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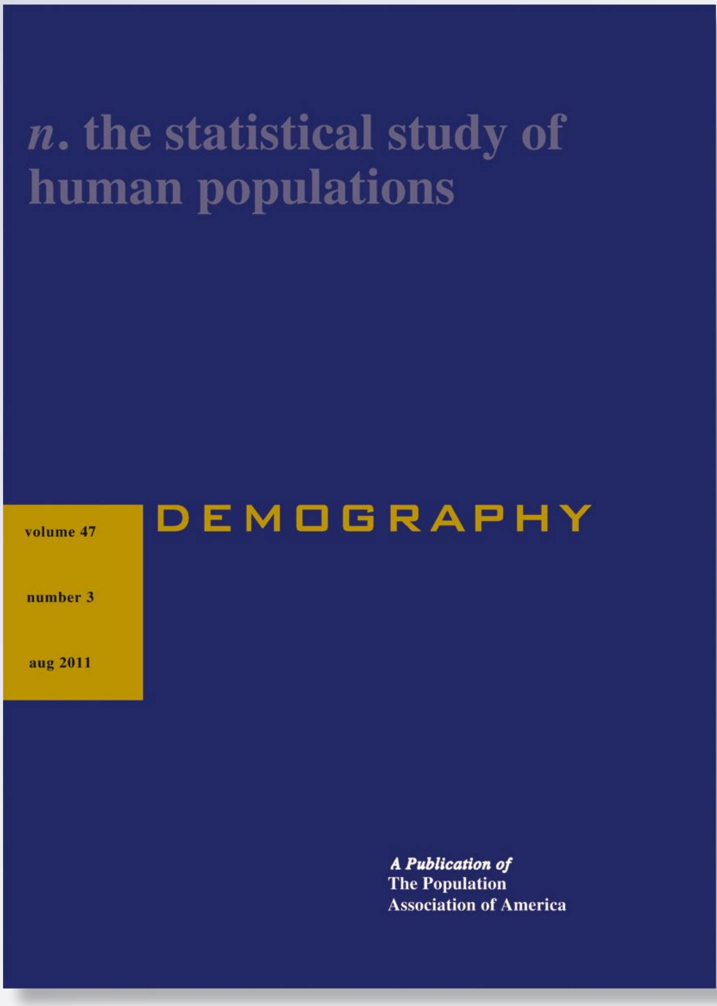

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The Long-Term Effects of Military Conscription on Mortality: Estimates From the Vietnam-Era Draft Lottery

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Abstract Research on the effects of Vietnam military service suggests that Vietnam veterans experienced significantly higher mortality than the civilian population at large. These results, however, may be biased by nonrandom selection into the military if unobserved background differences between veterans and nonveterans affect mortality directly. To generate unbiased estimates of exposure to conscription on mortality, the present study compares the observed proportion of draft-eligible male decedents born 1950–1952 to the (1) expected proportion of draft-eligible male decedents given Vietnam draft-eligibility cutoffs; and (2) observed proportion of draft-eligible decedent women. The results demonstrate no effect of draft exposure on mortality, including for cause-specific death rates. When we examine population subgroups—including splits by race, educational attainment, nativity, and marital status—we find weak evidence for an interaction between education and draft eligibility. This interaction works in the opposite direction of putative education-enhancing, mortality-reducing effects of conscription that have, in the past, led to concern about a potential exclusion restriction violation in instrumental variable (IV) regression models. We suggest that previous research, which has shown that Vietnam-era veterans experienced significantly higher mortality than nonveterans, might be biased by nonrandom selection into the military and should be further investigated.

Keywords Military service · Vietnam draft · Veterans · Mortality · Health

Introduction

Over 8 million men and women served in the military during America's formal involvement in the Vietnam War, with well over 3 million deployed to the theater

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in Southeast Asia (U.S. Department of Veteran Affairs 2008a). In 2008, Vietnam veterans constituted over 30% of the United States' veteran population, the largest period of service cohort by far, and received a greater proportion of service-related disability benefits than veterans of World War II, the Korean Conflict, and the Gulf War combined (U.S. Department of Veteran Affairs 2008b). The National Vietnam Veterans Readjustment Study in the 1980s, for instance, found that the lifetime incidence rate for posttraumatic stress disorder (PTSD), a disorder with symptoms that may persist up to 40 years after service (MacLean and Elder 2007), was over 30% in male Vietnam veterans (Kulka et al. 1990).

The consequences of service for the health and happiness of Vietnam-era veterans has thus been a subject of considerable research (see, e.g., Call and Teachman 1996; Davison et al. 2006; Gamache et al. 2001; Hearst et al. 1986; Hearst et al. 1991; Liu et al. 2005; London and Wilmoth 2006; Settersten 2006). Military combat, which has been associated with increased incidence of physical and mental illness, has important consequences for lifetime health and mortality rates among veterans beyond the immediate risks attributable to exposure to war (Cook et al. 2004; Davison et al. 2006; Gamache et al. 2001; Hearst et al. 1986; Liu et al. 2005; Ruger et al. 2002). In addition to the trauma of combat, some Vietnam-era combatants faced exposure to the environmental toxin, Agent Orange (2,3,7,8-Tetrachlorodibenzo-p-dioxin), which has been associated with Hodgkin's disease, non-Hodgkin's lymphoma, soft-tissue sarcoma, and chloracne (Institute of Medicine 1994). Vietnam veterans also report high rates of recreational drug use, including "hard" drugs like heroin—both in the field and upon return to the United States (Wright et al. 2005). Vietnam veterans typically came home to a hostile and certainly less-supportive social and political environment than did previous generations of veterans, perhaps increasing their stress levels (Angrist and Krueger 1994; Sampson and Laub 1996).

Selection Bias in Estimates of Military Service Effects

While past work suggests that the effects of military service may be both a fruitful and policy-relevant line of inquiry, nearly all studies of veterans, regardless of wartime period, have been plagued by the problem of selection bias. Because entry into the military is typically far from random, the men in the ranks of the armed forces may not be representative of the male population as a whole, thus making identification of a treatment effect of military service difficult at best.[1] Specifically, unobserved differences between veterans and nonveterans may influence substantive outcomes directly and may therefore contaminate efforts to estimate a treatment effect of military service or even an intent-to-treat effect of exposure to conscription.

To illustrate, an early investigation by Seltzer and Jablon (1974) found that even more than 20 years after military service, white male World War II veterans had drastically lower mortality rates compared with the overall white male population. Reduced mortality rates for tuberculosis, cardiovascular disease, and ulcers among

[1] For the purposes of this study, we restrict our analysis of the effects of military service to male veterans simply because during the Vietnam-era, an overwhelming majority of military personnel were men (Kulka et al. 1990).

these veterans were particularly pronounced and underscored the potential long-term impact of selection bias on health outcomes. If military pre-induction screenings weeded out men who appeared unhealthy or otherwise unfit to serve, reduced mortality rates may have reflected not the ameliorative effects of military service but instead a lasting health selection gradient.

Selection effects may work in the opposite direction, as well. For example, Boehmer et al. (2004) found significantly elevated all-cause mortality for Vietnam-deployed veterans as compared with their noncombat veteran counterparts in the first five years after the end of the conflict as well as elevated cardiovascular-related death rates for Vietnam veterans discharged from service after 1970 during the second half of a 30-year follow-up time frame (also see, Settersten (2006) with respect to mental health outcomes). In this case, the entire veteran population during this era could have been more or less healthy than the entire population. Alternatively, those deployed could have been healthier than those not deployed (because combat may be more demanding) or the reverse, wherein those most advantaged avoided deployment through state-side or European-based duties. In other words, assignment to the theater of war was by no means random.

In fact, a "healthy deployer effect" has already been demonstrated in more recent cohorts: combat deployers differ from veterans of noncombat assignments on observables (Armed Forces Health Surveillance Center 2007). During the Vietnam era, however, Gimbel and Booth (1996) showed that those with lower Armed Forces Qualification Test (AFQT) scores and fewer support role skills, as well as higher testosterone levels, were more likely to be exposed to combat. Thus, the comparison of deployed and nondeployed veterans may also be subject to biasing selective pressures.

Causal Estimation Strategies

Between December 1969 and February 1972, the United States Selective Service held four Vietnam draft lotteries. Each draft lottery randomly assigned men in the 1950, 1951, 1952, and 1953 birth cohorts order-of-induction numbers through a hand drawing of 365 birth dates (and 366 for the leap year lottery of the 1952 birth cohort).[2] Following the lotteries, men in a particular birth year classified as either 1-A or 1-A-O were called to report in order for possible induction, up to a yearly draft-eligibility cutoff (see Table 1). (Men available for immediate military service were classified as 1-A, and conscientious objectors to military training and combat were classified as 1-A-O, or "conscientious objector.") Draft lottery numbers, then, are highly correlated with Vietnam military service but are likely uncorrelated with unobservables that directly influence mortality outcomes (see Angrist 1990). For example, draft-eligible white men born in 1950 were 235% more likely than their draft-ineligible counterparts to serve in the military; the corresponding figure for nonwhites was a 102% increased risk. In the 1951 birth cohort, the likelihood of service was increased by 192% and 96%, respectively, for draft-eligible white and

[2] The first draft lottery held on December 1, 1969 actually assigned all men born between 1944 and 1950 order-of-induction numbers. However, for reasons explained shortly, we deal only with the 1950–1952 cohorts.

Table 1 Expected proportion of draft-eligible birth dates by birth year based on randomly assigned Vietnam lottery order of induction numbers, 1950–1952

Birth Year	Eligibility Ceiling	Days in Year	Expected Proportion Eligible
1950	195	365	.5342
1951	125	365	.3425
1952	95	366	.2596

nonwhite men. And in the 1952 birth cohort, draft eligibility raised the likelihood of veteran status by 143% for white men and 81% for nonwhite men.[3] The draft lottery thus provides a unique opportunity to use a random assignment method as a "natural experiment" and cleanly estimate the causal impact of exposure to military conscription on important outcomes net of selection bias.

This randomized "natural experiment" of the Vietnam draft lottery has been used in several studies to sort out the unique experience of the Vietnam-era cohort. As the first to deploy this novel estimation strategy, Hearst et al. (1986) documented the short-term consequences of Vietnam military service on subsequent mortality risk. Between 1974 and 1983, Hearst et al. found that draft-eligible Vietnam era men living in California and Pennsylvania experienced significantly higher mortality rates than draft-ineligible men—particularly from suicide and motor vehicle accidents. However, these results represent not a local average treatment effect (LATE), but rather an intent-to-treat (ITT) effect because actual veteran status was not observed. Later work by Angrist (1990) built on this by using draft eligibility as an instrumental variable (IV) for military service (which was observed in the data) to study income and earnings for male veterans, thereby estimating a LATE for Vietnam-era military service. Angrist's study showed that white veterans experienced a 15% drop in lifetime earnings compared with similar white nonveterans, while nonwhites faced no wage penalty as a result of military service. (It should be noted, however, that because of other behavioral responses to draft lottery number on the part of both the individuals and employers, the effect should rather be interpreted as an estimate of ITT.) Caveats aside, these findings jointly suggest that the effect of military service on the lives of these veterans may be causal instead of simply a remnant of differential selection into the military. Moreover, the work of Angrist (1990) suggests, in line with a significant sociological literature on the ameliorative effects of service for minorities (Lundquist 2004, 2008; Sampson and Laub 1996), that the effects of military service may vary by subpopulation.

More recently, Dobkin and Shabani (2009) deployed confidential data from the National Health Interview Study (NHIS), which included both birth date (for assignment to draft-eligibility status) and veteran status for a two-stage least squares (2SLS) estimation of Vietnam-era military service on health behaviors and morbidity. In the cross-sectional ordinary least squares (OLS) analysis, they found significant, deleterious effects. However, when 2SLS was used, the effects dissipated, and it remained

[3] These figures are based on calculations of the Defense Manpower Data Center Administrative Records combined with cohort size data from the Social Security Administration Continuous Work History Sample, as reported by Angrist (1990).

unclear whether that was due to imprecision on account of a limited sample size or, alternatively, whether there was truly no net causal effect of military service. Further, more recent research using 2SLS estimation by Angrist and Chen (2008) suggests that despite early evidence to indicate that the negative effects of military service may indeed have been causal, such putative deleterious effects may have faded over time. Using the 2000 decennial census, Angrist and Chen (2008) found that the wage penalty for white veterans has dissipated along with the elevated mortality rates previously documented by Hearst et al. (1986) for whites and nonwhites alike.

Important questions remain, however, about the potential causal impact of military service on health outcomes. Although Angrist and Chen (2008) turned up little evidence for a long-term effect of Vietnam-era military service on mortality, their study may have masked variation in the effects of Vietnam-era military service on specific causes of death—as Hearst et al.'s early study implied—as well as subgroup variation in mortality rates. For example, there may be different treatment effects by race, given the work by Angrist (1990) that showed different effects on income by race.

Likewise, results may differ for the college educated versus non–college educated given that postsecondary enrollment was a potential effect of draft eligibility, thus raising concerns by some of an exclusion restriction violation in IV models. By running education groups separately, we can see whether the ITT effect varies by this factor, which may shed some light on this question about alternative casual pathways. Namely, some researchers have suggested that draft-eligible status—both through conscription avoidance and veterans benefits, such as the GI Bill—lead draft eligibles to attain higher levels of education. If such an effect exists, the health-enhancing impact of education may counteract the negative effects of military conscription, deployment, or avoidance. Similarly, education level may moderate—in addition to mediate—effects of draft status because men with different education levels may pursue different strategies in response to draft eligibility, receive different deployments if conscripted, and enjoy differential levels of buffering from putative negative effects of conscription (or risk of conscription). Since education is somewhat endogenous to the military processes that we describe, we cannot adequately answer these questions with a fully identified structural model given existing data. That said, we discuss the implications of the draft status–mortality relationship (and interaction effect) in our Discussion section.

Methods

Our data come from the National Center for Health Statistics multiple cause of death file, 1989–2002. This file contains all deaths in the United States for each calendar year and thus represents the universe of recorded deaths during the time period of interest. Each record in the multiple cause file contains background information on the decedent—taken directly from the decedent's U.S. Standard Certificate of Death—including race, sex, level of education, and cause of death.[4] To assign draft lottery numbers, we also required the

[4] For data years 1989–1998, underlying cause of death is coded using the International Classification of Diseases (ICD) 9th Revision. In 1999, the NCHS began implementation of the updated ICD 10th Revision. Thus, to ensure comparability across data years, we code the underlying cause of death for all years based on the 34-category classification of the ICD-9.

decedent's day, month, and year of birth—identifying information available only in restricted data. All of our analyses were thus performed with restricted data, accessed through the NCHS Research Data Center. The file does not, however, contain information on veteran status; consequently, we estimate only an ITT effect.

Because of problems with the assignments of lottery numbers prior to 1970, we restrict our analyses to those decedents born between 1950 and 1952.[5] To further ensure comparability of our analyses across birth cohorts, we use only the data years for each birth cohort corresponding to ages 39–49 (e.g., data years 1989–1999 for the 1950 birth cohort). Thus, our sample totals 372,128 decedents, including 246,504 men (66.2%) and 125,624 women (33.8%) used for our first analysis strategy detailed below. Of these decedents, 245,088 are non-Hispanic whites (66.2%), 89,589 are non-Hispanic blacks (24.1%), and 27,947 are Hispanic (7.5%), with a remaining 9,504 classified as non-Hispanic "other" (2.6%) on the U.S. Standard Death Certificate.

Using publicly available records of the Vietnam-era lottery results (see http://www.sss.gov/lotter1.htm), we begin by assigning each decedent, women included, the draft lottery number corresponding to his or her date of birth. We then calculate the observed proportion of draft-eligible male and female decedents based on the highest draft lottery number called to report for a given draft year. In 1950, for instance, the highest draft lottery number called to report was 195. Thus, all decedents with a lottery number below or equal to the eligibility cutoff are coded as 1 for draft eligible, and all decedents above the 195 cutoff are coded as 0 for draft ineligible. Expected proportions for each draft eligible birth cohort are given in Table 1.

We next calculate the frequencies of draft-eligible men and women as a proportion of all male and female decedents. For example, for draft-eligible men, we compute

$$\pi_{EM} = \frac{n_{EM}}{n_M}, \tag{1}$$

where n_{EM} is the number of draft-eligible male decedents, n_M is the total number of male decedents, and π_{EM} gives the observed proportion of draft-eligible male decedents of the total number of male decedents in our "sample." Again, we proceed by calculating these relative frequencies for both draft-eligible men (π_{EM}) and draft-eligible women (π_{EF}) using Eq. 1.

After each relative frequency has been calculated for draft-eligible men and women for a variety of subgroups, we use an estimation strategy that deploys two counterfactuals to assess excess mortality among draft-eligible men. First, we compare the observed proportion of draft-eligible men against the observed proportion of draft-eligible women. Women, logically, should not demonstrate a "draft effect." That is, there is no reason to expect that any fertility difference by birth date differed significantly by sex, and thus any significant difference between the proportions of draft-eligible men and draft-eligible women may indicate a "draft exposure" effect.

Our second comparison uses information on the proportion of draft-eligible birth dates in each draft lottery calendar year to detect excess mortality among draft-eligible

[5] The 1970 draft lottery also applied to men born between 1944 and 1949, but most of these veterans had already entered the service prior to the lottery drawing. Thus, the remaining men eligible for induction under the 1970 lottery may not constitute a representative sample as Angrist (1990) indicates.

men. We do this by comparing the observed proportion of draft-eligible men (π_{EM}) to the expected or theoretical proportion of draft-eligible men, given the proportion of men draft-eligible for each birth cohort.[6] We compute the proportion of expected draft-eligible men as

$$\hat{\pi}_{EM} = \frac{max_i}{days_i}, \quad (2)$$

where i represents birth year, max represents the highest lottery number called to serve for the ith birth year, and $days$ represents the number of days in the ith birth year. The difference between the observed proportions of draft-eligible men minus the expected proportion of draft-eligible men ($\pi_{EM} - \hat{\pi}_{EM}$) yields our estimate of excess mortality. Again, we may test the difference of proportions to assess the significance of the draft exposure effect using a basic t test for the difference in proportions.

Because previous research has turned up evidence to suggest that a draft-exposure effect may be most pronounced for certain causes of death, we repeat both of the preceding counterfactual analyses by a variety of causes that have been linked to military service. These causes include malignant neoplasms (cancer), ischemic heart disease, chronic liver disease and cirrhosis, motor vehicle accidents, and suicide. (See Table 6 in the Appendix for coding rules.)

As a final specification check, we run several regression discontinuity (RD) models that regress the frequency of deaths by draft number on draft number in both linear and quadratic form for each birth cohort. Thus, we estimate

$$\hat{y} = \beta_0 + \beta_1 DRAFTNUM + \beta_2 DRAFTNUM^2 + \beta_3 ELIGIBLE + \beta_4 MONTH, \quad (3)$$

where the right-hand-side variables are draft number in both linear and quadratic form; a dummy variable indicator for eligibility status (*ELIGIBLE*); and as a robustness check, a vector of dummy variables for month of birth (*MONTH*).

Results

We begin with results for our first counterfactual, which compares the observed proportion of draft-eligible men with the observed proportion of draft-eligible women for our sample overall and for a variety of subgroups. As shown in Table 2, our first comparison turns up only slight evidence for a draft-exposure effect. The results for the combined sample are largely insignificant with one exception: decedents with 13 or more years of education. In this case, the proportion of draft-eligible male decedents is larger than the proportion of draft-eligible female decedents, indicating a slight draft-exposure effect. Highly educated draft-eligible male decedents thus exhibit excess mortality of about 1.17% compared with draft-eligible women. This is relevant to debates surrounding the interaction between PTSD and education level as

[6] This comparison assumes that birth dates are uniformly distributed across the calendar year, and thus any differences by lottery number are due to an eligibility effect rather than variation in fertility rates. Hence, we also use an alternate "check" using the male-female comparison, which should implicitly correct for the possibility of varying fertility rates.

Table 2 Estimated Vietnam-era draft exposure effect on mortality using mortality among "draft-eligible" female decedents as a counterfactual for draft-eligible male decedents, 1950–1952

	Proportion Draft-Eligible Male Decedents (π_{EM})	Proportion Draft-Eligible Female Decedents (π_{EF})	π_{EM} - π_{EF}
Overall	.3755	.3736	.0019
	[246,504]	[125,624]	(.0027)
Race			
Hispanic	.3754	.3720	.0034
	[20,077]	[7,870]	(.0105)
Non-Hispanic white	.3739	.3750	–.0011
	[163,502]	[81,586]	(.0034)
Non-Hispanic black	.3812	.3710	.0102
	[57,064]	[32,525]	(.0055)
Non-Hispanic other	.3660	.3681	–.0021
	[5,861]	[3,643]	(.0168)
Education			
0–12 years	.3703	.3742	–.0039
	[138,795]	[69,247]	(.0037)
13+ years	.3811	.3694	.0117*
	[78,532]	[43,244]	(.0047)
Nativity Status			
U.S. native	.3760	.3735	.0025
	[225,210]	[115,615]	(.0029)
Nonnative	.3700	.3758	–.0058
	[18,739]	[9,341]	(.0100)
Marital Status			
Never married	.3721	.3710	.0011
	[71,378]	[23,154]	(.0060)
Ever married	.3774	.3742	.0032
	[172,078]	[101,844]	(.0031)

Notes: Sample sizes are given in square brackets, and standard errors are shown in parentheses.

*$p < .05$

well as concerns about education effects of draft eligibility violating the exclusion restriction for estimation of military service on mortality or morbidity. We address these issues in the Discussion section.

We next present results for our second counterfactual. Again, for this comparison, we calculate the observed proportion of draft-eligible male decedents, as earlier, and the expected proportion of draft-eligible men based on the highest-called draft lottery number for each birth cohort. Table 3 presents the results for the sample overall as well as for a variety of subgroups. Here, too, we find little evidence of a draft-exposure effect in the form of excess (or reduced) mortality among draft-eligible male decedents. The one exception is a slight but statistically significant mortality reduction for draft-eligible male decedents with 12 years of schooling or less—again, a

Table 3 Estimated Vietnam-era draft exposure effect on mortality using expected mortality by draft-eligibility cutoff as a counterfactual for observed mortality for draft-eligible male decedents, 1950–1952

	Observed Proportion Draft-Eligible Males (π_{EM})	Expected Proportion Draft-Eligible Males ($\hat{\pi}_{EM}$)	$\pi_{EM} - \hat{\pi}_{EM}$
Overall	.3755	.3788	–.0033
	[246,504]	[248,670]	(.0022)
Race			
Hispanic	.3754	.3788	–.0034
	[20,077]	[20,259]	(.0079)
Non-Hispanic white	.3739	.3788	–.0049
	[163,502]	[165,645]	(.0028)
Non-Hispanic black	.3812	.3788	.0024
	[57,064]	[56,705]	(.0047)
Non-Hispanic other	.3660	.3788	–.0128
	[5,861]	[6,066]	(.0146)
Education			
0–12 years	.3703	.3788	–.0085**
	[138,795]	[141,981]	(.0030)
13+ years	.3811	.3788	.0023
	[78,532]	[78,058]	(.0040)
Nativity Status			
U.S. native	.3760	.3788	–.0028
	[225,210]	[254,088]	(.0024)
Nonnative	.3700	.3788	–.0088
	[18,739]	[19,185]	(.0082)
Marital Status			
Never married	.3721	.3788	–.0067
	[71,378]	[72,663]	(.0042)
Ever married	.3774	.3788	–.0014
	[172,078]	[172,716]	(.0027)

Notes: Sample sizes are given in square brackets, and standard errors are shown in parentheses.

$^{**}p < .01$

finding relevant to debates surrounding education, vulnerability, and other nonconscription effects of draft eligibility.

Next, we conclude our counterfactual analysis with results by particular causes of death. Even though our previous analyses turned up little evidence to indicate a draft-exposure effect, it could be the case that exposure to the draft elevated (or reduced) the probability of mortality attributable to certain conditions as research by Hearst et al. (1986) suggests. Table 4 presents results for each unique cause of death for the first comparison: men versus women. We find no evidence of elevated (or reduced) mortality for draft-eligible men for any of the particular causes of death that have previously been linked to Vietnam-era military service. The differences between draft-eligible men and draft-eligible women for each cause are small and statistically

Table 4 Estimated Vietnam-era draft exposure effect on mortality using mortality among "draft-eligible" female decedents as a counterfactual for draft-eligible male decedents by cause of death, 1950–1952

Cause of Death	Proportion Draft-Eligible Male Decedents (π_{EM})	Proportion Draft-Eligible Female Decedents (π_{EF})	π_{EM} - π_{EF}
Malignant Neoplasms	.3703	.3761	–.0058
	[40,045]	[24,185]	(.0064)
Ischemic Heart Disease	.3780	.3808	–.00228
	[31,011]	[8,271]	(.0097)
Chronic Liver Disease and Cirrhosis	.3776	.3821	–.0045
	[12,863]	[4,166]	(.0140)
Motor Vehicle Accidents	.3809	.3763	.0046
	[13,140]	[5,554]	(.0126)
Suicide	.3665	.3753	–.0088
	[14,857]	[4,365]	(.0136)

Notes: Sample sizes are given in square brackets, and standard errors are shown in parentheses.

insignificant. Lastly, Table 5 displays the cause-of-death results for the second counterfactual, which compares the observed proportion of draft eligible male decedents in our sample to a theoretical or expected proportion of draft-eligible decedents based on the highest lottery number called to report for service. Here, too, we find no evidence of a draft exposure effect on eligible male decedents. All calculated differences are small and statistically insignificant.

Finally, estimation of several regression discontinuity models corroborates the results of our counterfactual analyses. (Results available upon request.) There appears to be no draft exposure effect for any of the 1950–1952 birth cohorts even after controlling for month of birth and adding an interaction term for draft eligibility and birth year.

Table 5 Estimated Vietnam-era draft exposure effect on mortality using expected mortality by draft-eligibility cutoff as a counterfactual for observed mortality for draft-eligible males by cause of death, 1950–1952

Cause of Death	Observed Proportion Draft-Eligible Males (π_{EM})	Expected Proportion Draft-Eligible Males ($\hat{\pi}_{EM}$)	$\pi_{EM} - \hat{\pi}_{EM}$
Malignant Neoplasms	.3703	.3788	–.0085
	[40,045]	[40,964]	(.0056)
Ischemic Heart Disease	.3780	.3788	–.0008
	[31,011]	[31,077]	(.0063)
Chronic Liver Disease and Cirrhosis	.3776	.3788	–.0012
	[12,863]	[12,904]	(.0098)
Motor Vehicle Accidents	.3809	.3788	.0021
	[13,140]	[13,068]	(.0097)
Suicide	.3665	.3788	–.0123
	[14,857]	[15,356]	(.0092)

Notes: Sample sizes are given in square brackets, and standard errors are shown in parentheses.

Discussion

Given the considerable difficulties involved in identifying a true treatment effect of military conscription, our study contributes to a growing body of knowledge about the long-term health consequences of military service in general and service during the Vietnam era in particular. Using a novel estimation strategy, we assessed—and detected very little—excess mortality for the 1950–1952 draft lottery cohorts.

We found little convincing evidence of a lasting draft eligibility effect on the mortality of Vietnam-era birth cohorts after the initial ill effects observed by Hearst et al. (1986). Our analysis indicates that mortality for the draft-eligible population appears unaffected compared with both the expected proportion of eligible men by birth date and the female population regardless of educational attainment and nativity status. Similarly, analyses by underlying cause of death turned up little evidence that exposure to the draft may have elevated mortality attributable to, for example, suicide or motor vehicle accidents.

Using a complementary data source to that of Angrist et al. (2010), we attempted to flesh out the long-term effects of Vietnam draft eligibility (i.e., ITT) on mortality outcomes. The mortality data used here are complementary to the census data in that the census measures men still alive and residing in the United States while the death records we used account for those who died in the United States. Although both studies assumed random birth dates with respect to draft eligibility, Angrist et al. (2010) estimated excess morality by counting how many men are "missing" from the 2000 census differentially by draft lottery assignment status. The problem with this strategy is that men could be missing from the census not just because of death, but also because of emigration (or, to a lesser extent, homelessness or some other condition that makes their enumeration more difficult). In contrast, our approach starts with the same assumption of random births with respect to draft lottery status but then flashes forward to see who is recorded as deceased, differentially by draft treatment status. In our case, we assume that the men missing from our data set are indeed alive because deaths tend to be better recorded than live individuals. Whether those individuals are alive here, in Canada, or unreachably in the streets does not matter for our analysis. However, if one of the effects of draft lottery treatment status was to induce a very stressful move abroad—and, in turn, this emigration resulted in a greater than expected rate of early death—we would not detect that effect in our analysis because such individuals would have died outside the records of our data set. Indeed, the effect of out-migration appears to our analysis to be a life-prolonging effect of eligible draft status because it prevents those individuals from appearing in our universe, leading to an underestimation of the ITT effect. By contrast, the Angrist et al. (2010) approach treats those missing individuals as dead, thereby leading to an overestimation of the effect. Through the complementary parallax of the two studies, however, we can be more certain that such biases are not at work in any meaningful way.

Our analysis, on the contrary, suggests that the short-term elevation in mortality rates following combat may have dissipated over time or may have been a remnant of sampling (in the case of Hearst et al. (1986)). Taking our study alone, earlier elevated mortality as documented by Hearst et al. (1986) might obscure later elevated mortality by eliminating those decedents from the risk pool during our study's time frame.

But when combined with the lack of "missing" veterans from the still-living sample of Angrist et al. (2010), the two studies suggest that early post–Vietnam War mortality did not substantially affect these cohorts. Namely, although individuals who died prior to the earliest recorded deaths in our data set would appear to us to still be alive and thus bias our results downward, the complementary analysis of still-alive men in the census and American Community Survey (ACS) records should pick up these earlier deaths no matter when they occurred. Thus, we feel safe in concluding that such deaths do not significantly bias our risk estimates for the period of our study (1989–2002); likewise, we assume that any earlier effects of draft lottery status were small enough that they are washed out in the long-term analysis of Angrist et al. (2010). Alternatively, there remains the possibility set forth by Boehmer et al. (2004) that heterogeneity in exposure to combat generated significantly elevated mortality among combat veterans but a net health benefit for those who did not deploy. While our analysis ultimately cannot distinguish the effect of conscription on mortality by terms of service, we can address past claims that estimate excess mortality for the draft-eligible population as a whole.

Finally, it could be that our ITT averages 0 because of two counteracting factors: the confounding effects of stress/trauma raise mortality, while the increased educational attainment for some cohorts (1950 but not 1951, according to Angrist et al. (1996), for example) lowers mortality. Angrist et al. (2010) showed that through GI Bill eligibility, veterans during this period enjoyed higher postwar educational attainment. Likewise, Card and Lemieux (2001) argued that draft avoidance also led to some increased college enrollment and attainment—particularly among draft-eligible men. In addition to acting as a mediating variable, educational attainment may play a role as a moderator of the draft-eligibility effect given research suggesting that those with less education are more prone to PTSD (Macklin et al. 1998).

With respect to positive effects of draft eligibility on educational attainment (and, in turn, negative effects of educational attainment on mortality rates), we can only speculate in light of other literature. In fact, the differential treatment effects of draft eligibility on deployment (negative for longevity) and education (positive for life expectancy) may very well explain why Boehmer et al. (2004) found significant—although tapering—mortality effects of deployment status during the era of U.S. combat in Vietnam but we did not. It is entirely possible that those deployed suffered from negative long-term health consequences that were not adequately compensated by the health benefits of increased educational opportunity (through the GI Bill or deployment avoidance). Meanwhile, for those draft-eligible veterans and nonveterans alike who did not experience the negative effects of wartime, deployment may have a net positive effect of draft eligibility that, when averaged with the negative effects that Boehmer et al. described, results in the net zero effect we found here.

Second, by splitting our sample by educational attainment, we can begin to shed some light on the question of whether education moderates the mortality effects of draft eligibility. Using one methodology (a comparison of observed and expected draft-eligible men), Vietnam-era draft eligibility reduces mortality among those men with less than a college education, suggesting that the college enrollment hypothesis is probably not at work in explaining this anomaly. Rather, it is possible that for the less-educated, military service provides a positive effect on labor market outcomes and that other veteran benefits (such as health care) reduce mortality to a greater extent than the risks associated with conscription increase them. For a more advantaged group (the college educated) with better overall labor market prospects, good health care outside the VA system, and so on, such effects do not manifest themselves.

Further, with our alternate method—a comparison of draft-eligible men with draft eligible women—the effect becomes significant for those with at least some college education. However, this also works against the college enrollment or GI Bill hypothesis because here the findings reveal elevated mortality for draft-eligible, college-educated men. Thus, the effect works in the opposite direction as the education hypothesis (and its concordant violation of the exclusion restriction) would tell us. Perhaps, then, researchers should not be as concerned with increased education as an alternative pathway affecting the military conscription-mortality relationship. Rather, it could be that absent the salutary effects that men of low socioeconomic status (SES) differentially experience from military service, high-SES men experience only the "trauma" effect. However, these are only tentative conclusions because educational attainment is, of course, somewhat endogenous to the military conscription and deployment process. Thus, without a fully identified structural equation model, which is not possible to construct given existing data limitations, unpacking the relationships among military service, education, and mortality is left to further research.

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Appendix

Table 6 Coding classifications for underlying cause of death

Cause of Death	ICD-9[a]	ICD-10[b]
Ischemic Heart Disease	410–414	I20–I25
Chronic Liver Disease and Cirrhosis	571	K70, K73–K74
Motor Vehicle Accidents	E810–E825	V02–V04, V09.0, V12–V14, V19.0–V19.2, V19.4–V19.6, V20–V79, V80.3–V80.5, V81.0–V81.1, V82.0–V82.1, V83–V86, V87.0–V87.8, V88.0–V88.8, V89.0, and V89.2
Suicide	E950–E959	U03, X60–X84, and Y87.0
Malignant Neoplasms[c]	150–159, 160–165, 188–189, 204–208, 140–149, 170–173, 190–203	C16, C18–C21, C25, C33–C34, C64–C68, C82–C85, C91–C95, C00–C15, C17, C22–C24, C26–C32, C37–C49, C51–C52, C57–C60, C62–C63, C69–C81, C88, C90, C96–C97

[a] Years 1989–1998.

[b] Years 1999–2002.

[c] Excludes neoplasms of the breast and genital organs for both men and women decedents.

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