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

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Online Appendix

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).[1] 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:

$$\mathbf{service}_c = \beta_0 + \beta_1 \times \mathbf{eligible}_c + \boldsymbol{\varepsilon}_s,$$

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 (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

[1] 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).

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.[2] 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).

[2] To suggestively examine this issue, we exploit natural variation across dates of birth in the propensity of appearing in our sample. We ask to what extent differences in the propensity to show up in the sample relate to differences in the first stage. To do so, for each year, we regress the number of times the father appears in the sample on a linear control for exact age (since exact age will mechanically predict whether you claimed a child in 1996), take the residuals, and take the average of the residuals on the binned date-of-birth level to which the first-stage data are aggregated (one bin for every five draft lottery numbers). We then regress the proportion that served in the military on draft eligibility, the averaged residuals, and their interaction. We find a negative but insignificant interaction. This result suggests that the first stage we use may, if anything, be underscaling our Wald estimates.

Appendix 3: Additional Results on the Nature of Sons' Service Outcomes

In Appendix Table 1, 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 0.15 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 3.5%. In other words, the estimated increase on Air Force enlistment is 8.39% of baseline, compared with around 4.32% in the Army and 3.25% 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.[3]

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%) to hold a civilian military position in the VA or the Department of Defense than

[3] 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).

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.

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: Extended Discussion of Mechanisms

This appendix explores 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.[4] 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

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

investigate this further, we examine in Appendix Table 13 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 innate differences between fathers in our sample.[5]

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 mothers' earnings and other labor market measures (Table 7). First, we show 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. The next column verifies that statistically the two groups of mothers are about the same age. The final four columns 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.[6] Since from these findings, innate ability 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.[7] For instance, military enlistment may have

[5] 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.

[6] 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.

[7] 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).

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 $266 reduction in earnings per son is implausibly high: each son induced to enlist must have suffered a $102,307.69 setback in his 2013 earnings, a value several times the mean earnings of sons of non-draft-eligible fathers ($37,705.80).[8] Still, we probe this finding a bit further by comparing earnings among twins, when one twin enlists and the other does not. Assuming that we can hold genes and environment constant, the difference in earnings should yield the expected earnings effects from military service alone. While these assumptions will not fully hold and treatment heterogeneity may limit the comparability of different estimates, this exercise suggests we can rule out a decrease of earnings greater than $5,000 (i.e., the bottom of the 95% confidence interval), which is well below the nearly $100,000 that our estimates imply. Altogether, it seems extremely unlikely the military service effect could explain more than a fraction of the earnings response. Finally, while it remains possible that our earnings losses could result from treated sons being less likely, on the whole, to invest in their general human capital because of an intention to later join the military (regardless of whether they ultimately enlist), which in turn lowers earnings, this explanation would still imply quite a large effect of this decision and/or a large effect on the intention to join the military.

Third, treated sons may have 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

[8] 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).

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).[9] 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 14, we find no evidence of differences in an industry-based measure of hours worked among those that participated in the labor market.[10] Second, draft-eligible sons are no more likely to be self-employed (Appendix Table 14), 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--

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

[10] 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 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 (4.13 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.

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>0.5).

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>0.5).[11]

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 14, 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 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

[11] 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.

Appendix Table 14).[12] We find that the earnings effect is insignificantly lowered by 25%. 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 though the estimate is just out of range of being marginally statistically significant. Nonetheless, 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 14), 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

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

quality of parental resources, time, and/or decisions 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)).

We begin with an examination of household financial resources. The earnings effect 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. However, prior literature has evinced several channels that may have contributed to our result (see Appendix 1).[13] The two-stage least squares earnings losses of 15% in the 1970s and 10% in the 1980s among draft-eligible men from early work are consistent with a resource channel, especially as family size is not statistically different in our setting.[14] 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 11).[15] That said, a back of the envelope exercise that weds the prior estimates with ours suggests that our effect is likely too large to be fully explained by resources. Specifically, the (likely upwardly biased) OLS-measured intergenerational elasticity of income that can be recovered within our data is lower than the estimate that results from scaling the sons' earnings effect by the "fathers'," suggesting considerable scope for other mechanisms. Further, prior work suggests more limited scope for a financial resource channel (e.g., Lefgren, Sims, and Lindquist, 2012; Bleakley and Ferrie, 2016; Cesarini et al., 2016).

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

[14] 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.

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

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 (Table 7), 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.[16] 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 Table 7), which requires at least 20 years of military service. A military career may be indicative of less time spent with children if fathers had to relocate frequently and/or were stationed overseas and did not bring their families with them. (In related work, children's academic achievement has been shown to suffer when fathers are deployed (Lyle, 2006; Engel, Gallagher, and Lyle, 2010).[17])

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; Table 7 looking at disability insurance), which might point to differences in the quality of care parents are able

[16] 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).
[17] 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.

to provide.[18] 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.[19] 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 (Table 1; Table 7).

Finally, the results from Table 7 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.[20]

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

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[18] 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.

[19] 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.

[20] 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 25 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.

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 3.3%. If the story were fully one of fewer labor market opportunities, these findings would suggest that the elasticity between military service and earnings opportunities must be quite large (presumably still brought about by the father's risk of conscription, albeit indirectly).[21] 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 15). 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

[21] 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).

recover an estimate of -0.007.[22] 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 40% decrease in 1996 family income, which seems implausibly high.[23]

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.[24]

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 $266 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

[22] 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.
[23] 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).
[24] 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).

"latent" earnings must be closer to $35,000 to explain the effect (or that the influence of military service among the compliers must be an implausibly large *positive* number),[25] 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 16, 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.[26]

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

[25] This final elasticity exercise uses level earnings instead of log earnings so that the magnitude can be compared with our main effect on earnings.
[26] 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.

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 17, we find that the sons of older cohorts are slightly more likely to enlist in the Marines (0.0005 p.p.), which is consistent with a transmission of occupation.[27]

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

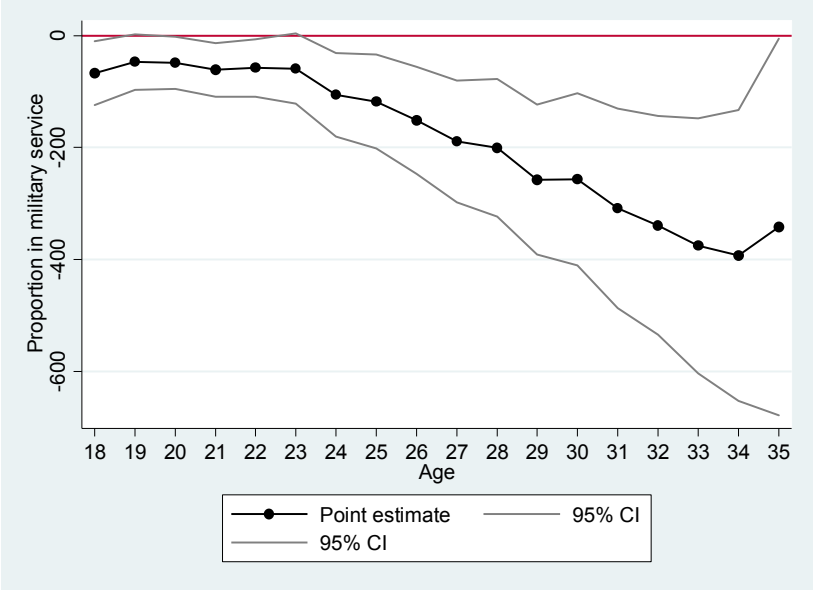
[27] Because of disclosure issues, we have to group the Marines with the Coast Guard, although enlistment behavior in the much larger Marines is driving the results.

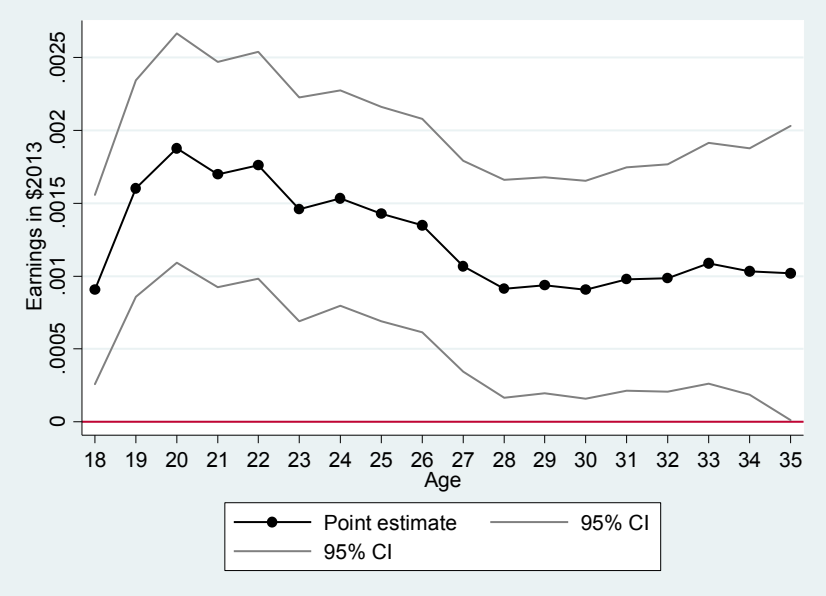
of military service. While we cannot quantify the extent to which this factor drives the transmission in service we observe relative to preferences, a few pieces of evidence suggest it may not be important in the relationship. First, given the overall lower earnings found among sons of draft-eligible fathers, all else equal, mental aptitude is unlikely to be higher among sons of draft-eligible fathers. Second, conditional on military service, military earnings are no different between the two groups (though a higher fraction of the treatment group does serve, introducing a potential selection bias).

Second, we consider the extent to which the U.S. military might actively recruit the children of veterans (COVs) to a greater extent than other children. While the incentives of recruiters are limited to filling their enlistment quotas and there is no evidence that the military formally targets COVs in their recruitment, it may indirectly expend additional resources to target COVs for reasons of self-selection. For example, those in high school Reserve Officers' Training Corps (i.e., "ROTC") programs are more heavily recruited (and COVs are likely overrepresented in these programs), and there may be a heightened military recruitment presence in areas with more military families.[28] 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.

[28] 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 Figure 1—Effect of Draft Eligibility on Son's (a) Earnings and (b) Military Service at Various Ages, Holding Constant Treatment Heterogeneity by Son's Year of Birth





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 cohort-by-month-of-birth fixed effects. We stack all observations between 1999 and 2013 and separately estimate the effect at each individual age while also controlling for a son's year of birth, an interaction between draft eligibility and a son's year of birth, and calendar year fixed effects. Standard errors are clustered at the father date of birth level.

Appendix Table 1—Effect of Father's Draft Eligibility on Other Son Military Outcomes (1999–2013)

	Coefficient (SE) on draft eligibility			
Panel A: Branch	(1)	(2)	(3)	(4)
	Air Force	Army	Navy	Civilian defense (DOD/VA)
Draft eligibility	0.0012	0.0015	0.0005	0.00033
	(0.0002)***	(0.0003)***	(0.0002)***	(0.00017)**
Control group mean	0.0143	0.0347	0.0154	0.0110
Panel B: Other outcomes	(5)	(6)	(7)	(8)
	Years of military service intensive margin	Any son in military	In military (one son in household)	In military (> one son household)
Draft eligibility	-0.0094	0.0031	0.0031	0.0021
	(0.0219)	(0.0005)***	(0.0006)***	(0.0006)***
Control group mean	6.37	0.1010	0.0783	0.0784

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 father's cohort-by-month-of-birth fixed effects. 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 = 2,088,240 in panel A, and N = 165,500, 1,504,248, 1,018,174, and 486,074 from left to right in panel B.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 2—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	-372.91	-50.83	-345.03	-422.63	-377.70	487.13
	(163.29)**	(132.72)	(111.91)***	(127.75)***	(195.53)*	(290.27)*
Control group mean	22,970.29	27,796.37	34,182.98	41,366.26	47,662.92	54,153.37
			Panel B: Military service (1999-2013)			
Draft eligibility	-0.0015	0.0024	0.0036	0.0038	0.0017	0.0031
	(0.0012)	(0.0011)**	(0.0009)***	(0.0009)***	(0.0011)	(0.0010)***
Control group mean	0.0644	0.0839	0.0929	0.0859	0.0701	0.0426
			Panel C: Pr(quantile)			
Draft eligibility	0.0007	-0.00003	0.0004	0.0005	-0.0006	-0.0007
	(0.0007)	(0.00070)	(0.0009)	(0.0008)	(0.0007)	(0.0006)

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 father's cohort-by-month-of-birth fixed effects. 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 = 207,738, 308,232, 521,038, 524,767, 315,249, and 211,068, while N = 2,088,240 in panel C.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 3—Effect of Father's Draft Eligibility on the Distribution of Son Earnings in 2013

	Coefficient (SE) on draft eligibility				
Earnings percentile	(1)	(2)	(3)	(4)	(5)
	Earn > *p*(10)	Earn > *p*(25)	Earn > *p*(50)	Earn > *p*(75)	Earn > *p*(90)
Draft eligibility	-0.0018	-0.0017	-0.0040	-0.0027	-0.0018
	(0.0007)***	(0.0007)**	(0.0008)***	(0.0007)***	(0.0005)***
Earnings cutoff	0	4,803	28,405	54,638	87,120

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 father's cohort-by-month-of-birth fixed effects. 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 = 2,088,240in panel C.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 4—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	-198.71	-0.0011	-0.0049	0.0016	0.0104
	(47.46)***	(0.0005)**	(0.0016)***	(0.0024)***	(0.0018)***
Control group mean	32,876.16	0.8150	40,336.54	0.04829	0.30288

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 father's cohort-by-month-of-birth fixed effects. 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 = 4,054,896 except in column (3), where N = 3,303,865.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 5—Effect of Father's Draft Eligibility by Gender

	(1)	(2)	(3)	(4)	(5)
Outcome	Earnings	Any work	Ln(earnings)	Military service	Years of service
Coefficient (SE) on draft	-138.06	-0.0014	-0.0011	0.0021	0.0116
eligibilityXson	(80.71)**	(0.0008)*	(0.0031)	(0.0005)***	(0.0036)***

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), father's cohort-by-month-of-birth fixed effects, 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 = 4,054,896 except in column (3), where N = 3,303,865.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 6—Effect of Fther'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) Son age effects	(5) Additional father+son controls	(6) Un-winsorized (A and C) or probit (B)
			Panel A: Earnings			
Draft eligibility	-274.21	-57.30	-250.10	-243.41	-235.62	-271.25
	(72.39)***	(261.53)	(70.48)***	(65.27)***	(64.40)***	(123.70)**
Control group mean	38,279.22	27,363.61	37,401.08	37,705.80	37,705.80	38,976.01
			Panel B: Any job			
Draft eligibility	-0.0017	-0.0024	-0.0017	-0.0019	-0.0019	-0.018
	(0.0006)***	(0.0034)	(0.0007)***	(0.0006)***	(0.0006)***	(0.0006)**
Control group mean	0.8380	0.6913	0.8291	0.8303	0.8303	0.8303
			Panel C: Ln(earnings)			
Draft eligibility	-0.0057	0.0013	-0.0049	-0.0038	-0.0036	-0.0053
	(0.0023)**	(0.0110)	(0.0024)**	(0.0019)**	(0.0017)**	(0.0023)**
Control group mean	45,662.89	39,649.09	45,110.55	45,412.87	45,412.87	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 father's cohort-by-month-of-birth fixed effects 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 fixed effects for the son's exact age. Column (5) adds father's state of birth effects. 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 = 1,978,613, 109,627, 2,201,953, 2,088,240, 2,088,240, and 2,088,240. In panel C, N = 1,657,230, 75,715, 1,824,716, 1,732,945, 1,732,945, and 1,732,945.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 7—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) Son age effects	(5) Additional father+son controls	(6) Probit (A) or Poisson (B)
	Panel A: Served in military					
Draft eligibility	0.0029	-0.0018	0.0026	0.0027	0.0026	0.0026
	(0.0004)***	(0.0014)	(0.0004)***	(0.0004)***	(0.0004)***	(0.0004)***
Control group mean	0.0805	0.0428	0.0792	0.0785	0.0785	0.0785
	Panel B: Years of service					
Draft eligibility	0.0178	-0.0143	0.0154	0.0165	0.0164	0.0161
	(0.0034)***	(0.0096)	(0.0032)***	(0.0033)***	(0.0033)***	(0.0033)***
Control group mean	0.5133	0.2639	0.5030	0.5002	0.5002	0.5002

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 father's cohort-by-month-of-birth fixed effects 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 fixed effects for the son's exact age. Column (5) adds father's state of birth effects. 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 = 1,978,613, 109,627, 2,201,953, 2,088,240, 2,088,240, and 2,088,240. See earlier in the paragraph for a description of how and why the sample sizes sometimes differ from our main sample.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 8—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	-2,099.67	-0.0140	-0.0428	0.0209	0.1271
	(574.44)***	(0.0051)***	(0.0178)**	(0.0034)***	(0.0260)***
EligibilityXyear	-2,108.09	-0.0142	-0.0431	0.0209	0.1266
	(570.06)***	(0.0050)***	(0.0177)**	(0.0034)***	(0.0256)***
Intervals	-2,118.76	-0.0125	-0.0454	0.020	0.1212
	(564.25)***	(0.0050)**	(0.0179)**	(0.0033)***	(0.0264)***
IntervalsXyear	-1,992.08	-0.0131	-0.0411	0.0205	0.1258
	(557.16)***	(0.0049)***	(0.0175)**	(0.0033)***	(0.0253)***
Control group mean	37,705.80	0.8303	45,412.87	0.0785	0.5002

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. 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 = 2,088,240 except in column (3), where N =1,732,945.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 9—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				
	(1)	(2)	(3)	(4)	(5)
Outcome	Earnings	Any job	Ln(earnings)	Any military service	Years of military service
Draft eligible last year+not this year	149.35	0.0016	0.0008	-0.0001	0.0024
	(102.84)	(0.0010)	(0.0028)	(0.0006)	(0.0048)
Control group mean	37,705.80	0.8303	45,412.87	0.0785	0.5002

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 father's cohort-by-month-of-birth fixed effects. The sample comprises male dependents claimed on 1996 tax returns by men born between 1951 and 1952. Control group means are derived from son 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 = 2,088,240 except in column (3), where N = 1,732,945.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 10—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	-163.93	11.22	-279.13	-235.93	-299.85	-107.47
	(119.39)	(118.23)	(103.77)**	(100.66)**	(101.73)***	(89.85)
Control group mean	42,896.87	41,070.42	39,319.84	38,898.45	36,778.81	32,999.90
	Panel B: Any military service (1999–2013)					
Draft eligibility	0.0005	0.0022	0.0018	0.0023	0.0029	0.0015
	(0.0006)	(0.0006)***	(0.0006)***	(0.0006)***	(0.0006)***	(0.0007)**
Control group mean	0.0738	0.0740	0.0743	0.0774	0.0794	0.0812
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 = 775,867, 848,171, 910,557, 1,003,793, 1,084,395, and 1,132,349 from left to right.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 11—Effect of Father's Draft Eligibility on and by Son's Birth Year

Outcome	(0) Draft eligibility on birth year	(1) Earnings	(2) Any work	(3) Ln(earnings)	(4) Military service	(5) Years of service
Coefficient (SE) on draft eligibility X son birth year	0.014 (0.009)	27.62 (10.25)***	0.00020 (0.00013)	0.0003 (0.0004)	-0.00014 (0.00007)**	-0.0018 (0.0005)***
Control group mean	1983	37,705.80	0.8303	45,412.87	0.0785	0.5002

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 father's cohort-by-month-of-birth fixed effects, 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, except in Column (0), which shows the standard regression of draft eligibility on son's year of birth. When we isolate this effect from treatment heterogeneity by the age at which we measure the outcomes (as in the exercise underlying Appendix Figure 1 where we exploit all of the data at our disposal), we continue to obtain significant effects for earnings and military service, although the effects are approximately half as large. 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. Standard errors are clustered at the father date of birth level. Earnings are in 2013 dollars. N = 2,088,240 except in column (3), where N = 1,732,945.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 12—Effect of Father's Draft Eligibility by Father-Son Linkage Type

	(1)	(2)	(3)	(4)	(5)
Outcome	Earnings	Any work	Ln(earnings)	Military service	Years of service
Coefficient (SE) on draft	2.15	-0.0002	0.0005	-0.0003	-0.0001
eligibility X SSA link	(33.75)	(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), father's cohort-by-month-of-birth fixed effects, a dummy variable for whether the linkage is through Kidlink, and the interaction of Kidlink with the prior variables. The interaction between Kidlink and draft eligibility is reported. The sample comprises males born beginning in 1983 to fathers born between 1951 and 1952 according to each data set; most sons appear in both data sets. Control group means are derived from sons of non-draft-eligible fathers. Earnings are in 2013 dollars. Standard errors are clustered at the father date of birth level. N =2,153,342 except in column (3), where N = 1,750,584.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 13—Effect of Draft Eligibility by 1996 Dependent Claiming on Own Labor Force Measures for All Draft Cohort Men (1999)

	Coefficient (SE) on draft eligibility			
	(1)	(2)	(3)	(4)
Outcome	Earnings	Any work	Ln(earnings)	Any disability insurance
Draft eligibility X	-69.29	0.0002	-0.0018	-0.0002
dependents claimed	(60.00)	(0.0005)	(0.0012)	(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), father's cohort-by-month-of-birth fixed effects, 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).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 14—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	-0.0004	-0.0002
	(0.0006)	(0.0005)	(0.0007)
Control group mean	0.1184	0.0668	0.1878
	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	-236.68	-176.33	-45.41
	(78.51)***	(69.30)***	(29.27)
Control group mean	45,405.82	45,405.82	45,405.82
	Panel C: Related outcomes		
	(7)	(8)	(9)
Outcome	Hours worked (industry imputation)	Any self-employment Income	Work at same firm as father?
Draft eligibility	-0.0098	-0.0006	0.0004
	(0.0071)	(0.0005)	(0.0005)
Control group mean	39.3928	0.1076	0.0730

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), father's cohort-by-month-of-birth fixed effects, 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 column (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. $N = 1{,}675{,}433$.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 15—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/266
Coefficient	-0.007	-0.0120	-0.00002
(SE)	(0.0002)***	(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 266 because it is our draft eligibility earnings estimate. Earnings are in 2013 dollars. Standard errors are clustered at the family level. N = 1,464,194 in columns (1) and (3), and N = 5,782 in column (2).

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 16—Effect of Father's Draft Eligibility on Son Military Service in Great Recession versus Other Years

	Military service
Coefficient (SE) on draft eligibility X Great	-0.0010
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), father's cohort-by-month-of-birth fixed effects, 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 the mean for 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 = 31,323,600.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

Appendix Table 17—Effect of Father's Draft Eligibility on Son Marines or Coast Guard Service by Father Draft

	Military service
Coefficient (SE) on draft eligibility X 1969	0.0005
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), father's cohort-by-month-of-birth fixed effects, 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 0.0135. 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 = 3,884,340.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.