the 1980s. Differences are statistically insignificant in 1990s and 2000s.
Conley and Heerwig (2012)	National Center for Health Statistics multiple cause of death file	Mortality	1989-2002	No long term effect of draft exposure on overall mortality or specific causes
Lindo and Stoecker (2014)	Surveys of Inmates in State and Federal Correctional Facilities	Incarceration	1979, 1986, 1991	White veterans more likely to be incarcerated for a violent crime (0.34 p.p. in 1979) and less likely to be incarcerated for nonviolent crime (0.30 p.p. in 1979).

Appendix 2: 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).[49] 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 effects of father's draft eligibility on son outcomes 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.[50] 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).

[49]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)).

[50] 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

Appendix 3: 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.[51]

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.

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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.

[51] 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 4: 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 determine causal effects of draft eligibility, it is less suited to conclusively determine the exact 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 develop different preferences toward work generally (e.g., lower reservation wages or riskier earnings choices) or preferences (or skills) for lower-paying sectors; or (4) lower parental inputs reduced their general human capital.[52] 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 14 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 between draft-eligibility and dependents claimed is insignificant across outcomes, which is inconsistent with genetic differences between fathers in our sample.[53]

[52] There may be other explanatory factors that we have not listed that could also explain this result.
[53] 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.

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.[54] 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 son military service alone can explain the earnings losses we recover.[55] 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 ($37,082.86).[56] 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

[54] 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. These findings are also consistent with mothers being no worse on non-genetic dimensions either.

[55] 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).

[56] This is implausibly high in terms of both possible deleterious effects of service on annual earnings and nontaxable benefits given to current service members (Basic Allowance for Housing) and veterans (GI Bill assistance and VA loans).

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 different preferences toward work generally (e.g., lower reservation wages or riskier earnings choices) or 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 consistent with systematic differences in reservation wages, 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 equal, would imply lower reservation wages).[57] Other pieces of evidence focusing on leisure and time spent outside of work more generally are also inconsistent with such differences. First, in Appendix Table 15, we find no evidence of differences in an industry-based measure of hours worked among those that participated in the labor market.[58]

[57] On their own, differences in reservation wages could therefore at best explain only a portion of the result.
[58] We construct this measure by taking the average of hours worked for males in our age range by North American Industry Classification System (NAICS) industry from the 2013 American Community Survey and assigning sons a value based off of their 2013 highest-earning industry (among those that worked). The American Community

Second, draft-eligible sons are no more likely to be self-employed (Appendix Table 15), pointing to relatively evenly-valued flexibility in the workplace. Third, with differences in preferences for family time in particular (whether for leisure or home production), we might expect to see differences in the size of their families, yet there are no differences in the number of children fathered by draft eligibility (Table 6). Relatedly, if draft-eligible sons earn less because they spend more time taking care of their fathers, we would expect them to be more likely to claim their father as an exemption on their 2013 tax returns--eligibility for this deduction is determined on the basis of how much support and care are being provided--yet we can rule out small differences in such claiming (p-value of 0.756).

We also consider whether the risk preferences of sons were altered. Indeed, our findings of higher rates of teenage fatherhood and incarceration point to non-cognitive skill gaps perhaps related to risk (as could our military service result), but our focus here is evaluating evidence for whether there is any differences in the concavity of the utility function (as opposed to other differences in risk-taking behavior). Several pieces of evidence are not consistent with this hypothesis. First, the variance of earnings for the sons of draft-eligible fathers is not larger, even when excluding those that do not work. Second, as discussed before, we found no differences in rates of self-employment (which is relatively risky) or working at a government job (which is relatively not risky). Third, we find no evidence that sons are any more likely to have gambling winnings or losses (p-value=0.785).[59]

To explore whether the earnings result is driven by treated sons having a proclivity towards lower-paying jobs, we carry out several empirical exercises. In Panel A of Appendix Table 15, we examine whether draft-eligible sons are more likely to work in (often lower-paying) government or social service jobs, perhaps via a more-general public service bent that was transmitted within their households. Yet, we can rule out even small differences in the

Survey measure is primarily based on three digit NAICS codes; there are a total of 95 industry categories with average hours worked ranging from 27 hours to 56 hours. While the confidence interval may be mechanically narrowed by taking industry averages, the standard deviation of the averages is still non-trivial (4.12 hours); indeed, when we standardize this measure, we can still rule out a precise difference in hours worked of less than -0.006 and greater than 0.001 of a standard deviation.

[59] We similarly can rule out even small difference in gambling winnings or losses between draft-eligible and non draft-eligible fathers. This is seemingly not consistent with the draft lottery influencing how "lucky" father felt in a way that influenced their decisions.

likelihood of working in the (non-military) government or not-for-profit sector. To examine sorting more generally, we estimate the intensive 2013 earnings effect of draft eligibility with and without six-digit North American Industry Classification System (NAICS) industry controls for their highest-paying job (Panel B of Appendix Table 15).[60] We find that the earnings effect is insignificantly lowered by less than 15%. To account for any bias introduced by treated sons sorting into particular industries but who may earn less due to other mechanisms, we separately calculate industry-simulated earnings measured by mean earnings among control group 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 lower-paying industries for the exclusive reason that they have lower earnings potential; and (2) the estimate mechanically includes fixed differences in earnings in the military sector as well. We also do not find any evidence that draft-eligible sons are less likely to work at the same firm as their father (Panel C of Appendix Table 15), suggesting that our earnings result is not mediated by draft-eligible fathers being less likely to help their son get a job (Kramarz and Skans, 2014).

To sum up, the data we have yield little evidence in support of preference-based mechanisms. While we cannot conclusively rule them out (e.g., there could be sorting across occupation that cannot be measured with tax data and will not perfectly overlap with detailed industry), the presence of such mechanisms would ultimately still indicate that environmental factors brought on by parents' circumstances are driving our results.

We could, at this point, by elimination 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. Moreover, this channel would in fact be consistent with the broad differences in treated sons' human capital and other measures of disadvantage we presented in Table 6. However, we can also present evidence pointing to a reduction in the quantity and quality of parental resources, time, and/or decision that either directly or indirectly influences children's human capital development (separate from the effects of parents on preferences or job-specific skills that underlie mechanisms (2)-(3)).

[60] We note the limitation that the sample is selected due to an extensive margin work response.

We begin with an examination of household resources. This setback in earnings could not have been the result of the draft directly (including father absenteeism due to Vietnam service), as dependents claimed in 1996 would not have been born yet. Prior literature has evinced several channels that may have contributed to our result (see Appendix 1).[61] The two-stage least squares earnings losses of 15% in the 1970s and 10% in the 1980s from early work are consistent with a resource channel.[62] 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).[63]

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 16), 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.[64] 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 17), 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,

[61] The higher levels of education among draft-eligible men (Angrist and Chen, 2011) are likely working against our finding.

[62] 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.

[63] Of course, other setbacks for draft-eligible men may have exhibited a similar pattern. 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.

[64] 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).

children's academic achievement has been shown to suffer when fathers are deployed ((Engel, Gallagher, Lyle, 2010; Lyle, 2006)).[65]

While our data do not allow us to examine it directly nor are we aware of any studies with such a finding in this context, we cannot rule out differences in the quality of parenting. Lindo and Stoecker (2014) found some evidence that draft-eligible men were slightly more likely to be incarcerated for violent crime (while were slightly less likely to be incarcerated for nonviolent crime, with no overall effect on the likelihood of incarceration), suggestive of such differences. Draft-eligible men have poor physical health and higher disability rates relative to their peers (Angrist, Chen, and Frandsen, 2011, Appendix Table 17 looking at disability insurance), which might point to differences in the quality of care parents are able to provide.[66] The evidence on mental health is more mixed. Dobkin and Shabani (2009) is consistent with perhaps some mental health differences early on. In addition, early work exploiting the draft lottery (Hearst, Newman, and Hulley, 1986) found initially elevated rates of death from suicides and accidents among draft-eligible men, which could suggest such men were less stable more generally.[67] However, Angrist, Chen, and Frandsen (2011) find no evidence of later-life mental health differences, and Conley and Heerwig (2012) find no long-run differences in mortality, including mental-health-related causes of mortality. Further, in our data, we do not detect any differences in family structure (Appendix Table 1).

[65] 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.

[66] 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; Johnston, Shields, and Siminski, 2015) has reached similar conclusions.

[67] 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.

Finally, the results from Appendix Table 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.[68]

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

First, 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

[68] 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 2016) 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 (endogenous) 1996 county or zip code fixed effects in our main specification insignificantly lowers the earnings estimate by about 10 or 20 percent, respectively, though given geographic sorting of families by income level and other characteristics, this estimate likely overstates how much can be explained by neighborhoods. The results also do not appear to stem from draft-eligible sons living in lower cost living areas (as measured by the Missouri Economic Research and Information Center cost of living index); a regression by draft eligibility is insignificant with a p-value of 0.302.

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).[69] 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 18). To begin, we attempt to directly estimate the relationship between opportunities and enlistment, where, for the purposes of this exercise, we use family income as a proxy for opportunities. Among non-draft-eligible sons, we regress military enlistment on log 1996 family income and recover an estimate of -0.006.[70] 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.[71]

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.[72]

[69] 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).

[70] 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.

[71] 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).

[72] 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).

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),[73] 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 (although it is very possible the opposite is the case). Still, while we cannot determine the size or direction of the omitted variable bias, there would likely have to be quite substantial downward bias in these elasticities for the first channel to fully explain our military result.

Third, if sons of draft-eligible fathers indeed have a stronger preference to enlist in the military, we might expect that during times of economic distress—when economic opportunities are more likely to drive enlistment decisions—the gap in participation between the two groups shrinks. We test for this expectation by investigating whether the effect of draft eligibility on service is weaker during the Great Recession. As we show in Appendix Table 19, we find evidence consistent with this prediction. Because a father's draft eligibility is less important in determining enlistment outcomes when overall job opportunities are fewer, treated sons' increased military attachment does not appear to be the result of lower economic opportunity alone.[74]

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

[74] However, while our current body of evidence does not support this, 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.

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 (Shulimson et al. 1997).) Therefore, we examine whether there is a difference in a son's proclivity to enlist in the Marines according to whether his father was at risk of being drafted into the Marines. In other words, we compare the 1949 and 1950 cohorts—for which the draft did lead to larger enlistment behavior of the fathers as well as their children, as documented in the appendix—with the 1951 and 1952 cohorts. As shown in Appendix Table 20, we find that the sons of older cohorts are slightly (0.0005 p.p.) more likely to enlist in the Marines, which is consistent with a transmission of occupation.[75]

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

[75] 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.

may not be important in the relationship. First, given the overall lower earnings found among sons of draft-eligible fathers, all else equal, mental aptitude is unlikely to be higher among sons of draft-eligible fathers. Second, conditional on military service, military earnings are no different between the two groups (though a higher fraction of the treatment group does serve, introducing a potential selection bias).

Second, we consider the extent to which the U.S. military might actively recruit the children of veterans (COVs) to a greater extent than other children. While the incentives of recruiters are limited to filling their enlistment quotas and there is no evidence that the military formally targets COVs in their recruitment, it may indirectly expend additional resources to target COVs for reasons of self-selection. For example, those in high school Reserve Officers' Training Corps (i.e., "ROTC") programs are more heavily recruited (and COVs are likely overrepresented in these programs), and there may be a heightened military recruitment presence in areas with more military families.[76] 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.

[76] Relatedly, the military service result does not appear to be explained much by neighborhood, since adding 1996 county or zip code controls does not significantly change the enlistment estimate.

Appendix 5: Notes to Table 6

Table 6 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 between 1999 and 2012 the son received either a Form W-2 or 1099-MISC (for nonemployee compensation) from any employer, or for civilian experience, non-military employers. College attendance is calculated as the number of years the son received a Form 1098-T for years of college, or whether one was received for undergraduate or graduate school, for any undergraduate or graduate school attendance, respectively. Postsecondary quality is measured as the weighted average of the quality of institutions each son attended (whereby quality is measured as the average 2013 earnings of male dependents aged 30-35 claimed in 1996 by men outside of the 1951-52 cohorts who attended each institution), and excludes those who attended no college. Veterans schooling benefits and program completion are derived from the NSLDS database and information reported by schools to the Department of Education, and excludes non-matches to the database, which is similarly balanced to our measure of any college attendance (p-value of 0.982). 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. Incarceration is measured from data IRS receives on the incarcerated population. Fertility behavior is derived from Kidlink (through 2013). Control group means are derived from sons of non-draft-eligible fathers. The sample size is 2,153,234, except for postsecondary quality (1,547,905) and VA benefits for education and program completion (964,543). Standard errors are clustered at the father date of birth level.

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 (2016), 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 2 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's draft eligibility on other son military outcomes (1999–2013)*

	Coefficient (SE) on draft eligibility			
Panel A: Branch	(1) Air Force	(2) Army	(3) Navy	(4) 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) Years of military service intensive margin	(6) Any son in military	(7) In military (one son in household)	(8) 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 father's 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 father'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*

Outcome	(1) Earnings	(2) Any work	(3) Ln(earnings)	(4) Military service	(5) 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 father's draft eligibility by gender*

Outcome	(1) Earnings	(2) Any work	(3) Ln(earnings)	(4) Military service	(5) 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 father's 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. See earlier in the paragraph for a description of how and why the sample sizes sometimes differ from our main sample. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 9. *Effect of father's 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. See earlier in the paragraph for a description of how and why the sample sizes sometimes differ from our main sample. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 10a. *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 10b. *Effect of father's potential-but-unrealized draft eligibility on son's outcomes, conditional on actual draft eligibility (avoidance test)*

	Coefficient (SE) on draft eligibility				
Outcome	(1) Earnings	(2) Any job	(3) Ln(earnings)	(4) Any military service	(5) Years of military service
Draft eligible last year+not this year	164.42	0.0013	0.0017	-0.0003	0.0009
	(118.14)	(0.0010)	(0.0034)	(0.0006)	(0.0045)
Control group mean	37,082.86	0.8150	45,501.22	0.0765	0.4878

Notes: The table presents estimates from regressions of son outcomes on a dummy variable indicating whether the father would have been Vietnam draft eligible in the prior year's lottery but not actually draft eligible in the lottery to which he was subject (based on his date of birth), a dummy variable for whether the father was draft eligible in the current year, 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. Earnings are in 2013 dollars. Standard errors are clustered at the father date of birth level. 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 father's 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 father's 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 father's 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 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	0.0333

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 15. *Effect of father's draft eligibility on son industry and related choices in 2013*

	Coefficient (SE) on draft eligibility		
Panel A: Sorting in public or social service sectors			
	(1)	(2)	(3)
Outcome	(Non-military) Government	Not-for-profit	(Non-military) Government or Not-for-profit
Draft eligibility	-0.0001	-0.0003	-0.0003
	(0.0006)	(0.0004)	(0.0007)
Control group mean	0.1187	0.0668	0.1880
Panel B: Sorting into lower paying industries			
	(4)	(5)	(6)
Outcome	Nonzero earnings	Nonzero earnings (w/ 6-digit industry control)	Simulated nonzero earnings based on industry
Draft eligibility	-247.23	-213.29	-44.33
	(84.26)***	(77.54)***	(29.91)
Control group mean	45,496.74	45,496.74	45,496.74
Panel C: Related outcomes			
	(7)	(8)	(9)
Outcome	Hours worked (industry imputation)	Any self-employment Income	Work at same firm as father?
Draft eligibility	-0.0093	-0.0007	0.0004
	(0.0072)	(0.0005)	(0.0005)
Control group mean	39.3971	0.1080	0.0724

Notes: The table presents estimates from regressions of industry related 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, and, in column (5), fixed effects for 6-digit NAICS industry. The sample comprises male dependents with nonzero earnings claimed on 1996 tax returns by men born between 1951 and 1952. Columns (1)-(3) outcomes are derived from the Business Operating Division (BOD) Code for each employer while columns (4)-(5) use the North American Industry Classification System (NAICS) industry for employer. Simulated earnings are based on the earnings of sons of non-draft-eligible fathers in each son's 6-digit, and individuals are assigned their industry based on the industry in which they had the highest earnings in 2013. Column (7) imputes typical hours worked from the 2013 American Community Survey using a NAICS industry crosswalk which mostly rely on 3 digit industry codes; there are 95 industry categories ranging from 27 hours to 56 hours. Any self-employment income is defined as receiving a 1099-MISC. Worked at the same firm as the son's father is calculated as whether the son in 2013 works at any firm his father has ever worked (beginning in 1999). 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. $N = 1{,}743{,}056$. *** denotes $p < 0.01$; ** denotes $p < 0.05$; * denotes $p < 0.10$.

Appendix Table 16. *Effect of father's 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 17. *Effect of father's 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 18. *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	-0.006	-0.0120	-0.00002
(SE)	(0.001)***	(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 19. *Effect of father's draft eligibility on son military service in Great Recession versus other years*

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

	Military service
Coefficient (SE) on draft	0.0005
eligibility X 1969 draft lottery	(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 $p < 0.10$.