LONG-TERM EFFECTS OF VIETNAM-ERA CONSCRIPTION:

SCHOOLING, EXPERIENCE AND EARNINGS

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Abstract

Instrumental variables (IV) estimates using the draft lottery show that white Vietnam-era draftees suffered substantial post-service earnings losses in the 1970s and 1980s. Angrist (1990) explains these losses as due primarily to lost labor market experience. Non-public use data from the 2000 Census allow the first longerterm follow-up for a large sample from the draft-lottery cohorts. We use these data to estimate the effects of military service on earnings, schooling, and a number of other variables. Consistent with the loss-of -experience model, IV estimates of the effects of Vietnam-era service on earnings are close to zero in 2000, when the draft-lottery cohorts were middle-aged and experience profiles relatively flat. On the other hand, draft-lottery estimates show a marked increase in schooling for Vietnam-era veterans. A variety of evidence suggests this increase reflects the impact of the Vietnam-era GI Bill more than draft-avoidance behavior. The economic return to the increased schooling generated by Vietnam-era service, estimated in a wage equation that constrains the impact of Vietnam-era military service to run solely through the experience and schooling channels, appears to be less than the OLS return. Finally, we look at measures of disability. The IV estimates point to an increase in non-work-related disability rates and non-SSA disability income, but the fact that there is no corresponding effect on employment, hours worked, or work-related disability rates suggests health was affected little by Vietnam-era service. Allowing for excess disability among veterans raises the estimated returns to GI-Bill schooling slightly.

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I. Introduction

Conscription ended in July of 1973, but interest in the long-term consequences of compulsory military service remains strong. The intellectual case against conscription was largely the work of economists involved with the Gates commission, especially Walter Oi, who was the first to estimate the cost of conscription to draftees (Oi, 1967). Many other economists contributed to the case against the draft as well, both through scholarship and advocacy (Henderson, 2005). Economists argued that conscription amounts to a hidden tax on soldiers, with the further disadvantage of uncertainty as to who exactly would have to pay.

The negative view of the draft notwithstanding, comparisons of veterans and non-veterans typically show veterans with somewhat better civilian outcomes than non-veterans in WWII cohorts and only modestly worse outcomes for cohorts from the Vietnam era.[1] A fundamental difficulty with these simple comparisons, however, is selection bias. The process of screening for military service generates a pool of veterans that differs in important ways from non-veterans. For example, in a comparison of the civilian mortality risk of WWII veterans with others from the same cohorts, WWII veterans had lower death rates, primarily due to a reduced risk of deaths from disease (Seltzer and Jablon, 1974). This is almost certainly an artifact of health-related selection bias.

Although not as obvious as the selection bias inherent in comparisons of mortality, selection bias is also a concern in studies of the economic effects of the draft. The military enlistment process selects soldiers on the basis of factors related to earnings potential in at least two ways. On one hand, the military prefers soldiers to be high school graduates, and screens out those with very low test scores. For this reason, men with very low earnings potential are unlikely to end up as soldiers.[2] On the other hand, some potential recruits find military service attractive precisely because their prospects in the civilian labor market are poor, while those with the highest earnings potential might find it worthwhile to work hard to escape the draft. The net selection bias in this case is unclear.

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[1]See, for example, studies cited in Angrist and Krueger (1994).

[2]See, e.g., Eitelberg, *et al* (1984).

The draft lottery provides an opportunity to overcome the problem of selection bias in estimates of the economic and other consequences of Vietnam-era military service. From 1970-72, induction priority was determined by a series of lotteries in which cohorts at risk of conscription were assigned random sequence numbers (RSNs) from 1-365. Men were then called for pre-induction processing from lowest-to-highest, up to a ceiling (e.g., 195 in 1970). Because draft lottery numbers are highly correlated with veteran status, and probably uncorrelated with other factors related to earnings, lottery RSNs can be used to construct instrumental variables (IV) estimates of the causal effect of Vietnam-era conscription.

Angrist (1990) used the draft lottery to construct IV estimates of the effect of compulsory military service on civilian earnings through 1984. For white veterans these estimates show substantial earnings losses - as much as 15 percent of average civilian earnings in the cohort.[3] However, the negative impact of military service appears to have decreased over time. Angrist (1990) argued that the time series pattern of estimated earnings losses is explained by lost labor market experience. In particular, estimates of a log-quadratic potential-experience model suggest military service generates earnings losses equivalent to roughly two years of lost experience, the period of service for draftees. An implication of the loss-of-experience story is that Vietnam-era draftees should eventually catch up with non-veterans, though they had not done so by 1984, when the draft-lottery cohorts were in their early-to-mid thirties.

The empirical analysis in this paper begins with new estimates of the long-term effects of Vietnam-era service. The 2000 Census long form, which collects information on exact dates of birth, provides an opportunity to look at the economic and other consequences of Vietnam-era conscription as the draft-lottery cohorts approach age 50. Although birthday information is not made publicly available, we have used it through an agreement with the Census Bureau's Center for Economic Studies. Among other things, this allows us to check the loss-of-experience interpretation of the veteran earnings penalty. We also look at other outcomes, most importantly schooling and disability status.

[3]Angrist (1989) estimated larger though imprecise effects on white veteran's wages using a smaller sample. Estimates for nonwhites are not significantly different from zero in either the 1989 and or 1990 studies.

In addition to estimating the long-term impact of Vietnam-era conscription, we offer an interpretation of these effects using a Mincer-style wage equation. Our interpretation is motivated by two findings. First, the earnings effects (as well as effects on other labor market variables such as employment) are close to zero, consistent with the overtaking age of about 50 estimated for veterans in Angrist (1990). Second, the 2000 census data show a marked impact of Vietnam-era conscription on schooling. For a variety of reasons, detailed below, we believe that the increased schooling received by Vietnam-era cohorts reflects the impact of the Vietnam-era GI Bill more than education-related draft deferment. We then argue that the net wage effects observed in the 2000 data can be explained by a flattening of the experience profile in middle age and a modest return to the post-service schooling paid for by veterans benefits.[4] Although there is some evidence of an increase in non-work-related disability rates, adjusting the wage estimates for these health effects matters little.

The paper is organized as follows. The next section describes the 2000 Census data and reports the draft-lottery first stage from these data. Section III discusses instrumental variables estimates of effects of Vietnam-era conscription on labor market outcomes and Section IV discusses instrumental variables estimates of effects on schooling. Motivated by earlier studies of the consequences of Vietnam-era military service for health, especially the Hearst, Newman and Hulley (1986) study using the draft lottery, Section V reports estimates of the effects of Vietnam-era service on disability status and disability income. Section VI discusses estimates of a human capital earnings function that assigns all veteran effects to operate through the schooling and experience channels. Finally, the paper concludes in Section VII.

II. Data and First-Stage

A. The 2000 Census 1-in-6 File

The 2000 Census long form sample includes approximately one-sixth of US households. The original

[4]The first attempt to estimate the returns to veterans post-service schooling is Griliches and Mason (1972), who report results for a sample of WWII veterans from the 1964 CPS. Other related work is discussed below.

1-in-6 long form sample is the basis for the publicly available 5% long-form sample that is widely used in academic research. The publicly available samples are simple random samples of the 1-in-6 file, though the 1-in-6 file is not a simple random sample from the census sampling frame. Rather, the Census Bureau reduces the sampling rate in more densely populated areas. Adjustment for variation in sampling rates is made by using the weighting variables that are included in the long-form data file. These weights adjust for non-response as well as for non-random sampling, and are designed to match external population totals by age, race, sex and Hispanic origin.[5]

For the purposes of this study, we created an extract of US-born men residing in the 50 States and the District of Columbia, born between 1948 and 1953 or in subsets of these birth years. The cohorts of 19-year-olds at risk of conscription in the draft lotteries were born 1950-52 so much of our analysis focuses on the sample of men in this age group, which includes about 700,000 whites and 96,000 nonwhites. There is a smaller but still substantial draft-lottery impact for men born in 1948 and 1949, so we also report results for samples born 1948-52, which include about 1.14 million whites and 155,000 nonwhites. Finally, although no one born after 1952 was drafted, men born in 1953 were assigned RSNs and a few volunteered in anticipation of possible conscription. We therefore report first-stage estimates for this cohort as well.

Roughly 24 percent of men born 1950-52 served in the Vietnam era and about 38 percent were draft-eligible. These and other descriptive statistics appear in Table 1, which reports means by veteran status and race for the 1950-52 sample. White veterans from these cohorts have lower employment rates and earnings than white non-veterans, while the pattern is reversed for nonwhites. For example, the annual 1999 earnings of white veterans was about $39,500, while white non-veterans earned $48,500 that year. Unemployment rates are low in both the veteran and non-veteran groups, but many men, especially nonwhites, were out of the labor force.

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[5]Publicly available long-form data are documented in US Census Bureau (2005). In practice, weighting appears to matter little for the results in our study. We also confirmed that the means from publicly available data from the 1-in-6 file are close to those from the 5% file distributed through IPUMS. The original 2000 long form sample includes Puerto Rico and island territories; residents of these areas are omitted from our study.

The contrast in average educational attainment by veteran status parallels that in earnings, with white veterans obtaining less schooling and non-white veterans obtaining more schooling than their non-veteran counterparts.[6] The difference in the average schooling of white men by veteran status masks a more complicated distributional difference: white veterans are more likely than non-veterans to be high school graduates, but less likely to have attended or completed one or more years of college. For non-whites, the schooling differential by veteran status changes in favor of non-veterans only at the BA level or higher.

A large number of men born in 1950-52 report having a work-related disability – about 12 percent of whites and 21 percent of nonwhites. The proportion reporting other disabilities, but no disability related to work, is 7 percent for whites and 12 percent for nonwhites. The work and non-work disability variables used here are mutually exclusive. Thus, a total of 19 percent of whites and 33 percent of nonwhites are affected by some kind of disability (i.e., the sum of work and non-work disability rates).[7] White veterans have somewhat higher disability rates than non-veterans, while disability rates differ little by veteran status for nonwhites.

Veterans below the age of 65 are eligible for two types of disability payments, service-related veterans disability compensation (VDC) and means-tested military disability pensions (MDP) for disabled low-income veterans. We therefore look at the effects of Vietnam-era service on four disability-related income variables: an "other income" variable that includes VDC; a non-Social-Security (SSA) retirement and

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[6]We use Jaeger (1997) to impute highest grade completed from the categorical Census schooling variable.

[7]The disability variables used here come from two questions on disabilities by type. Respondents were asked: [Question 17] "Because of a physical, mental, or emotional condition lasting 6 months or more, does this person have any difficulty in doing any of the following activities: (a) learning, remembering or concentrating (b) dressing, bathing or getting around inside the home (c) going outside the home alone to shop or visit a doctor's office (d) working at a job or business?" The previous question [16] asks "Does this person have any of the following long-lasting conditions: (a) Blindness, deafness, or a severe vision or hearing impairment (b) A condition that substantially limits one or more basic physical activities such as walking, climbing stairs, reaching, lifting, or carrying?" We code a work-related disability for those who answer yes to (17d). Other disabilities were coded as follows: (16a) vision or hearing; (16b) physical; (17a) mental; (17b) self-care; (17c) mobility. Respondents can have more than one disability. We coded a non-work disability for anyone who responded yes to 16a, 16b, 17a, 17b, or 17c, i.e., they indicated some disability, but no work-related disability (17d). In this scheme, the presence of any disability is indicated by the sum of the work-related and non-work disability indicators.

disability pension variable that includes MDP; and two SSA variables that include income from Social Security Disability Insurance (SSDI) and Supplemental Security Income (SSI).[8] Table 1 shows much higher amounts for veterans in the other income and non-SSA retirement and disability categories. This is almost undoubtedly due to veterans receiving VDC and MDP. In contrast, the difference in SSDI and SSI amounts by veteran status is much smaller.

Table 1 focuses on men born 1950-52 because these cohorts were most affected by the draft lottery. As noted, above, however, we also present results for an expanded sample of men born 1948-52. Appendix Tables A1 and A2 show descriptive statistics for the 1948-52 sample, as well as means by single year of birth for men born 1948-53. The 1948 and 1949 cohorts were more likely to serve in the Vietnam era than the 1950-52 group, with Vietnam-era service rates peaking at 45 percent for whites born in 1948. Men born in 1953 were less likely to serve, though many still did (14 percent of whites and 18 percent of nonwhites).

B. The Draft-Lottery First Stage

The first draft lottery, held in December 1969, affected men born 1944-50 at risk of conscription in 1970, while subsequent draft lotteries affected 19-year-olds only. Men born in 1951 were at risk of conscription in 1971 and men born in 1952 were at risk of conscription in 1972. Men born in 1953 were assigned lottery numbers in 1972, but there were no draft calls in 1973. Although men as old as 26 could have been drafted as a result of the 1970 lottery, the risk of conscription for all cohorts affected by a lottery was limited to a single year.

Each lottery was associated with a draft-eligibility ceiling or cut-off. Those with an RSN below the ceiling were draft-eligible while those with an RSN above the ceiling were draft-exempt. The draft-eligibility ceilings were 195 in the 1970 lottery, 125 in the 1971 lottery and 95 in the 1972 lottery. Draft eligibility is

[8]The non-SSA retirement income and disability pension question states 'Do not include Social Security," but includes military pensions, while the "other income" question explicitly mentions "VA payments," which presumably includes VDC. In practice, veterans may confuse VDC and MDP payments though they could not receive both before 2003. Another difference is that VDC is not taxable while disability pensions are.

highly correlated with Vietnam-era veteran status, but the link is far from deterministic. Many men with draft lottery numbers below the ceiling were able to avoid conscription (via occupational or educational deferment, poor health, or low test scores) while many with lottery numbers above the ceiling volunteered for service. Throughout the Vietnam era (1964-1975), most soldiers were volunteers.

In the sample of men born 1950-52, the effect of draft eligibility on Vietnam-era veteran status was .145 for whites and .094 for nonwhites. These and other draft-eligibility effects are reported in the first rows of Table 2 (Panel A for whites and Panel B for nonwhites). The table also shows draft-eligibility effects for the pooled sample of men born 1948-52. These effects are somewhat smaller than in the younger subsample, .11 for whites and .072 for nonwhites, because the draft-eligibility first-stage is smaller for men born in 1948 and 1949 than for men born in 1950. This is not surprising since many of those who served in the older cohorts had entered the military before the 1970 draft lottery. Table 2 also documents a small draft-eligibility first stage for the 1953 cohort (about .031), where "draft-eligibility" was coded using the 1972 lottery cutoff of 95. Because the effect on men born in 1953 is small, we omit this cohort from the empirical analysis that follows. Draft-eligibility effects for men born 1944-47 (not reported here) are smaller than those for men born 1953 so we omit these cohorts as well.

The most important feature of the relation between lottery numbers and military service is the drop in the probability of service at the draft-eligibility cutoff. This can be seen in Figure 1, which plots estimates of the conditional probability of service given lottery numbers for men born 1950-53. The figure shows smoothed estimates across 5-RSN cells by single year of birth, but the smoothing does not straddle the draft-eligibility cutoff in each cohort.[9] The figure also documents modest variation in the probability of service within draft-eligibility groups. Part of this variation is due to higher voluntary enlistment rates among men with low lottery numbers – men who volunteered could expect more choice regarding terms of service (e.g.,

[9]Estimates were smoothed using lowess with a bandwidth of .4 and the standard tricube weighting function.

choice of branch of service), while draftees mostly served in the Army.[10] Another important feature of Figure 1 is the muted relation between veteran status and lottery numbers for nonwhites. Angrist (1991) shows that this can be explained by the fact that nonwhites were more likely than whites to see military service as an attractive career option.

Additional Instrument Sets

Let V_i denote individual i's veteran status and R_i his RSN. In principle, the efficient IV estimator in a model with covariates W_i uses a non-parametric estimate of $E[V_i| W_i, R_i]$ as an instrument for V_i (assuming constant coefficients and homoscedastic residuals; see Newey, 1990). We might therefore use the fitted values plotted in Figure 1 as instruments (where the covariates are year of birth and race). In practice, however, our experiments with alternative models produced little efficiency gain from a non-parametric first stage or draft- lottery instruments coded finely. We therefore chose to work with a compact first-stage, starting with a just-identified model using draft-eligibility status as the single instrument. This is an attractive starting point since just-identified IV estimates are median-unbiased, though they have no moments.[11]

In addition to just-identified estimates, we also use a set of five unrestricted lottery-group dummies chosen to match draft-eligibility cutoffs for each cohort, with allowance for additional draft-motivated enlistment as high as RSN 230. The *5z* instrument set is $\{z_{1i}, z_{2i}, z_{3i}, z_{4i}, z_{5i}\}$ where

$z_{1i} = 1[RSN_i \leq 95]$,

$z_{2i} = 1[95 < RSN_i \leq 125]$,

$z_{3i} = 1[126 < RSN_i \leq 160]$,

$z_{4i} = 1[161 < RSN_i \leq 195]$,

[10]On the other hand, volunteers typically served for three years while draftees served for two (see, e.g., the terms of service quoted in Angrist, 1991).

[11]Median-unbiasedness of 2SLS in just-identified models is implied by the equality of 2SLS and LIML in the just-identified case. The finite-sample behavior of LIML is discussed in, e.g., Anderson, Kunitomo, and Sawa (1982) and Angrist, Imbens, and Krueger (1999).

$$z_{5i} = 1[196 < RSN_i \leq 230],$$

and $1[\cdot]$ is the indicator function. This allows for kinks at each draft-eligibility cutoff, while breaking the set of lottery numbers up into roughly equal groups between RSN 95, the lowest cut-off, and RSN 230, beyond which the effect of lottery numbers on enlistment is negligible. Note that the draft eligibility dummy, $ELIG_i$, can be constructed from the elements of *5z* as follows

$$ELIG_i = 1[YOB_i \leq 50](z_{1i}+z_{2i}+z_{3i}+z_{4i}) + 1[YOB_i = 51](z_{1i}+z_{2i}) + 1[YOB_i \geq 52](z_{1i}),$$

where YOB_i is i's year of birth. This can also be written

$$ELIG_i = z_{1i} + 1[YOB_i \leq 51](z_{2i}) + 1[YOB_i \leq 50](z_{3i}+z_{4i}),$$

which shows that $ELIG_i$ involves both lottery-number main effects and interactions with year of birth.

Estimates of the *5z* first stage are reported in Table 2 below the estimated draft-eligibility effects. The first two columns in each panel report estimates from pooled samples.[12] For example, column 1 shows that men born 1950-52 with RSNs up to 95 were .16 more likely to serve than men with RSNs above 230 (the reference group). The next group, with RSN 96-125, was .091 more likely to serve than the reference group; the next group was .059 more likely to serve; the next group after that was .04 more likely to serve; and the last group with RSN 196-230 was .0065 more likely to serve. All of these first-stage effects are precisely estimated and significantly different from zero. As with the draft-eligibility effects, estimates of *5z* effects are consistently smaller for nonwhites than for whites. F-statistics in the pooled 1950-52 and 1948-52 samples range from 134 for nonwhites to over 2400 for whites.

In a further effort to increase precision, we experimented with an instrument list constructed by interacting the *5z* instrument set with a full set of dummies for year of birth. This leads to an instrument set, labeled *5zx*, that includes 15 instruments for the 1950-52 sample and 25 instruments for the 1948-52 sample. Estimates of the *5zx* first-stage are given in columns 3-8 of Table 2, which reports the effects of the elements of *5z* by single year of birth. The effects of lottery number groups fade more quickly for younger cohorts than

[12]The estimates in Table 2 control for year of birth, State of birth, and month of birth.

for older since younger cohorts were subject to lower draft-eligibility cutoffs. On the other hand, there are small effects above the cutoff for some cohorts. For example, even though the 1971 draft-eligibility cutoff was 125, men born in 1951 with lottery numbers between 126 and 160 were .05 more likely to serve than men with lottery numbers above 230. We might therefore expect the *5zx* instrument to generate efficiency gains relative to both ELIG_i and *5z*.

Finally, when estimating the returns to schooling, we tried an instrument set that breaks the below-95 RSN group up into three parts for a total of 7 RSN dummies, generating 35 instruments when the 7 RSN groups are interacted with year of birth in the 1948-52 sample (*7zx*). The additional instruments are dummies for RSN 1-30 and RSN 31-60. Most of our substantive conclusions, however, are based on just-identified models or models estimated with a low degree of over identification.

C. Sample Selection Issues

A final issue before turning to the second-stage estimates is the possibility of selection bias due to excess mortality among draft-eligible men. Selection bias might arise if veterans suffered excess mortality during or after their service, in which case the mix of draft-eligible and ineligible men in the 2000 Census is distorted relative to the random-assignment baseline. There are two likely channels for this. The first is war-related deaths. The second is elevated post-service mortality due to injuries with long-term consequences, service-related trauma, difficulty adjusting to civilian life, or other long-term consequences of military service such as an increased likelihood of smoking (as suggested by Bedard and Deschenes, 2006, for WWII veterans).

Roughly 47,000 men died as a result of hostile action in the Vietnam Era (1964-75) while 8.7 million personnel served in the military during this period. Overall casualty rates among Vietnam-era veterans were low in part because less than half of active duty personnel served in Indochina, and many served in positions not exposed to combat. Although causality rates among draftees were higher than the overall death rate (because most draftees served in the Army), draftees accounted for a minority of combat deaths. Moreover,

over 80 percent of combat deaths occurred before 1970.[13] It therefore seems unlikely that war-related deaths have a large effect on the composition of the sample used in our study.

Excess civilian mortality for veterans seems more likely to affect the composition of post-Vietnam samples than combat-related deaths, especially in view of Hearst, Newman and Hulley's (1986) findings of elevated civilian mortality for draft-eligible men. The excess deaths in the Hearst, Newman and Hulley study were due to suicide and motor vehicle accidents, possibly the result of post-traumatic stress disorder (PTSD). We discuss this finding further in our analysis of health effects in Section VI. At this point, our primary interest is in implications of differential mortality for the composition of the sample.

As a simple check on the possibility of mortality-related selection bias we compared the actual and expected number of draft-eligible men in the 2000 Census by race and year of birth. Following Hearst, Newman, and Hulley (1986), the expected ratio is computed assuming birthdays (and hence lottery numbers) are uniformly distributed. Overall, draft-eligible men are represented in the census sample almost exactly as predicted assuming a uniform distribution of lottery numbers. Among whites, the predicted proportion eligible is .40553, while the empirical proportion eligible is .40539. Among nonwhites, there are slightly *more* eligibles than predicted, .4085 versus .4038.

Among whites, comparisons by single year of birth for men born 1948-53 show draft-eligible men slightly over-represented in three cohorts and slightly under-represented in 3 cohorts (one of these is the 1953 cohort, with no draftees). Some of these differences are significant, though all are small. Only one out of six cohort-specific contrasts is significant for nonwhites, with slightly more eligibles than predicted for nonwhites born in 1950.[14] Given the size and sign pattern of these comparisons, it seems unlikely that excess civilian mortality has a substantial effect on the composition of the 2000 Census sample. In addition, this

[13]Service and casualty statistics are from Table 583 in the 2000 Statistical Abstract, available on-line at http://www.census.gov/prod/2001pubs/statab/sec11.pdf. Data on casualties by year can be found on a resource page available from the national archives: http://www.archives.gov/research/vietnam-war/casualty-statistics.html#year. Statistics on service in Indochina and exposure to combat are from Hearst, Newman and Hulley (1986).

[14]The estimates behind this discussion are given in Appendix Table A3.

comparison weighs against the view that Vietnam-era service led to elevated civilian mortality. We return to this point in a direct examination of health outcomes in Section V.

III. Labor-Market Effects

We begin with the long-run consequences of conscription for earnings and other labor market variables. The results reported here are 2SLS estimates of the parameter α in the equation

$$Y_i = W_i'\beta + \alpha V_i + \epsilon_i, \quad (1)$$

where Y_i is an outcome variable (e.g., earnings); W_i is a vector of covariates that includes year of birth dummies, state of birth dummies, and month of birth dummies; and V_i denotes Vietnam-era veteran status. Year of birth is a necessary control in models identified by the exclusion of draft-eligibility since older men were more likely to be eligible. Month of birth adjusts for any bias arising from the fact that the 1970 lottery, the only one to use physical randomization, resulted in a lottery sequence correlated with month of birth (in practice this does not appear to be important). State of birth is a natural pre-treatment control, inclusion of which might increase the precision of second-stage estimates. As a benchmark, ordinary least squares (OLS) estimates of equation (1) are also reported.

As discussed in Angrist (1990), 2SLS estimates of equations like (1) can be expected to capture the effect of service on those who were drafted or who volunteered in the face of draft risk. More generally, the 2SLS estimates in this case can be interpreted as the local average treatment effect (LATE) of Vietnam-era veteran status for draft-induced soldiers (Imbens and Angrist, 1994).[15] In the language of Angrist, Imbens, and Rubin (1996), these men can be said to be draft-lottery *compliers*. That is, they served in the Vietnam era because they were assigned a low lottery number, but would not have served otherwise.

Most soldiers from the Vietnam period were not compliers – rather, they were true volunteers in the

[15]In this case, LATE is an effect weighted across covariate cells (year, state, and month of birth).

sense that they were not drafted and they did not volunteer to avoid conscription.[16] The results reported here need not generalize to the population of true volunteers. Nevertheless, the effects of military service on those compelled to serve against their will are relevant for both an understanding of the historical consequences of conscription and for contemporary discussions of military manpower policy (since the Vietnam period provides the most recent evidence on the economic consequences of a draft). Moreover, given an economic mechanism for these effects, we might also draw broader conclusions as to how conscription affected soldiers, though these conclusions will naturally be more tentative and require stronger assumptions than an analysis of the causal effect of Vietnam-era conscription *per se*.

Draft-lottery estimates constructed using the 2000 Census show little evidence of an effect of Vietnam-era conscription on the labor market outcomes of whites. This can be seen in Panel A of Table 3, which reports estimates of effects on labor market status and earnings using different instrument sets. For example, 2SLS estimation using draft-eligibility status as an instrument in the sample of white men born 1950-52 generates an effect of -.0043 (s.e.=.0072) on employment and -517 (s.e.=1240) on earnings. The corresponding estimates in the sample of white men born 1948-52 are -.0047 (s.e.=.0072) and -115 (s.e.=1243). Estimates of effects on log weekly wages, computed for the sample of men with positive earnings, are similarly small. They are also precise enough that negative effects of less than 4 percent would turn up as significant (s.e.s on the order of .016). In contrast, OLS estimates, reported in the table in columns 2 and 7, show that veteran status is associated with substantially worse labor market outcomes and lower employment rates. The OLS estimates, -8,616 for annual earnings and -12 percent of weekly wages, are far outside the 2SLS confidence intervals.

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[16]The proportion draft-lottery compliers can be calculated as follows: let V_{1i} denote i's veteran status if i is draft eligible ($ELIG_i$=1) and V_{0i} denote i's veteran status if i is ineligible ($ELIG_i$=0). Random assignment makes $ELIG_i$ independent of $\{V_{1i},V_{0i}\}$. Observed veteran status is $V_i=V_{0i} + ELIG_i(V_{1i}-V_{0i})$ and compliers have $V_{1i}-V_{0i}=1$. Given monotonicity, i.e., $V_{1i}\geq V_{0i}$, the proportion of the study population who are draft-eligibility compliers is given by the draft-eligibility first stage, $P[V_{1i}-V_{0i}=1]=E[V_{1i}-V_{0i}] =E[V_i|\, ELIG_i=1]-E[V_i|\, ELIG_i=0]$. The proportion of veterans who are draft-eligibility compliers is $P[V_{1i}-V_{0i}|\, V_i=1] = P[V_i=1|\, V_{1i}-V_{0i}=1]\{P[V_{1i}-V_{0i}=1]/P[V_i=1]\} = P[ELIG_i=1]\{P[V_{1i}-V_{0i}=1]/P[V_i=1]\}$. For white men born 1950-52, this calculation is .376*(.145/.236)=.231.

The pattern of OLS estimates is reversed for nonwhites, with nonwhite veterans estimated to be more likely to be working and earning more than non-veterans. The 2SLS estimates in Panel B of Table 3 also offer no evidence of a negative causal effect of Vietnam-era service on employment or earnings – the 2SLS estimates in this case are positive. It should be noted, however, that the estimates for nonwhites are considerably less precise than those for whites due both to the smaller sample of nonwhites and the weaker first-stage relation between lottery numbers and veteran status in the nonwhite sample. Using draft eligibility as an instrument, the estimated effect of Vietnam-era service on the log weekly wages of nonwhites born 1950-52 is -.037 with a standard error of .067. The earnings effects are positive but mostly insignificant. Some of the estimated effects on weeks and hours worked by nonwhites are positive and significantly different from zero. There is also some evidence of reduced unemployment. On the other hand, there is no corresponding increase in employment and the effects on a dummy indicating positive earnings are not significantly different from zero. The size and significance level of these results is also reduced in the sample of men born 1948-52. On balance, the results for nonwhites seem inconclusive, but perhaps leaning towards positive long-run effects.

2SLS estimates constructed with larger instrument sets differ little from those using draft-eligibility alone. There is also little gain in precision from expanding the instrument set. Over-identified estimates computed using the *5z* instrument set are typically slightly less precise than the just-identified draft-eligibility estimates. This reflects the fact that while the *5z* instrument set allows a more complicated relation between lottery numbers and the probability of service, this relation is restricted to be the same for all cohorts. The draft-eligibility instrument appears to do a good job of capturing year-of-birth variation in the first stage as well as variation across lottery number groups. The most precise estimates in Table 3 use the *5zx* instrument set, i.e., a full set of lottery number and year of birth interactions. But the resulting efficiency gains are not dramatic – for example, the standard error for the effect on log weekly wages in the 1948-52 sample of whites falls from .016 to .015, with similar coefficient estimates.

The 2SLS estimates in Table 3 offer no evidence of a lasting impact of Vietnam-era service on the

earnings or employment of whites. This is a change from the findings for whites in Angrist (1990), which indicate substantial earnings losses for Vietnam veterans born 1950-52. The estimated earnings losses in the earlier study range from 10-15 percent of average social-security-taxable earnings in 1981-84, with somewhat larger losses in earlier years.

Experience and Earnings

The results from the 2000 Census can be reconciled with results for the earlier period if the economic consequences of conscription are generated primarily by lost labor market experience. Although veterans were out of the civilian labor market for about two years, by 2000 the draft lottery cohorts had reached middle age, when experience profiles are typically fairly flat. In fact, estimates of the experience profile reported in Angrist (1990) suggest the loss of earnings to Vietnam-era compliers declines linearly with age, eventually reaching zero at around age 50 (though this was an out-of-sample prediction).

The predicted veteran earnings loss generated by lost experience can be derived from a Mincer-style wage equation. As in Angrist (1990), suppose that in the absence of military service, individual i's earnings evolve according to

$$Y_i = \beta_0 + \beta_1 X_i + \beta_2 X_i^2 + \rho S_i + \upsilon_i \tag{2}$$

where Y_i is the log weekly wage, S_i is years of schooling, X_i is potential work experience, and υ_i is a random component in the human capital earnings function. Potential work experience is assumed to vary with veteran status according to

$$X_i = A_i - S_i - 6 - V_i\ell = \bar{X}_i - V_i\ell, \tag{3}$$

where A_i is age and $\bar{X}_i$ is potential experience for non-veterans. In other words, veterans have ℓ fewer years of experience than non-veterans of the same age and educational attainment.

Equations (2) and (3) can be re-arranged to produce a wage equation with an additive veteran effect and an interaction term between V_i and $\bar{X}_i$. This reduced-form equation is

$$Y_i = \pi_0 + \beta_1 \bar{X}_i + \beta_2 \bar{X}_i^2 + \rho S_i + (\pi_1 + \pi_2 \bar{X}_i) V_i + \upsilon_i \tag{4}$$

where

$$\pi_1 = -[\beta_1 \ell - \beta_2 \ell^2] \tag{5a}$$

$$\pi_2 = -2\beta_2 \ell. \tag{5b}$$

Since β_1 and β_2 are identified by the regression terms in $\bar{x}_i$ and $\bar{x}_i^2$, the loss of experience parameter, ℓ, is over-identified by (5a,b).

An unrestricted model can be obtained by allowing the linear potential experience term to vary with veteran status according to $\beta_{1i} = \beta_{10} + \beta_{11} V_i$. The relationship between the reduced form and human capital earnings function then becomes

$$\pi_1 = -[(\beta_{10}+\beta_{11})\ell - \beta_2 \ell^2] \tag{6a}$$

$$\pi_2 = -[2\beta_2 \ell - \beta_{11}]. \tag{6b}$$

Since β_{11} is an additional free parameter, (6a,b) imposes no restrictions on the reduced form.

Using data on the 1976-1984 earnings of men born 1950-52, Angrist (1990) estimated ℓ to be 2.08 (s.e.=.38) using (5) and 1.84 using (6). The unrestricted reduced-form veteran effect is $\pi_x = -.189 + .006\bar{x}$. In other words, veterans start out at a −.189 wage disadvantage but the gap closes by .006 each year, reaching zero when $\bar{x} = .189/.006 = 31.5$, or $A_i \approx 50$ for those with a high school diploma. The restricted reduced form is estimated to be $\pi_x = -.225 + .011\bar{x}$, with an over-taking age of about 38 for high school graduates (but older for those with more education).

A key implication of the pure loss-of-experience model of veteran wage effects is that by the time of the 2000 Census, the veteran earnings gap should have largely closed and perhaps even reversed. This is illustrated in Figure 2, which plots the profiles estimated from equation (4) assuming veteran status operates to reduce experience as in (3). The upper panel of the figure, which is based on the unrestricted model allowing veterans to have a profile with lower slope, shows the veteran/non-veteran profiles converging by about 2000, while the restricted model has veterans converging sooner and earning more by 2000. In both

models, however, the broad picture is of a shrinking veteran wage penalty.[17] Moreover, as we show in the next section, a second force that appears to have contributed to the closing veteran wage gap is schooling.

IV. Effects on Schooling

Compulsory military service appears to have increased the educational attainment of Vietnam-era veterans. This is documented in Table 4a, which reports 2SLS estimates of effects on (imputed) highest grade completed and on the likelihood of attaining the education credentials identified in the Census. For example, the 2SLS estimates show white veterans with .33-.35 more years of schooling in the 1950-52 sample. These effects are slightly lower in the 1948-52 sample, but change little when estimated with an expanded instrument list. Both samples generate precise estimates, with standard errors on the order of .05. In contrast with the results for whites, however, the estimates for nonwhites are smaller, mostly below .2, and not significantly different from zero.

The remainder of Table 4a shows that the increase in highest grade completed for white veterans comes about primarily through an increase in the likelihood veterans attended some college (including partial years) or obtained an associate's degree. These effects are on the order of .08. The increase in the likelihood of completing a BA is smaller, at around .05. Perhaps surprisingly, there is also a small effect on high school completion (roughly 2 percentage points) and a very small effect on upper secondary grade completion. These effects may be due to GEDs obtained by veterans without a high school diploma. In addition, since the 1990s, many states have offered Vietnam-era veterans honorary high school diplomas solely on the basis of their military service. It's also worth noting that, as in the descriptive statistics in Table 2, OLS estimates with controls for state, month, and year of birth (reported in column 2) show white veterans with less education than non-veterans overall, and fewer with each credential beyond high school.

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[17]Imbens and van der Klaauw (1995) report an estimated earnings loss of about 5% for Dutch conscripts ten years after their service. This is consistent with the earnings penalty that might be expected from lost experience given their shorter period of service.

The 2SLS estimates are summarized in Figures 3a and 3b, which plot the coefficient estimates reported in Table 4a, along with pointwise confidence bands. There is a clear jump in the effects on post-high school schooling levels, with a marked drop at the BA level. The figure also highlights the much smaller and mostly insignificant shift in the schooling CDF for nonwhite veterans. The single (marginally) significant effect on nonwhites' education is for the likelihood of obtaining some college but less than a complete year.

Veterans Benefits vs. Draft Deferments

For a number of reasons, we believe the most likely explanation for the schooling shifts documented in Table 4a and Figure 3 is the educational subsidies and stipends available through the Vietnam-era GI Bill. In this case, increased schooling for veterans is a downstream consequence of military service. The leading alternative explanation attributes the estimated schooling effects to *pre-service* efforts by draft-eligible men to avoid military service. In this scenario, education represents a distinct channel whereby draft-eligibility affects outcomes such as earnings, in additional to veteran status *per se*.

The case for a service-only explanation of increased schooling begins with the observation that Vietnam-era veterans, who received GI Bill stipends similar in generosity to those available to WWII and Korea veterans, were especially likely to have used these benefits for education and training. Data from the 2001 Survey of Veterans (SOV) show that among whites, 44 and 42 percent of WWII and Koran-era veterans used benefits for education and training, while the usage rate is 50 percent for Vietnam-era veterans. Vietnam-era veterans are also more likely than earlier cohorts to have used their benefits for college course work. In particular, 63 percent of Vietnam-era benefit users used their benefits for college courses, while the corresponding figures for WWII and Korea benefit-users are 53 and 56 percent.[18] The notion that military service increased schooling through education benefits is also supported by earlier research on the GI Bill.

[18]The pattern for nonwhite veterans is similar, though the levels are lower. Statistics in this paragraph are from the authors' tabulation of responses to the 2001 SOV. For this purpose, samples of veterans were limited to the principle birth cohorts who served in each era (years of birth with at least 100 observations in the SOV).

For example, Bound and Turner (2002) and Stanley (2003) argue that the WWII and Korea GI Bills led to modest increases in educational attainment for affected cohorts. Lemieux and Card (2001) find similar education effects arising as a consequence of the availability of the Canadian GI Bill, while Angrist (1993) finds a large post-service schooling increase associated with the use of the Vietnam-era GI Bill.

The schooling effects estimated using draft-lottery instruments might also be explained by draft-avoidance behavior through education-related draft deferments. In the 1960s, college students could delay and eventually escape conscription by staying in school. Men with low draft lottery numbers may therefore have been more likely to stay in college or to enroll in college, hoping to avoid service through an educational deferment. Weighing against this possibility is the fact that President Nixon announced a plan to end educational deferments in April 1970, and in 1971 new deferments indeed ended and existing deferments were phased out.[19] Thus, the importance of educational deferments declined sharply during the draft-lottery period. This fact is reflected in the cohort- and sex-specific enrollment rates analyzed by Card and Lemieux (2001). Figure 2 in their paper suggests there was no draft-related increase in the college graduation rate or in the proportion with some college for men born 1950 or later, and only a modest increase in the college enrollment rate for men born in 1950, with no impact on enrollment thereafter.[20]

Additional Evidence on the GI Bill Hypothesis

We present two additional pieces of evidence on the interpretation of Table 4a. The first is a set of estimates by single year of birth, reported in Table 4b. This table shows that in spite of the fact that the

[19]In April 1970, Nixon issued an executive order barring occupational, agricultural, and paternity deferments. In January 1971, Nixon called again for the elimination of student deferments, which required an act of Congress, passed in September of that year. Existing deferments were then retained for one semester only (or , for seniors, until graduation). A historical chronology of selective service system provisions appears in Selective Service System Office of Public Affairs (1986). See also the Semiannual Reports of the Director of the Selective Service System from the early 1970s.

[20]Angrist and Krueger (1992) look at the relation between lottery numbers and education using data from the 1979-85 CPS's. These results are insignificant but too imprecise to detect effects on schooling of the size reported here. Moreover, as we argue below, some of the veteran schooling advantage seems likely to have accumulated during and after the Angrist and Krueger (1992) sample period.

availability and value of college deferments was declining from 1970 onwards, the estimated effects on highest grade completed are substantial for men born in 1951 and 1952. The largest effects of military service on highest grade completed are for the 1951 cohort, few of whom would have been deferred for long. Estimates on highest grade completed for the 1952 cohort (which had no access to education deferments) are somewhat smaller, but the impact on highest grade completed for men born in 1952 is still significant and differs little from the estimated effect on highest grade completed for men born in 1948.

Cohort differences in the estimated effects on some-college dummies are somewhat larger than the differences in estimates of effects on highest grade completed. For example, the estimated effect on one or more years of college falls from .083 for the 1951 cohort to .053 for the 1952 cohort. This is consistent with a role for deferments. On the other hand, similar to results for highest grade completed, the effects on both associate's degrees and BA's peak for men born in 1951. It's also worth noting that the estimates by single year of birth for nonwhites are typically as large or larger in the 1952 cohort than in other years. On balance, therefore, we read Table 4b as pointing away from draft deferment as the primary explanation for the results in Table 4a.

Our second tack is to present independent evidence in favor of increased post-service schooling for Vietnam veterans. We do this using a model that divides total educational attainment into three parts: pre-service schooling, including early educational attainment for non-veterans; the change in schooling at ages when those who were drafted would have been in the military; and the difference between post-discharge schooling and completed education. For veterans, this last is the schooling increment funded by the GI Bill.

To formalize, let e_i denote completed education and write

$$e_i = e_i^0 + \Delta e_i^1 + \Delta e_i^2, \tag{7}$$

where is e_i^0 pre-service education for veterans and schooling completed as of the typical entry age for nonveterans, Δe_i^1 is the schooling increment between the typical entry and discharge ages (defined for both veterans and non-veterans) and Δe_i^2 is the difference between completed schooling and the schooling completed at the typical discharge age (again, defined for both veterans and non-veterans). For concreteness,

we think of e_i^0 as schooling at age 20, $e_i^0 + \Delta e_i^1$ as schooling at age 24, and e_i as schooling at around age 40, when GI Bill eligibility expired for the cohorts studied here.

Although almost certainly correlated with veteran status, entry-level schooling is not causally related to veteran status. The causal effect of schooling is a result of differences in both the service-period and post-discharge period schooling increments, Δe_i^1 and Δe_i^2. To make this explicit, let $\Delta e_i^1(v)$ denote the potential schooling increment that an individual would obtain during the service period, indexed by veteran status v=0,1. Similarly, let $\Delta e_i^2(v)$ denote the potential schooling increment in the post-service period. Both $\Delta e_i^j(0)$ and $\Delta e_i^j(1)$ are defined for all i and for j=1, 2, regardless of i's realized veteran status.

For most soldiers $\Delta e_i^1(1)$ is zero: they get no schooling in the military. Therefore,

$$\Delta e_i^1 = \Delta e_i^1(0)[1 - v_i].$$

We also assume that non-veterans complete their education by the time most veterans are discharged (or shortly thereafter), so that $\Delta e_i^2(0)=0$. Therefore,

$$\Delta e_i^2 = \Delta e_i^2(1)v_i.$$

Combining these two assumptions with the decomposition in (7) shows that the causal effect of veteran status on individual veterans is $\Delta e_i^2(1) - \Delta e_i^1(0)$. In other words, the causal effect of veteran status on schooling is the schooling gained by veterans after discharge minus the schooling they would have obtained during the years they were in the military had they not served. The average causal effect of military service on veterans' schooling is

$$E[\Delta e_i^2(1) - \Delta e_i^1(0) | v_i=1] = E[\Delta e_i^2(1) | v_i=1] - E[\Delta e_i^1(0) | v_i=1] \quad (8)$$

The question of whether veterans ultimately come out ahead boils down to whether post-discharge increases in schooling are enough to overcome the education lost while serving.

We attempted to measure $E[\Delta e_i^2(1) - \Delta e_i^1(0) | v_i=1]$ directly using repeated cross-sections from the Current Population Surveys (CPS) from 1964-1991. This covers the time from the beginning of the Vietnam era to just beyond the expiration of Vietnam-era GI Bill entitlements in 1989. We compare the change in highest grade completed over this period by veteran status. Under the assumptions given here, the difference-

in-differences in average education by veteran status is

$$E[e_i - e_i^0 | v_i=1] - E[e_i - e_i^0 | v_i=0] = E[\Delta e_i^2(1) | v_i=1] - E[\Delta e_i^1(0) | v_i=0]. \quad (9)$$

Assuming $E[\Delta e_i^1(0) | v_i=0]=E[\Delta e_i^1(0) | v_i=1]$, i.e., that the schooling veterans would have obtained during their service years had they not served is equal to the schooling non-veterans obtained at the same ages, equation (9) is the causal effect of veteran status on schooling, as expressed in (8). In practice, however, it seems likely that the schooling veterans lost while in the military is less than the schooling non-veterans obtained during the service years, in which case the empirical counterpart of (9) is an underestimate of (8).

Consistent with this reasoning, CPS data show that the educational attainment of Vietnam veterans born 1948-52 increased relatively little when these men were in their early twenties, while the schooling of nonveterans the same age was rising sharply. On the other hand, the age-schooling profile of nonveterans flattens out relatively early, while the schooling of Vietnam veterans continued to increase when these men were in their thirties. This can be seen in Figure 4, which plots educational attainment by age and veteran status. A drawback of the CPS for our purposes is that most active duty soldiers are not in the sampling frame. The figure therefore shows two panels, constructed with and without those active duty soldiers who respond to the survey (this includes soldiers in the US, living off-base or with their families). The schooling profiles in both versions exhibit a similar pattern.[21]

As a comparison, Figure 4 also shows the schooling profiles for veterans and non-veterans from Korea cohorts. These are similar to the Vietnam-era profiles at the same ages. Interestingly, however, while Vietnam veterans had less schooling than discharged Korea veterans around the time they left the military, Vietnam veterans end up with more education than Korea veterans and largely close the gap with non-veterans the same age.

The empirical counterpart of equation (9) appears in Figure 5. Panel A plots the difference in average education by veteran status at each age, using either a two-year or three-year moving average. The figure

[21]A detailed description of the data and methods used to construct Figure 4 and 5 appears in the appendix.

highlights the rapidly increasing then shrinking veteran/non-veteran schooling differential.[22] Panel B plots the difference in the moving average of schooling by veteran status relative to the base period (i.e., the average over the first two or three years of age in Panel A). This plot shows that – relative to their starting point – veterans indeed eventually overtake non-veterans. The estimates using equation (9) and data from the end of the period (ages 35-39) are on the order of .1-.3 depending on the moving average window and the width of the terminal group. The latter estimate is significantly different from zero with standard error of about .08. Thus, the analysis of schooling trends in the CPS also points to post-discharge schooling gains as a likely explanation for 2SLS estimates of effects on schooling.

V. Other outcomes

Experience and schooling need not be the only channels whereby military service affected soldiers' earnings. An alternative channel is health, as suggested by Hearst, Newman and Hulley's (1986) pioneering study, which found elevated civilian mortality risk among draft-eligible men. As noted in Section II, however, we found no evidence that draft-eligible men are disproportionately missing in the 2000 Census, as might be expected if Vietnam veterans suffered excess mortality. Angrist, Imbens and Rubin (1996), using the same data as Hearst, Newman, and Hulley (1986), likewise found no significant mortality effects.[23] Other, later studies using the draft lottery also report less evidence of adverse health consequences than Hearst, Newman, and Hulley (1986).[24]

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[22]The sample used here includes active-duty CPS respondents. The absence of most active-duty soldiers probably tends to bias the average schooling for veterans upwards at young ages since some of those counted as veterans may have returned to school while active-duty soldiers have not yet had the chance to do so.

[23]This is partly due to a difference in methods. Angrist, Imbens, and Rubin (1996) compute IV (Wald) estimates of the effects of military service on suicide and total mortality by single year of birth. Hearst, Newman, and Hulley (1986) report reduced-form odds ratios, with insignificant results for overall mortality by single year of birth but a significantly higher pooled odds ratio estimate for suicide.

[24]Goldberg, Richards, Anderson, and Rodin (1991) found no evidence of increased alcohol consumption among draft-eligible men. Dobkin and Shabani (2006), using draft-lottery instruments, conclude that there is no clear evidence for effects of Vietnam-era service on a range of health outcomes. Hearst, Buehler, Newman and Rutherford (1991), using draft-lottery instruments, found no increase in AIDS among Vietnam-era veterans (linking this to intravenous drug

The mixed evidence for health effects from Vietnam-era service notwithstanding, the possibility that military service affected health is a clear concern in principle. Veterans may have been injured in combat, either physically or as a result of post-traumatic stress disorder (PTSD). Many Vietnam veterans have also been concerned about exposure to the Agent Orange defoliant used by American forces. Finally, the loss of earnings associated with Vietnam-era conscription may itself have been debilitating. We therefore look directly at disability outcomes in the Census. The Census disability variables are those detailed in the discussion of Table 2: self-reported disability status and disability-related income.

The 2SLS estimates of effects on disability outcomes, reported in Table 5a for whites and 5b for nonwhites, suggest that Vietnam-era conscription increased self-reported disability rates. The estimated effect on the likelihood of reporting a non-work disability in the white sample is .017 in the 1950-52 sample and .013 in the 1948-52 sample. Both of these effects are significantly different from zero, with standard errors on the order of .005. The average non-work disability rate is about .07, so the estimates imply a marked increase. Among the individual categories, the largest effects are on mobility-related disabilities. On the other hand, the 2SLS estimates show no evidence of an impact on work-related disability rates, a result that can be seen in the first row of Table 5a. There is also less evidence of an effect on disability for nonwhites, with (marginally) significant estimates for one outcome only (vision and hearing). Given the large estimates and small average disability rates in this category, this could be a chance finding.

The 2SLS estimates also suggest that compulsory military service increased the likelihood and amount of disability-related income. The estimates for whites range from about $200-300 for the category that includes VDC, with a .04 increase in the likelihood of receipt, to $400-600 for the category that includes MDP, with a .03 increase in the likelihood of receipt. All of these estimates are reasonably precise. In

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use). Bedard and Deschenes (2006) make a good case that WWII service increased smoking and smoking-related disease, probably because WWII veterans were given free cigarettes. This was not true in the Vietnam era, however. Eisenberg and Rowe (2007), using draft-lottery instruments, find some evidence of increased smoking in the immediate post-Vietnam period, but these effects, which are not very precisely estimated, disappear in later data (including effects on a retrospective ever-smoked outcome). They also find no evidence of effects on other health outcomes.

contrast, the estimates for nonwhites are more mixed, with (marginally) significant increases in disability-related income found almost exclusively in the sample born 1948-52. The difference in significance levels for whites and nonwhites in the 1950-52 sample is more a problem of precision than effect size. Not surprisingly, given veterans' age and work history, there is no effect of military service on Social Security income or income from the SSI program.

The increased receipt of disability-related income reported in Table 5a is to some extent a mechanical phenomenon since only veterans are eligible for VDC and MDP. On the other hand, the higher disability rates reported in Table 5a may reflect a negative causal impact on health for white veterans. Weighing against this is the absence of an effect on work-related disability, a surprising result in this context since work-related disability rates are higher than non-work related disability rates. A second finding inconsistent with a long-term health impact is the absence of an effect on employment outcomes: the disability increase reported in Table 5a for whites does not appear to translate into lower employment rates, higher unemployment, or reduced hours and weeks worked, as would usually be expected for disabled workers (see Table 3).

Given these inconsistencies, it may be that impact on disability rates for veterans reflects, at least in part, the financial incentives in the veterans' compensation system. As noted by Autor and Duggan (2007), VDC is not taxed to offset earnings (though MDPs are means-tested). Veterans therefore have a strong incentive to claim benefits. Much of the growth in VDC claims since the late 1990s has been from Vietnam veterans. While rapid growth in PTSD claims by Vietnam veterans since September 11, 2001 has been widely noted, PTSD claims by Vietnam veterans also grew 18 percent from 1999-2001.[25] A recent VA study investigates the growth in VDC claims from 1999-2004 and the large variation in these claims across states. Among other things, the success and amount awarded to VDC claimants appears to be linked to the likelihood

[25]These data are from Veterans Benefits Administration (2000 and 2002). Three-quarters of all PTSD claims in 2001 were by Vietnam veterans. PTSD is the second-largest category of Vietnam-era claims in 2001 (after scars). VDC recipients can (and usually do) claim multiple disabilities.

that claimants used power-of-attorney representatives from veterans service organizations (VA, 2005).[26] Along these same lines, Duggan, Rosenheck and Singleton (2007) show that enrollment in the VDC program appears to be highly sensitive to small changes in eligibility criteria and to the unemployment rate. Claims for civilian Social Security Disability Insurance have similarly been linked to the labor market (Autor and Duggan, 2003).

To get an independent look at trends in veterans health and disability income, we tabulated the amount and proportion receiving income from the VA (including VDC) and the amount and proportion receiving non-SSA disability income (including MDP) using data from the CPS. The results are plotted by year and veteran status for Vietnam and Korea cohorts in Figures 6 and 7. Few non-veterans have income in either the VA or disability categories while both Korea and Vietnam-era veterans have substantial amounts. Importantly, the proportion of Vietnam-era veterans with VA and disability income appears to jump in the late 1990s, especially relative to the flatter or declining trend for Korea veterans.

Direct measures of self-reported disability rates and the likelihood of fair-to-poor health, plotted in Figure 8, also show a jump for Vietnam veterans around the same time that disability income went up. This might signal a deterioration in the health of Vietnam veterans, but the suddenness of the recent increase in both disability rates and the receipt of disability income suggests that policy or regulatory changes may play a role. It should also be noted that the causality between self-reported disability and disability income can run in both directions: a deterioration in cohort health may have increased claims, but increases in VDC awards may also increase the likelihood veterans categorize themselves as disabled or as suffering from poor health in government surveys (in fact, the disability question in the CPS is a screener for questions about disability income). On balance, therefore, it seems fair to interpret the data on veterans health as inconclusive.

[26]Beginning in 2002, diabetes claims for Vietnam veterans jumped sharply after a 2001 VA ruling allowing diabetes to be treated as service-related (based on evidence linking diabetes and Agent Orange; see Autor and Duggan, 2007). This ruling is unlikely to affect disability outcomes in the 2000 Census.

VI. Schooling, Experience, and Earnings

As noted in Section III, the lack of an effect of military service on wages can be reconciled with earlier findings indicating a substantial wage loss by a flattening of the experience profile in middle age. At the same time, the estimates in Section IV show a marked increase in the educational attainment of Vietnam-era veterans. Increased schooling may also have offset earnings losses. Here, we bring these pieces together in an attempt to see whether the overall effect of military service on wages can be accounted for by the combined impact of lost labor-market experience and increased schooling. We are especially interested in the economic return to the schooling veterans got as a consequence of their service since this schooling increment is heavily subsidized. In view of the non-work-related disability effects reported in the previous section, we also explore models that allow for higher disability rates among Vietnam-era veterans.

The empirical framework in this section is essentially the same as outlined in equations (2) and (3), with the addition of exogenous covariates (month of birth and state of birth). The channels by which veteran status is assumed to affect wages are potential experience, x_i, and schooling, s_i. The loss of experience associated with veteran status is fixed at 2 years, as estimated in Angrist (1990) and consistent with the terms of service for draftees. The equation of interest has three endogenous variables, x_i, x_i^2, and s_i. All age effects are assumed to be captured by the potential-experience quadratic so that age or year of birth is available as an instrument. Veteran status influences wages by reducing potential experience and by increasing schooling. Draft-eligibility therefore functions as an instrument for both schooling and potential experience. Likewise, there are cohort as well as draft-lottery effects in schooling.

Estimates of equation (2) are reported in Table 6 for the sample of white men born 1948-52. We focus on whites because the schooling effects for nonwhites are much smaller. The 1948-52 sample is more useful than the 1950-52 sample in this context because the wider age range helps to pin down the experience profile. As a benchmark, column (1) reports OLS estimates treating all variables as exogenous. Model I defines potential experience as in equation (3), adjusting for time in school. Model II replaces individual schooling by cohort-average schooling. The returns to schooling are on the order of .12 when potential

experience is adjusted for time in school. The estimated experience profile in this case does not have the usual concavity, but replacing individual schooling by cohort-average schooling fixes this. In both models, the experience derivative is small, about .01 (s.e.=.0006). The reduced-form veteran effect, constructed from equations (5a) and 5b), gives the predicted veteran earnings loss due to lost experience. This is –.016 (s.e.=.011) when potential experience is adjusted for schooling and –.023 (s.e.=.0012) when potential experience is adjusted for average schooling by year of birth.

The instrumental variables estimates of the return to schooling – which we interpret as the return to the extra schooling caused by veteran status – are considerably smaller than the OLS estimates. This can be seen in columns 2-8 of Table 6, which reports 2SLS and LIML estimates of equation (2). In over-identified models, LIML provides a check for bias in 2SLS. Estimates from a just-identified model using age, age^2 and draft-eligibility as instruments for the three endogenous variables x_i, x_i^2, and s_i (reported in column 2) generate a return of .07 (s.e.= .035) or .064 (s.e.=.036) depending on whether potential experience is taken to be a function of individual schooling. Swapping year-of-birth dummies for age and age^2 changes the 2SLS estimates little, as shown in column 3. The resulting estimates are .073-.078.[27]

The F-statistic for all excluded instruments in column 3 is large (93.3) though this is misleading since age and year of birth serve as instruments for potential experience more than for schooling. The F-statistic for RSN instruments only (in this case, a single draft-eligibility dummy) is 39.4 in column 3, roughly the square of the t-statistic for the just-identified schooling model in Table 4a. Most relevant is the F-statistic that explicitly takes account of multiple endogenous variables in the 2SLS calculation. This statistic, which adjusts for the fact that all the instruments predict all the endogenous variables, is smaller still (15 in column 3), but still outside the range where bias in 2SLS estimates is usually a concern.[28] The LIML estimates in

[27]Estimates using linear potential-experience controls, not reported to save space, are almost identical to those reported in Table 6 (linear experience terms are close to the average experience derivatives in the quadratic model.)

[28]The multivariate first-stage F is constructed as follows. Assume covariates have been partialled out of the instrument list and that there are two endogenous variables, W_1 and W_2 with coefficients δ_1 and δ_2. We are interested in the bias of the 2SLS estimator of δ_2 when W_1 is also treated as endogenous. In matrix notation, the instrument vector

column 4 are close to the corresponding 2SLS estimates, as expected since the degree of over-identification for the model reported in column 3 is two.

In an attempt to increase the precision of the estimated schooling coefficients, we used the *5zx* and *7zx* instrument sets constructed from 5 and 7 RSN dummies interacted with year of birth. As before, the *5zx* instrument set in the 1948-52 sample includes 25 RSN instruments interacted with year-of-birth dummies. In addition, the instrument list includes 4 year-of-birth dummies. The *7zx* instrument set includes 35 RSN instruments plus 4 year-of-birth dummies. The resulting 2SLS estimates of the returns to schooling, reported in columns 5 and 7 of Table 6, are indeed more precise than the estimates in columns 2 and 3, though they are also smaller. For example, the estimated returns to schooling in model I using the *5zx* and *7zx* instrument sets are .043 (s.e.=.03) and .051 (s.e.=.028). The multivariate first-stage F-statistics for both of these models are low, about 3.6 in column 5 and 2.9 in column 7. This reflects the fact that many of the interaction terms included in the expanded instrument lists have little predictive power. Also of concern in the heavily over-identified specifications is that fact that the LIML estimates fall to zero with standard errors almost twice as large as those for the corresponding 2SLS estimates. On balance, therefore, the low degree-of-overidentification estimates in columns 2 and 3 appear more reliable.[29]

Finally, we report a set of estimates that allows for the possibility that Vietnam-era service increased

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is Z, with projection matrix $P_z=Z(Z'Z)^{-1}Z'$. The second stage equation is

$$Y = P_zW_1\,\delta_1 + P_zW_2\,\delta_2 + [\epsilon + (W_1-P_zW_1)\,\delta_1 + (W_2-P_zW_2)\,\delta_2]$$

where ϵ is the vector of structural errors. The 2SLS estimator of δ_2 can be seen to be the OLS regression on $P_z[M_{1z}W_2]$, where $M_{1z}=[I-P_zW_1(W_1'P_z\,W_1)^{-1}\,W_1'P_z]$. This is also 2SLS using P_z to instrument $M_{1z}W_2$. In other words, the endogenous variable of interest is $M_{1z}W_2$, itself the residual from a 2SLS regression of W_2 on W_1. Note that the 2SLS estimator of δ_2 can be written

$$\delta_2 + [W_2'M_{1z}P_zM_{1z}W_2]^{-1}\,W_2'M_{1z}P_z\,\epsilon.$$

The explained sum of squares (numerator of the F-statistic) that determines bias is therefore the expectation of $[W_2'M_{1z}P_zM_{1z}W_2]$, as can be shown formally using the group-asymptotic sequence in Bekker (1994) and Angrist and Krueger (1995).

[29]The standard errors reported for both the LIML and 2SLS estimates in Table 6 are heteroscedasticity-consistent. LIML is motivated by a homoscedastic normal model but can be understood as a k-class estimator in either case. On the other hand, the collapse of LIML towards zero in columns 5 and 7 is something of a puzzle. This may be explained by the group-asymptotic inconsistency of LIML under some forms of heteroscedasticity. Hausman, Newey, and Woutersen (2006) report a heteroscedastic simulation in a many-weak IV scenario where the bias of LIML is away from OLS.

disability rates. The model of interest in this case is

$$Y_i = \beta_{0\gamma} + \beta_{1\gamma}X_i + \beta_{2\gamma}X_i^2 + \rho_\gamma S_i + \gamma D_i + \upsilon_i, \quad (10)$$

where D_i indicates non-work disability status (the disability variable that is most affected by veteran status in Panel A of Table 5a), with coefficient, γ. In practice, we have too few instruments to learn much from an estimation strategy that adds D_i to the list of endogenous variables. We therefore get a sense of the consequences higher disability rates might have for the estimates of (10) by considering plausible values of γ, say γ^*, and estimating

$$Y_i^* \equiv Y_i - \gamma^* D_i = \beta_{0\gamma} + \beta_{1\gamma}X_i + \beta_{2\gamma}X_i^2 + \rho_\gamma S_i + \upsilon_i, \quad (11)$$

by 2SLS (or LIML) as before. As a benchmark, we set $\gamma^*=-.2$, slightly larger in magnitude than the OLS estimate of the wage loss associated with non-work disabilities, estimated using an equation like (10).

Adjusting for the relation between veteran status and disability status in this manner increases the 2SLS estimates of the returns to schooling by .005-.007. This can be seen in Panel B of Table 6, which reports the estimated returns to schooling and reduced-form veteran effects coming out of equation 8. Variations on these results for alternative choices of γ^* can be obtained by observing that $\hat{\rho}_\gamma$, the 2SLS estimate of the schooling coefficient in equation (7), is related to the 2SLS estimate of the schooling coefficient in equation (2), $\hat{\rho}$, by the formula

$$\hat{\rho}_\gamma = \hat{\rho} - \hat{\lambda}\gamma^*,$$

where $\hat{\lambda}$ is the 2SLS estimate of the coefficient on S_i in a regression of D_i on the variables on the right-hand side of (2) (again, treating all variables as endogenous). The coefficient $\hat{\lambda}$ in this adjustment is equal to about .03, so that the disability effects reported in Table 5a, even if related to veterans' earnings potential, do not necessitate a major change in the estimated returns to schooling, which remain low relative to OLS.

A reduced return to GI-Bill-subsidized schooling is not a universal finding. Lemieux and Card (2001), using the Canadian GI Bill as a source of exogenous variation, report IV estimates larger than the corresponding OLS estimates in a study of Canadian WWII cohorts. But attenuated returns to post-service schooling reported here are consistent with a number of earlier investigations of the returns to schooling for

Vietnam veterans. For example, Schwartz (1986) estimated the returns to schooling to be .025 lower for Vietnam veterans than comparably-aged nonveterans. Angrist (1993) reports a return to veterans' post-service schooling increment on the order of .043 using a 1987 sample of Vietnam and early-volunteer-era veterans. This return appears to be higher for those who used veterans benefits for education and training in traditional college programs, as was typically the case for Vietnam-era GI bill users. This may explain the somewhat higher returns to GI-bill schooling reported here.

VII. Summary and Conclusions

The adverse economic consequences of Vietnam-era service appeared to have faded by the time the draft-lottery cohort entered middle age. Moreover, on the plus side, 2000 census data show a strong positive connection between military service and schooling. In spite of the fact that many Vietnam veterans lost educational ground as well as labor-market experience while they were in the military, veterans ended up with more education than they otherwise would have obtained, primarily through increases in post-secondary schooling. Seen through the lens of a Mincer-style wage equation, the near-zero net veteran wage penalty can be explained as the combined result of lost experience and the economic return to additional schooling. We find mixed evidence of disability effects as well, but these effects did not translate into reduced employment, and do not matter much for the Mincer equation.

Finally, it is noteworthy that 2SLS estimates of the returns to post-service schooling are smaller than the corresponding OLS estimates. This may reflect the large subsidies for schooling available to veterans through the GI bill, a point first noted by Berger and Hirsch (1983). The opportunity cost – and hence the marginal return – to the schooling covered by veterans benefits may be low relative to the population average return to post-secondary education. The reduced-form earnings effect of GI-bill funded schooling is on the order of 2.5 percent in the 2000 data, the result of a .33 increase in highest grade completed times a .075 return. Although modest in absolute terms, these effects appear to differ little from the schooling and earnings consequences of earlier American GI bills.

Table 1. Descriptive statistics, by race and veteran status, men born 1950-52

	Whites			Nonwhites		
	All	Vietnam veteran	Non-veteran	All	Vietnam veteran	Non-veteran
	(1)	(2)	(3)	(4)	(5)	(6)
Draft eligibility (by RSN)	.376	.532	.327	.382	.482	.350
Veteran status (served in Vietnam Era)	.236	1	0	.244	1	0
Post-Vietnam service	.038	.064	.030	.068	.078	.065
Now in military	.0027	.0065	.0015	.0029	.0067	.0016
Now in school	.028	.031	.027	.046	.053	.043
Age	48.2	48.4	48.2	48.2	48.3	48.2
A. Labor market variables						
Employment	.861	.844	.866	.665	.702	.654
Unemployment	.027	.030	.026	.056	.053	.057
Not in labor force	.112	.126	.108	.279	.245	.290
Usual hours worked	41.5	40.7	41.7	32.8	34.3	32.3
Weeks worked	44.8	44.1	45.0	35.9	37.5	35.4
Wage and salary income	46406	39472	48553	27584	28505	27287
Log weekly earnings (positive values)	6.75	6.65	6.78	6.41	6.43	6.41
Self employment income (positive values)	5261	3123	5923	1709	1230	1863
B. Disability and disability income variables						
Work disability	.123	.136	.118	.210	.207	.211
Non-work disability	.070	.084	.066	.116	.120	.114
Mobility disability	.082	.103	.075	.132	.145	.128
Self-care disability	.021	.023	.021	.040	.036	.041
Physical disability	.051	.055	.050	.121	.111	.124
Mental disability	.044	.049	.042	.073	.072	.073
Vision or hearing disability	.036	.042	.034	.045	.045	.045
Other income (e.g.,VA, UI, child support, alimony; includes VDC)	392	754	279	566	1085	398
Other income >0	.054	.107	.037	.072	.134	.052
Retirement/Dis. income (Retirement, survivor, disability pensions; incl. MDP)	691	1445	458	848	1622	598
Retirement/Dis. income >0	.043	.091	.028	.065	.118	.047
Social Security income (incl. SSDI)	293	331	281	424	423	425
Supplemental Security income (SSI)	110	94.4	114	273	190	299
C. Education						
Imputed highest grade completed	13.8	13.3	13.9	12.6	13.0	12.4
7th or 8th grade +	.990	.996	.988	.973	.989	.968
9th grade +	.977	.988	.974	.948	.981	.938
10th grade +	.965	.978	.961	.923	.970	.908
11th grade +	.948	.962	.943	.882	.950	.860
12th grade (no diploma) +	.931	.949	.926	.832	.923	.802
High school graduate +	.910	.927	.904	.770	.881	.735
Some college (less than 1 year) +	.655	.616	.667	.468	.585	.431
1 or more years of college (no degree) +	.582	.519	.601	.400	.486	.372
Associate's degree +	.411	.313	.441	.226	.243	.221
Bachelor's degree +	.333	.204	.373	.160	.136	.168
Master's degree +	.135	.071	.155	.057	.042	.062
Professional degree +	.051	.017	.061	.018	.0094	.021
Doctorate	.016	.0056	.019	.0061	.0040	.0067
N	696530	166652	529878	96217	23246	72971

Note: Sample weights are used in all estimates and statistics.

Table 2. First-stage, by race and year of birth

	Pooled cohorts		By single year of birth					
	1950-52	1948-52	1948	1949	1950	1951	1952	1953
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			A. Whites					
Draft-eligibility effect	.145	.112	.058	.074	.133	.138	.168	.031
	(.0013)	(.0010)	(.0010)	(.0025)	(.0024)	(.0023)	(.0022)	(.0024)
RSN effects (5z):								
RSN 1-95	.160	.128	.065	.088	.154	.155	.173	.032
	(.0015)	(.0013)	(.0031)	(.0031)	(.0029)	(.0026)	(.0026)	(.0022)
RSN 96-125	.091	.082	.060	.077	.131	.128	.023	.0002
	(.0023)	(.0019)	(.0047)	(.0046)	(.0044)	(.0040)	(.0034)	(.0031)
RSN 126-160	.059	.058	.054	.061	.126	.050	.0084	.00002
	(.0020)	(.0017)	(.0045)	(.0043)	(.0041)	(.0036)	(.0031)	(.0029)
RSN 161-195	.040	.044	.044	.054	.102	.024	-.0013	.0017
	(.0020)	(.0017)	(.0044)	(.0043)	(.0041)	(.0034)	(.0030)	(.0029)
RSN 196-230	.0065	.0059	.0043	.0062	.013	-.0012	.0077	.0008
	(.0019)	(.0017)	(.0043)	(.0042)	(.0038)	(.0032)	(.0031)	(.0029)
F-statistics	2403	2294	111	202	731	861	1028	50.3
			B. Nonwhites					
Draft-eligibility effect	.094	.072	.031	.049	.090	.096	.096	.027
	(.0034)	(.0028)	(.0069)	(.0065)	(.0059)	(.0060)	(.0063)	(.0058)
RSN effects (5z):								
RSN 1-95	.100	.081	.039	.059	.101	.101	.099	.029
	(.0041)	(.0034)	(.0086)	(.0081)	(.0074)	(.0072)	(.0070)	(.0064)
RSN 96-125	.062	.058	.027	.072	.089	.090	.016	.0043
	(.0061)	(.0050)	(.013)	(.012)	(.011)	(.011)	(.0095)	(.0093)
RSN 126-160	.044	.041	.027	.042	.093	.034	.0052	.0018
	(.0057)	(.0047)	(.012)	(.012)	(.011)	(.010)	(.0092)	(.0086)
RSN 161-195	.022	.021	.012	.027	.066	-.0047	.0055	.0023
	(.0055)	(.0046)	(.012)	(.011)	(.010)	.0092	(.0092)	(.0087)
RSN 196-230	-.0031	.0007	-.004	.018	.008	-.010	-.0055	.0021
	(.0054)	(.0046)	(.012)	(.011)	(.010)	.0093	(.0088)	(.0090)
F-statistics	138	134	4.98	14.3	48.9	55.1	47.3	4.51

Note: Draft-eligibility effects and RSN group effects are from separate regressions. Robust standard errors in parentheses. All models include a full set of dummies for years of birth, states of birth, and month of birth. Sampling weights are used in all estimates and statistics.

Table 3. Effects on labor market outcomes

	1950-52					1948-52				
			2SLS					2SLS		
	Mean	OLS	elig	5z	5zx	Mean	OLS	elig	5z	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
				A. Whites						
Work variables in 1999										
Employment	.861	-.020	-.0043	-.0023	-.0026	.855	-.010	-.0047	-.0060	-.0033
		(.0012)	(.0072)	(.0074)	(.0070)		(.0009)	(.0072)	(.0074)	(.0066)
Unemployment	.027	.0043	.0028	.0011	.0017	.027	.0028	.0022	.0014	.0014
		(.0005)	(.0033)	(.0034)	(.0032)		(.0004)	(.0033)	(.0034)	(.0030)
Not in labor force	.112	.016	.0014	.0012	.0009	.118	.0074	.0025	.0046	.0019
		(.0011)	(.0066)	(.0068)	(.0064)		(.0008)	(.0066)	(.0068)	(.0060)
Usual hours worked	41.5	-.888	-.101	-.416	-.230	41.2	-.544	.055	-.216	-.137
		(.054)	(.334)	(.345)	(.325)		(.040)	(.335)	(.342)	(.305)
Weeks worked	44.8	-.752	-.133	-.282	-.192	44.5	-.243	-.120	-.280	-.175
		(.054)	(.330)	(.340)	(.321)		(.040)	(.331)	(.338)	(.301)
Income variables in 1999										
Wage and salary income	46406	-8616	-517	-1168	-873	46595	-7936	-115	-779	-548
		(161)	(1240)	(1283)	(1209)		(128)	(1243)	(1272)	(1133)
Log weekly wage	6.75	-.121	-.0038	-.018	-.0094	6.75	-.110	.009	-.0063	-.0030
		(.0026)	(.016)	(.017)	(.016)		(.0019)	(.016)	(.017)	(.015)
Self employment income	5261	-2772	855	890	867	5285	-2846	487	593	668
		(77.8)	(616)	(637)	(606)		(62.3)	(616)	(629)	(567)
Wage and salary income>0	.831	.014	.0046	.0006	.0032	.826	.021	-.0007	-.0064	.0005
		(.0012)	(.0076)	(.0078)	(.0074)		(.0009)	(.0076)	(.0078)	(.0069)
Self employment income>0	.129	-.042	.0023	.0036	.0029	.128	-.041	.0068	.0103	.0054
		(.0010)	(.0067)	(.0069)	(.0065)		(.0007)	(.0066)	(.0067)	(.0061)
				B. Nonwhites						
Work variables in 1999										
Employment	.665	.049	.018	.050	.033	.662	.063	.0013	.024	.020
		(.0040)	(.040)	(.042)	(.039)		(.0030)	(.040)	(.041)	(.037)
Unemployment	.056	-.0035	-.047	-.054	-.048	.054	-.0063	-.027	-.040	-.036
		(.0019)	(.019)	(.020)	(.019)		(.0014)	(.019)	(.019)	(.018)
Not in labor force	.279	-.045	.029	.0038	.015	.284	-.057	.026	.016	.016
		(.0039)	(.039)	(.040)	(.038)		(.0029)	(.039)	(.040)	(.035)
Usual hours worked	32.8	1.97	3.58	4.87	4.12	32.6	2.33	3.68	3.59	3.76
		(.171)	(1.71)	(1.79)	(1.68)		(.129)	(1.73)	(1.75)	(1.57)
Weeks worked	35.9	2.14	2.84	3.52	3.15	35.7	2.73	2.41	2.54	2.71
		(.186)	(1.86)	(1.94)	(1.82)		(.141)	(1.88)	(1.90)	(1.70)
Income variables in 1999										
Wage and salary income	27584	1324	3476	6706	4969	27711	2109	1006	2567	3314
		(313)	(3231)	(3418)	(3199)		(239)	(3255)	(3312)	(2968)
Log weekly wage	6.41	.028	-.037	.045	.012	6.43	.042	-.0090	.035	.019
		(.0074)	(.067)	(.069)	(.065)		(.0057)	(.067)	(.068)	(.060)
Self employment income	1709	-616	328	527	436	1708	-511	1750	1364	1115
		(108)	(1177)	(1246)	(1147)		(82.4)	(1167)	(1154)	(1077)
Wage and salary income>0	.736	.049	.058	.071	.067	.730	.058	.039	.042	.054
		(.0037)	(.037)	(.039)	(.037)		(.0028)	(.038)	(.038)	(.034)
Self employment income>0	.057	-.011	.012	.0086	.011	.057	-.0094	.017	.021	.017
		(.0019)	(.019)	(.020)	(.019)		(.0014)	(.019)	(.020)	(.018)

Note: All regressions include a full set of dummies for state of birth, year of birth and month of birth. Columns 3-5 and 8-10 report 2SLS estimates with the instrument sets listed. Robust standard errors are reported in parentheses. Estimates computed using sample weights.

Table 4a: Estimates of effects of military services in Vietnam era on education, by race and by year of birth

	1950-52					1948-52				
			2SLS					2SLS		
	Mean	OLS	elig	5z	5zx	Mean	OLS	elig	5z	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
				A. Whites						
Years of schooling (imputed)	13.8	-.551	.332	.353	.336	13.8	-.550	.294	.307	.314
		(.0074)	(.053)	(.055)	(.052)		(.0057)	(.053)	(.054)	(.049)
7th or 8th grade +	.990	.0083	.00005	.0013	.0005	.990	.010	-.0007	-.0002	-.0001
		(.0003)	(.0021)	(.0022)	(.0020)		(.0002)	(.0021)	(.0021)	(.0019)
9th grade +	.977	.015	.0056	.0075	.0061	.975	.020	.0021	.0035	.0040
		(.0004)	(.0031)	(.0031)	(.0030)		(.0003)	(.0031)	(.0032)	(.0028)
10th grade +	.965	.018	.0080	.0093	.0083	.963	.025	.0042	.0055	.0062
		(.0005)	(.0037)	(.0038)	(.0036)		(.0004)	(.0038)	(.0039)	(.0034)
11th grade +	.948	.021	.012	.013	.013	.946	.029	.0071	.0076	.010
		(.0007)	(.0045)	(.0046)	(.0044)		(.0005)	(.0045)	(.0046)	(.0041)
12th grade (no diploma) +	.931	.024	.015	.018	.016	.930	.033	.009	.013	.013
		(.0008)	(.0051)	(.0052)	(.0049)		(.0006)	(.0050)	(.0052)	(.0046)
High school graduate or higher +	.910	.025	.023	.024	.023	.908	.034	.017	.018	.020
		(.0009)	(.0057)	(.0059)	(.0056)		(.0006)	(.0057)	(.0058)	(.0052)
Some college (less than 1 year) +	.655	-.050	.079	.083	.079	.659	-.048	.064	.065	.070
		(.0015)	(.009)	(.010)	(.0093)		(.0011)	(.0094)	(.010)	(.0086)
1 or more years of college (no degree) +	.582	-.082	.090	.093	.089	.588	-.083	.074	.076	.080
		(.0016)	(.010)	(.010)	(.010)		(.0012)	(.010)	(.010)	(.0090)
Associate's degree +	.411	-.126	.081	.081	.079	.419	-.133	.074	.074	.076
		(.0015)	(.010)	(.010)	(.010)		(.0011)	(.010)	(.010)	(.0091)
Bachelor's degree +	.333	-.168	.053	.051	.051	.341	-.176	.051	.051	.051
		(.0014)	(.010)	(.010)	(.0094)		(.0010)	(.010)	(.010)	(.0088)
Master's degree +	.135	-.082	.016	.019	.017	.140	-.090	.019	.020	.018
		(.0009)	(.0070)	(.0072)	(.0068)		(.0007)	(.0070)	(.0072)	(.0064)
Professional degree+	.051	-.043	.0047	.0029	.0037	.052	-.046	.010	.0050	.0057
		(.0005)	(.0045)	(.0047)	(.0044)		(.0004)	(.0045)	(.0046)	(.0041)
Doctorate	.016	-.013	-.0011	-.0032	-.0022	.017	-.015	-.0021	-.0058	-.0032
		(.0003)	(.0026)	(.0026)	(.0025)		(.0003)	(.0026)	(.0026)	(.0023)

(Continued)

Table 4a: Estimates of effects of military services in Vietnam era on education, by race and by year of birth, continued

	1950-52					1948-52				
			2SLS					2SLS		
	Mean	OLS	elig	5z	5zx	Mean	OLS	elig	5z	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
B. Nonwhites										
Years of schooling (imputed)	12.6	.512	.203	.168	.190	12.6	.643	.184	.187	.196
		(.020)	(.230)	(.240)	(.226)		(.016)	(.235)	(.237)	(.211)
7th or 8th grade +	.973	.022	-.0029	.0058	-.0001	.971	.029	.0002	.0062	-.0012
		(.0012)	(.014)	(.015)	(.014)		(.0009)	(.015)	(.015)	(.013)
9th grade +	.948	.043	.0013	-.0038	.0003	.944	.055	-.009	-.0080	-.0018
		(.0016)	(.019)	(.020)	(.019)		(.0013)	(.020)	(.020)	(.018)
10th grade +	.923	.063	-.0056	-.0045	-.0044	.918	.079	-.015	-.0073	-.0050
		(.0019)	(.023)	(.024)	(.022)		(.0015)	(.023)	(.024)	(.021)
11th grade +	.882	.090	.019	.022	.019	.876	.110	.016	.030	.025
		(.0023)	(.027)	(.029)	(.027)		(.0018)	(.028)	(.028)	(.025)
12th grade (no diploma) +	.832	.122	-.0021	-.0045	-.0027	.826	.144	-.014	.0007	.0039
		(.0027)	(.032)	(.033)	(.031)		(.0021)	(.032)	(.032)	(.029)
High school graduate or higher +	.770	.147	.055	.051	.055	.766	.170	.045	.046	.058
		(.0032)	(.035)	(.037)	(.034)		(.0024)	(.035)	(.036)	(.032)
Some college (less than 1 year) +	.468	.158	.080	.074	.083	.468	.171	.094	.099	.092
		(.0042)	(.041)	(.043)	(.040)		(.0031)	(.041)	(.042)	(.037)
1 or more years of college (no degree) +	.400	.117	.070	.057	.068	.400	.132	.054	.056	.065
		(.0042)	(.040)	(.042)	(.040)		(.0032)	(.041)	(.041)	(.037)
Associate's degree +	.226	.024	.055	.041	.051	.228	.031	.042	.032	.051
		(.0036)	(.035)	(.036)	(.034)		(.0027)	(.035)	(.035)	(.032)
Bachelor's degree +	.160	-.032	.028	.010	.019	.163	-.026	.012	-.0070	.010
		(.0030)	(.031)	(.032)	(.030)		(.0023)	(.031)	(.031)	(.028)
Master's degree +	.057	-.020	.0080	.0035	.0067	.060	-.021	.020	.012	.011
		(.0018)	(.019)	(.020)	(.019)		(.0014)	(.020)	(.020)	(.018)
Professional degree+	.018	-.012	-.0028	-.0072	-.0026	.019	-.012	.0086	.0032	.0018
		(.0010)	(.011)	(.011)	(.011)		(.0008)	(.011)	(.012)	(.010)
Doctorate	.0061	-.0029	-.0032	-.0048	-.0030	.0065	-.0034	-.0025	-.0044	-.0030
		(.0006)	(.0066)	(.0068)	(.0065)		(.0005)	(.0069)	(.0069)	(.0061)

Note: All regressions include a full set of dummies for state of birth, year of birth and month of birth. Columns 3-5 and 8-10 report 2SLS estimates with the instrument sets listed. Robust standard errors are reported in parentheses. Estimates computed using sample weights.

Table 4b: 2SLS Estimates of effects of military services in Vietnam era on education, by race and single year of birth

	Whites					Nonwhites				
	1948	1949	1950	1951	1952	1948	1949	1950	1951	1952
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Years of schooling (imputed)	.321	.335	.359	.391	.287	.266	.165	.258	.400	.373
	(.057)	(.058)	(.060)	(.071)	(.099)	(.241)	(.242)	(.249)	(.299)	(.430)
7th or 8th grade +	-.0010	-.0009	.0001	.0006	-.0006	.019	.013	.022	.032	.013
	(.0022)	(.0023)	(.0023)	(.0028)	(.0039)	(.015)	(.015)	(.015)	(.018)	(.027)
9th grade +	.0030	.0029	.0063	.0046	.0018	.0068	.0071	.014	.040	.026
	(.0033)	(.0033)	(.0034)	(.0040)	(.0057)	(.020)	(.020)	(.021)	(.025)	(.035)
10th grade +	.0058	.0046	.0090	.0072	.0044	.017	.018	.024	.039	.049
	(.0040)	(.0040)	(.0042)	(.0049)	(.0069)	(.024)	(.024)	(.025)	(.030)	(.042)
11th grade +	.0087	.010	.014	.015	.012	.051	.042	.049	.056	.084
	(.0048)	(.0049)	(.0050)	(.0059)	(.0084)	(.029)	(.029)	(.030)	(.036)	(.052)
12th grade (no diploma) +	.016	.017	.022	.023	.023	.014	.010	.011	.013	-.0014
	(.0054)	(.0055)	(.0057)	(.0068)	(.0095)	(.033)	(.034)	(.035)	(.042)	(.061)
High school graduate or higher +	.021	.022	.028	.028	.028	.046	.049	.051	.082	.104
	(.0061)	(.0062)	(.0065)	(.0077)	(.011)	(.037)	(.037)	(.039)	(.047)	(.067)
Some college (less than 1 year) +	.065	.070	.080	.077	.050	.076	.061	.047	.075	.088
	(.010)	(.010)	(.011)	(.013)	(.018)	(.043)	(.043)	(.045)	(.055)	(.079)
1 or more years of college (no degree) +	.075	.081	.089	.081	.053	.036	.035	.033	.066	.100
	(.011)	(.011)	(.011)	(.013)	(.019)	(.042)	(.043)	(.045)	(.054)	(.078)
Associate's degree +	.075	.083	.081	.083	.065	.0028	.0009	.0042	.019	-.0081
	(.011)	(.011)	(.011)	(.013)	(.019)	(.036)	(.036)	(.038)	(.046)	(.066)
Bachelor's degree +	.053	.057	.053	.060	.044	-.015	-.014	-.0030	-.0062	-.0001
	(.010)	(.010)	(.011)	(.013)	(.018)	(.032)	(.032)	(.033)	(.040)	(.058)
Master's degree +	.023	.022	.022	.032	.026	.0073	-.0039	-.0015	-.010	.0025
	(.0075)	(.0075)	(.0078)	(.0092)	(.013)	(.020)	(.020)	(.021)	(.025)	(.035)
Professional degree+	.0066	.0058	.0052	.0060	.0046	-.0087	-.019	-.021	-.023	-.033
	(.0048)	(.0048)	(.0050)	(.0060)	(.0083)	(.012)	(.011)	(.012)	(.014)	(.020)
Doctorate	-.0039	-.0030	-.0009	-.0001	.0021	-.011	-.013	-.013	-.010	-.026
	(.0027)	(.0027)	(.0028)	(.0033)	(.0046)	(.0069)	(.0067)	(.0069)	(.0081)	(.011)

Note: The table reports 2SLS estimates of schooling effects by single year of birth using the *5z* instrument set. All regressions include a full set of dummies for state of birth, year of birth, and month of birth. Robust standard errors in parentheses. Estimates were computed using sample weights.

Table 5a: 2SLS estimates of effects of military services in Vietnam era on disability and other income variables, Whites

	1950-52					1948-52				
			2SLS					2SLS		
	Mean	OLS	elig	5z	5zx	Mean	OLS	elig	5z	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Disability variables										
Work disability	.123	.017	-.0005	-.0021	-.0012	.124	.013	-.0005	-.0003	-.0007
		(.0011)	(.0067)	(.0069)	(.0065)		(.0008)	(.0066)	(.0068)	(.0061)
Non-work disability	.070	.017	.017	.017	.017	.074	.011	.013	.013	.014
		(.0009)	(.0052)	(.0054)	(.0051)		(.0006)	(.0053)	(.0054)	(.0048)
Mobility	.082	.027	.013	.014	.013	.086	.018	.0085	.014	.012
		(.0010)	(.0056)	(.0058)	(.0054)		(.0007)	(.0057)	(.0058)	(.0051)
Self care	.021	.0022	.0080	.0081	.0080	.022	.00002	.0074	.0077	.0077
		(.0005)	(.0030)	(.0031)	(.0029)		(.0004)	(.0030)	(.0031)	(.0028)
Physical	.051	.0044	.0046	.0058	.0049	.052	.0013	.0050	.0045	.0046
		(.0007)	(.0046)	(.0047)	(.0045)		(.0005)	(.0045)	(.0047)	(.0042)
Mental	.044	.0065	.0062	.0048	.0055	.045	.0033	.0069	.0048	.0055
		(.0007)	(.0042)	(.0044)	(.0041)		(.0005)	(.0043)	(.0044)	(.0039)
Vision or hearing	.036	.0070	.014	.012	.013	.038	.0050	.011	.0094	.012
		(.0006)	(.0038)	(.0039)	(.0037)		(.0005)	(.0039)	(.0039)	(.0035)
B. Disability-income variables										
Other income (includes VDC)	392	472	235	231	229	440	499	302	314	266
		(12.6)	(68.1)	(71.6)	(66.5)		(9.14)	(69.2)	(71.8)	(62.6)
Other income >0	.054	.070	.039	.038	.038	.059	.072	.042	.042	.040
		(.0009)	(.0046)	(.0047)	(.0045)		(.0007)	(.0047)	(.0048)	(.0042)
Retirement/Dis. income (includes MDP)	691	984	380	393	398	855	947	563	523	475
		(23.2)	(115)	(120)	(113)		(18.1)	(129)	(133)	(109)
Retirement/Dis. income >0	.043	.062	.030	.032	.032	.051	.057	.030	.030	.031
		(.0009)	(.0042)	(.0043)	(.0041)		(.0006)	(.0044)	(.0045)	(.0039)
Social Security income (incudes SSDI)	293	42.5	-14.3	5.23	1.08	316	22.0	-4.40	18.5	4.69
		(6.42)	(38.3)	(39.8)	(37.3)		(4.82)	(39.5)	(40.4)	(35.3)
Social Security income >0	.033	.0027	.0030	.0051	.0044	.035	.00003	.0015	.0040	.0037
		(.0006)	(.0037)	(.0038)	(.0036)		(.0004)	(.0037)	(.0038)	(.0034)
Supplemental Security income (SSI)	110	-20.9	12.2	4.86	9.52	113	-29.9	3.19	-7.68	3.01
		(3.12)	(20.0)	(20.5)	(19.5)		(2.36)	(20.3)	(20.6)	(18.3)
Supplemental Security income >0	.017	-.0048	.0042	.0031	.0041	.017	-.0064	.0014	-.0001	.0025
		(.0004)	(.0027)	(.0027)	(.0026)		(.0003)	(.0026)	(.0027)	(.0024)

Note: All regressions include a full set of dummies for state of birth, year of birth and month of birth. Columns 3-5 and 8-10 report 2SLS estimates with the instrument sets listed. Robust standard errors are reported in parentheses. Estimates computed using sample weights.

Table 5b: 2SLS estimates of effects of military services in Vietnam era on disability and other income variables, Nonwhites

	1950-52					1948-52				
			2SLS					2SLS		
	Mean	OLS	elig	5z	5zx	Mean	OLS	elig	5z	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
A. Disability variables										
Work disability	.210	-.0050	-.050	-.052	-.054	.212	-.010	-.045	-.045	-.054
		(.0035)	(.034)	(.036)	(.033)		(.0026)	(.034)	(.035)	(.031)
Non-work disability	.116	.0051	-.0066	-.012	-.0062	.120	-.0013	-.016	-.012	-.0062
		(.0028)	(.027)	(.028)	(.026)		(.0021)	(.028)	(.028)	(.026)
Mobility	.132	.016	-.037	-.053	-.036	.139	.0055	-.028	-.035	-.030
		(.0030)	(.029)	(.030)	(.028)		(.0023)	(.029)	(.030)	(.026)
Self care	.040	-.0051	-.0048	-.013	-.0063	.042	-.0085	.011	-.0037	-.0011
		(.0017)	(.017)	(.018)	(.017)		(.0013)	(.017)	(.017)	(.016)
Physical	.121	-.012	-.0065	-.016	-.0015	.122	-.014	-.0080	-.024	-.0080
		(.0028)	(.028)	(.029)	(.027)		(.0021)	(.028)	(.028)	(.025)
Mental	.073	-.0012	.012	.0062	.0091	.076	-.0066	.015	.023	.011
		(.0023)	(.023)	(.023)	(.022)		(.0017)	(.023)	(.023)	(.021)
Vision or hearing	.045	-.0006	.037	.033	.034	.048	-.0032	.039	.038	.036
		(.0018)	(.018)	(.018)	(.017)		(.0014)	(.018)	(.018)	(.016)
B. Disability-income variables										
Other Income (includes VDC)	566	682	112.5	375	230	628	743	764	983	540
		(37.5)	(294)	(305)	(285)		(29.3)	(320)	(323)	(273)
Other income >0	.072	.083	.022	.044	.033	.078	.087	.034	.059	.040
		(.0027)	(.021)	(.022)	(.021)		(.0020)	(.022)	(.022)	(.020)
Retirement/Dis. income (includes MDP)	848	1017	378	420	415	973	956	1015	1066	847
		(63.2)	(493)	(471)	(465)		(51.6)	(590)	(534)	(458)
Retirement/Dis. income >0	.065	.071	.036	.028	.033	.071	.067	.063	.062	.052
		(.0025)	(.020)	(.021)	(.020)		(.0019)	(.021)	(.021)	(.019)
Social Security Income (includes SSDI)	424	-5.01	-258	-428	-328	464	-16.8	-64.4	-226	-220
		(18.6)	(182)	(191)	(180)		(15.0)	(195)	(196)	(172)
Social Security income >0	.057	-.0049	-.028	-.037	-.033	.060	-.0070	-.027	-.030	-.030
		(.0020)	(.019)	(.020)	(.019)		(.0015)	(.020)	(.020)	(.018)
Supplemental Security Income (SSI)	273	-109	102	97.0	89.8	276	-125	192	157	115
		(11.5)	(122)	(128)	(120)		(8.77)	(124)	(126)	(112)
Supplemental Security Income >0	.044	-.020	.010	.011	.0084	.044	-.023	.022	.021	.013
		(.0016)	(.018)	(.019)	(.017)		(.0012)	(.018)	(.018)	(.016)

Note: See notes to Table5a.

Table 6. Wage equations for white men born 1948-52

		Instrumental variables estimates						
		Elig+age	Elig+yob		5zx		7zx	
	OLS	2SLS	2SLS	LIML	2SLS	LIML	2SLS	LIML
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
First stage F-statistics, (Model I, Panel A)								
All instruments	-	155	93.3	-	16.8	-	12.7	-
RSN instruments	-	38.8	39.4	-	2.45	-	1.95	-
Adjusted multivariate F	-	24.5	15.0	-	3.61	-	2.89	-
Estimates	A. Without disability controls							
I. Potential experience adjusted for schooling								
Years of schooling (imputed)	.118	.070	.078	.075	.043	.0066	.051	.0025
	(.0007)	(.035)	(.034)	(.037)	(.030)	(.047)	(.028)	(.054)
Veteran-adjusted potential experience	-.057	-.0036	-.015	-.015	.011	.010	.017	.015
=age-educ-6-2*vet	(.0048)	(.031)	(.040)	(.040)	(.038)	(.040)	(.038)	(.040)
Potential experience squared	.0011	.0002	.0004	.0004	-.0001	.000001	-.0002	-.0001
	(.0001)	(.0005)	(.0007)	(.0007)	(.0007)	(.0007)	(.0007)	(.0007)
Experience derivative	.0090	.0069	.0066	.0067	.0082	.010	.0078	.010
	(.0006)	(.0019)	(.0018)	(.0020)	(.0017)	(.0025)	(.0016)	(.0028)
Reduced-form veteran effect	-.016	-.013	-.013	-.013	-.017	-.020	-.016	-.021
	(.0011)	(.0038)	(.0038)	(.0040)	(.0033)	(.0048)	(.0032)	(.0054)
II. Potential experence at average schooling								
Years of schooling (imputed)	.108	.064	.073	.070	.037	.0002	.045	-.0034
	(.0004)	(.036)	(.036)	(.039)	(.031)	(.049)	(.030)	(.055)
Veteran-adjusted potential experience	.153	-.0031	-.017	-.016	.017	.014	.023	.019
=age-mean(educ)-6-2*vet	(.016)	(.032)	(.041)	(.042)	(.039)	(.041)	(.039)	(.041)
Potential experience squared	-.0025	.0002	.0004	.0004	-.0002	-.0001	-.0003	-.0001
	(.0003)	(.0005)	(.0007)	(.0007)	(.0007)	(.0007)	(.0007)	(.0007)
Experience derivative	.010	.0070	.0066	.0067	.0085	.010	.0080	.010
	(.0006)	(.0019)	(.0019)	(.0020)	(.0017)	(.0025)	(.0016)	(.0028)
Reduced-form veteran effect	-.023	-.014	-.013	-.013	-.017	-.021	-.016	-.021
	(.0012)	(.0039)	(.0040)	(.0043)	(.0035)	(.0049)	(.0033)	(.0055)
	B. Using disability-adjusted log wage							
I. Potential experience adjusted for schooling								
Years of schooling (imputed)	.116	.077	.084	.082	.048	.014	.056	.012
	(.0007)	(.035)	(.034)	(.037)	(.030)	(.047)	(.028)	(.053)
Reduced-form veteran effect	-.017	-.014	-.013	-.013	-.017	-.020	-.016	-.021
	(.0011)	(.0037)	(.0037)	(.0040)	(.0033)	(.0048)	(.0032)	(.0054)
II. Potential experience at average schooling								
Years of schooling (imputed)	.106	.071	.079	.076	.042	.0069	.050	.0049
	(.0004)	(.036)	(.035)	(.038)	(.031)	(.049)	(.030)	(.055)
Reduced-form veteran effect	-.023	-.014	-.013	-.013	-.018	-.021	-.017	-.021
	(.0012)	(.0039)	(.0040)	(.0042)	(.0034)	(.0049)	(.0033)	(.0055)

Notes: The table reports estimates of the structural wage equation in the text. The sample includes white men born 1948-1952. The average experience in the sample is 28.85; average schooling is 13.8.

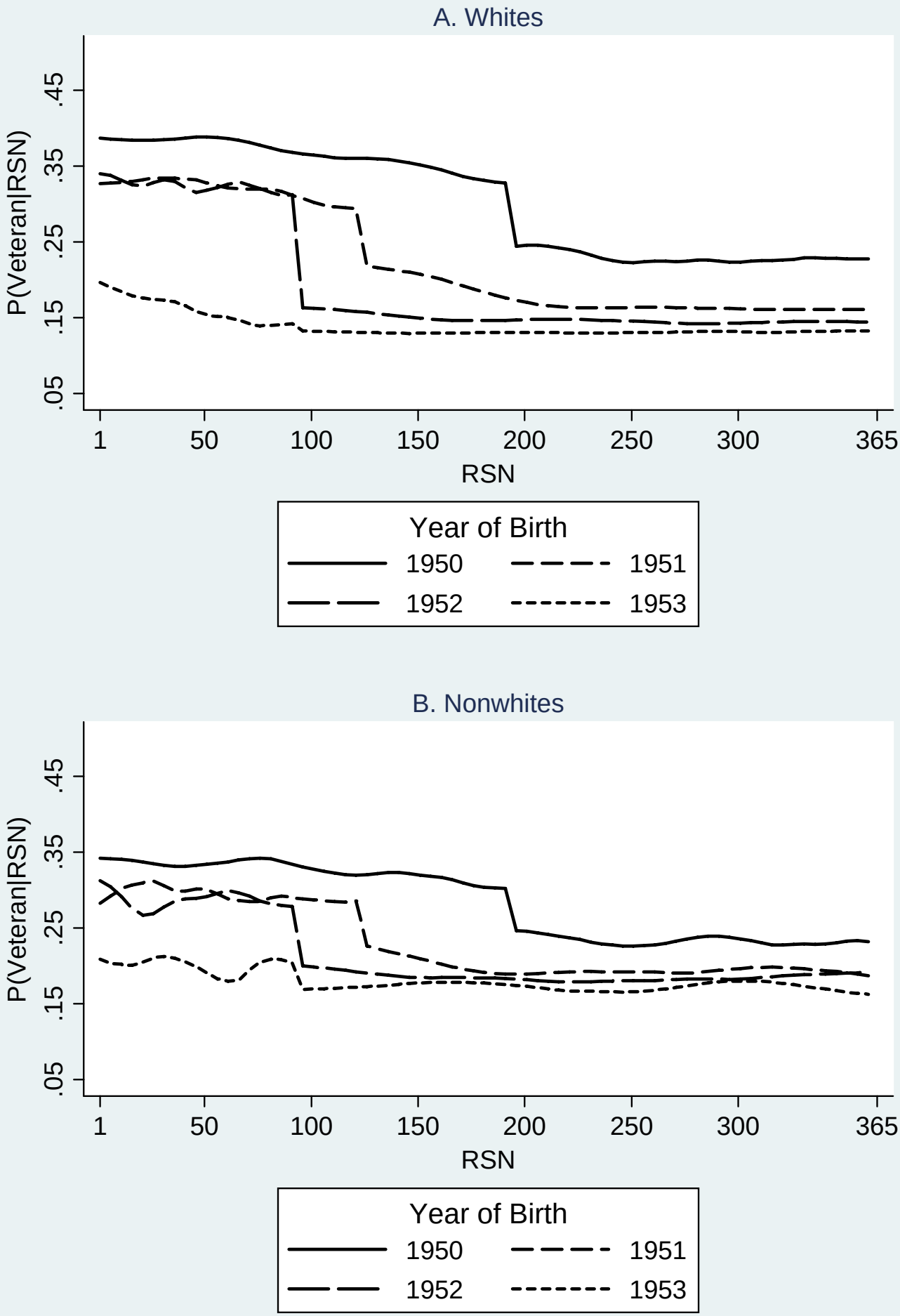
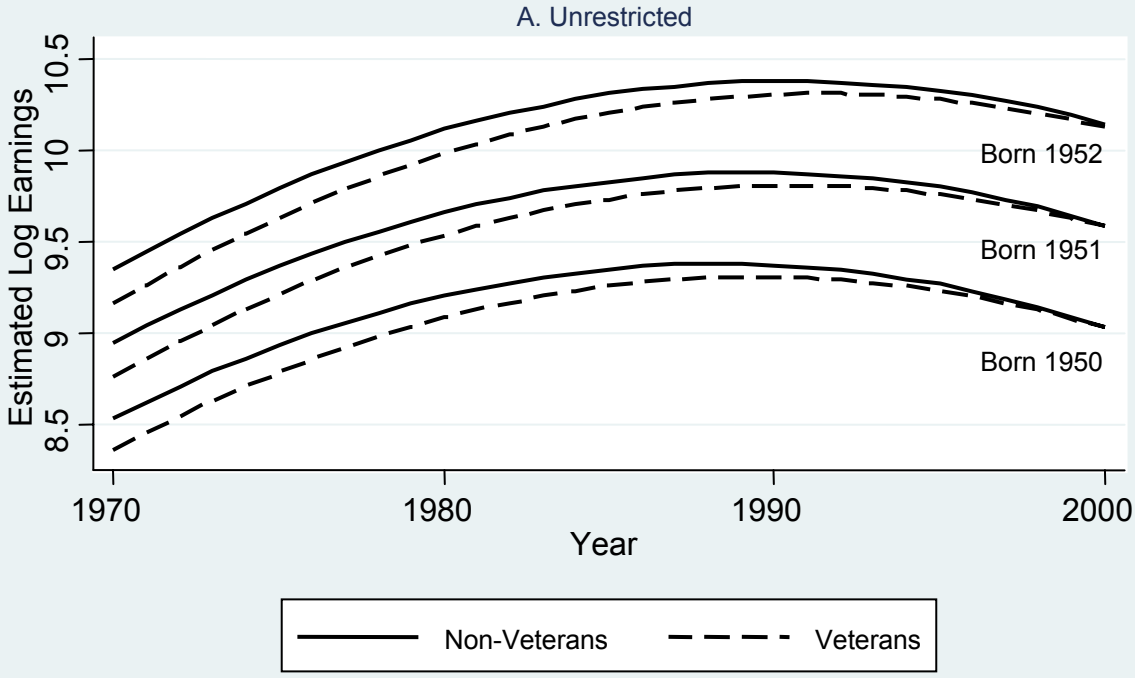
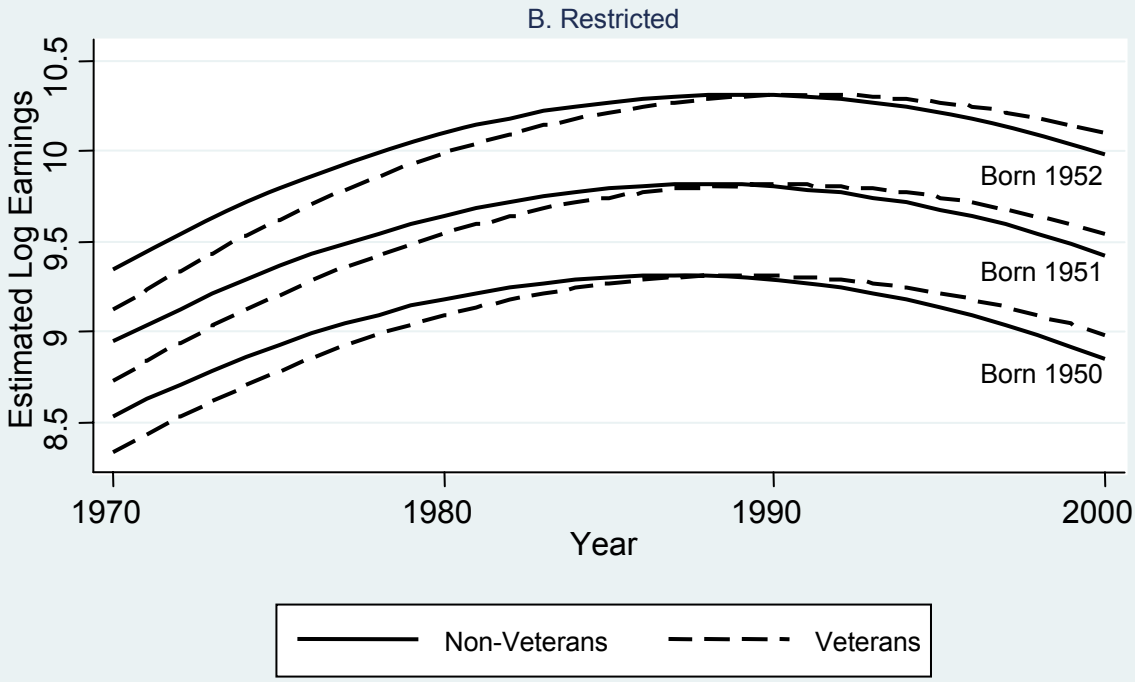


Figure 1. First-stage plots (bandwidth=.4). The relation between the probability of military service and draft lottery numbers. Data from the 2000 Census.



Note: Estimates from Angrist (1990), equation 6.



Note: Estimates from Angrist (1990), equation 5.

Figure 2. The Effect of Veteran Status on Experience Profiles

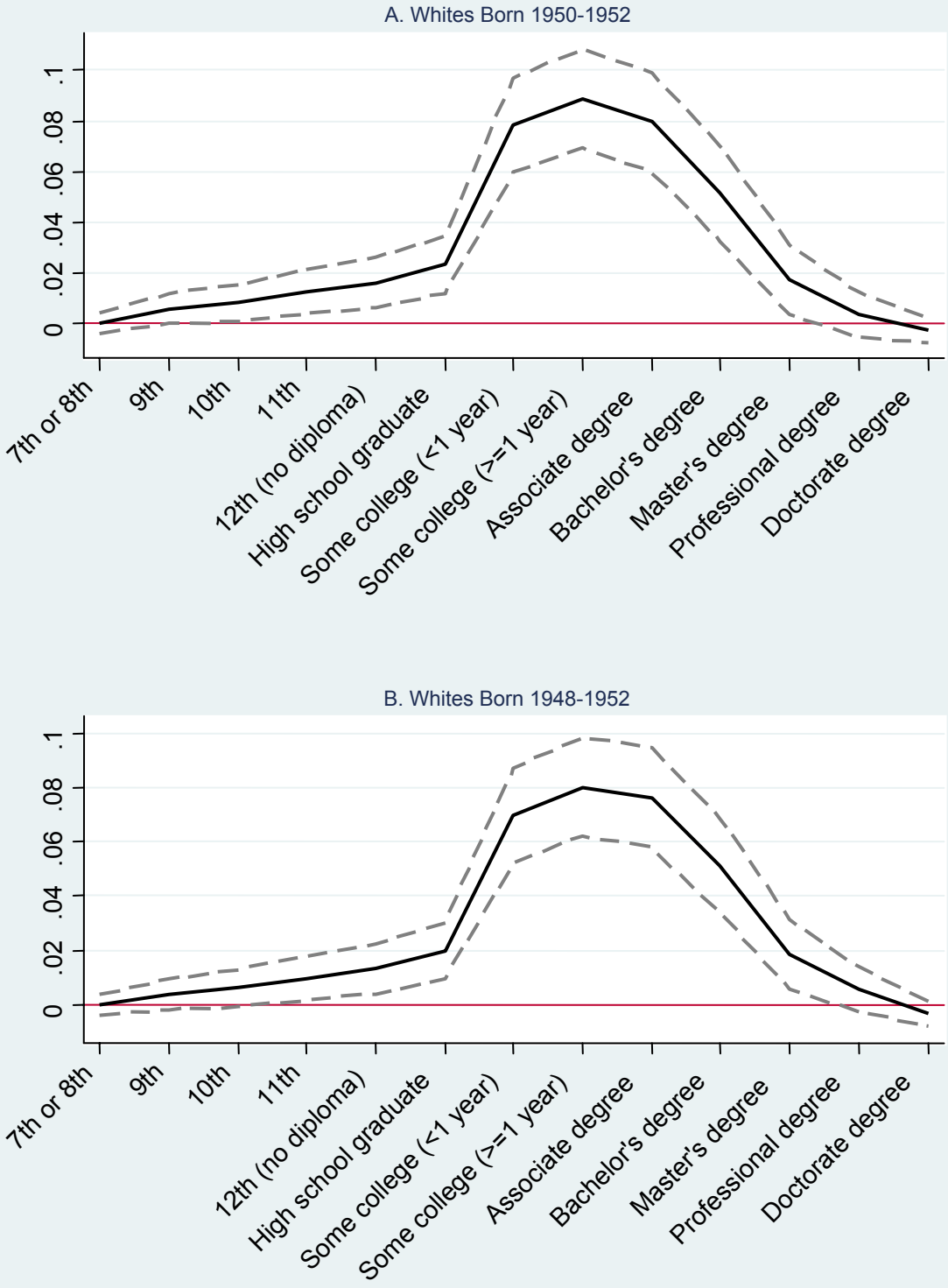
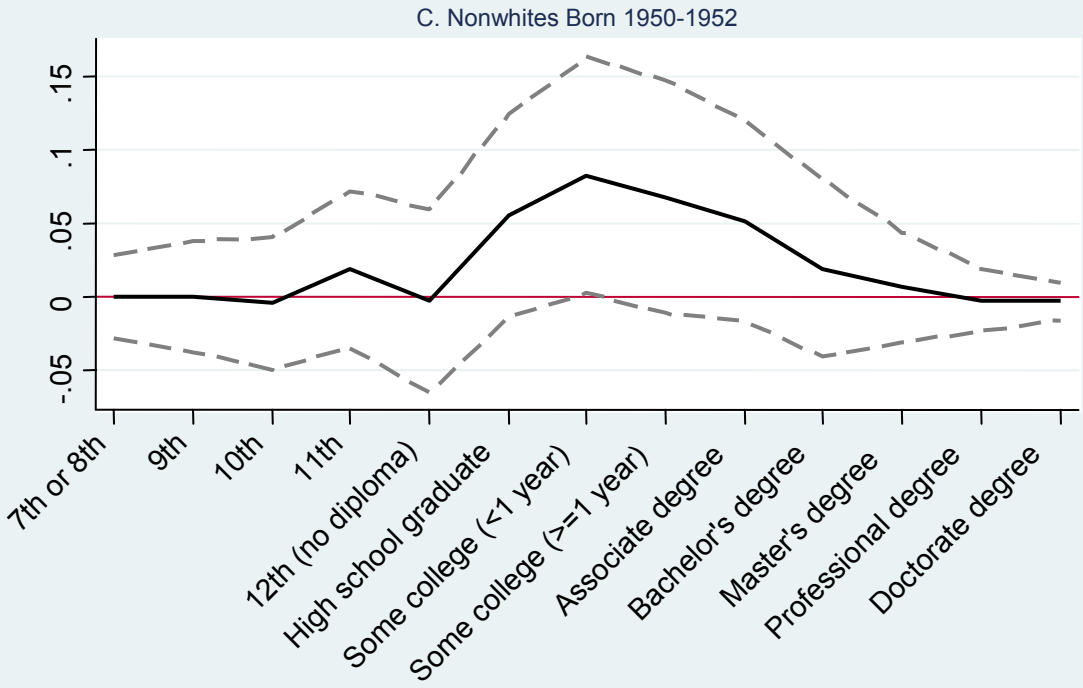


Figure 3a. 2SLS Estimates of the Effects of Vietnam-era Military Service on Education - White Men

Note: The figures plot estimates and standard error bands from Table 4.



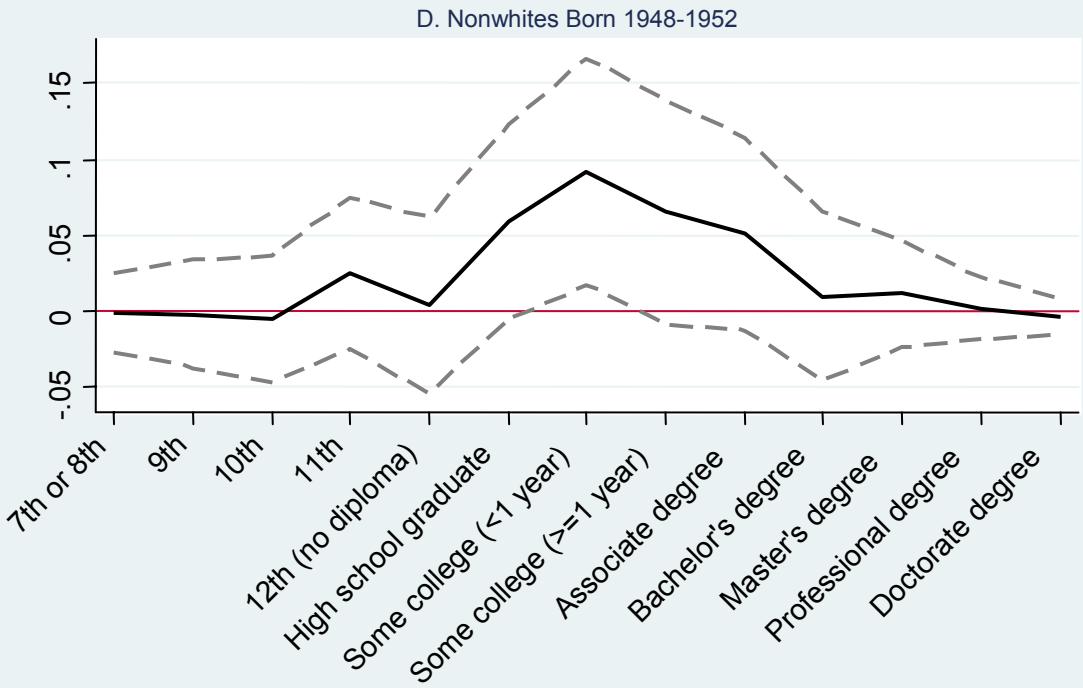
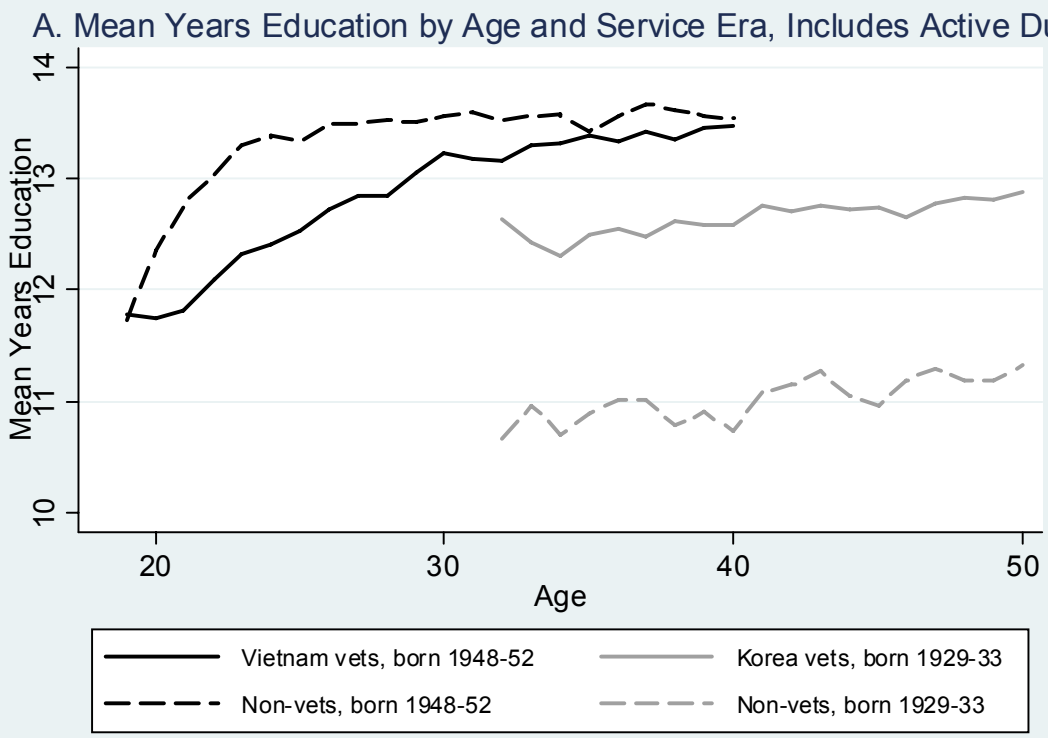


Figure 3b. 2SLS Estimates of the Effects of Vietnam-era Military Service on Education - Nonwhite Men

Note: The figures plot estimates and standard error bands from Table 4.



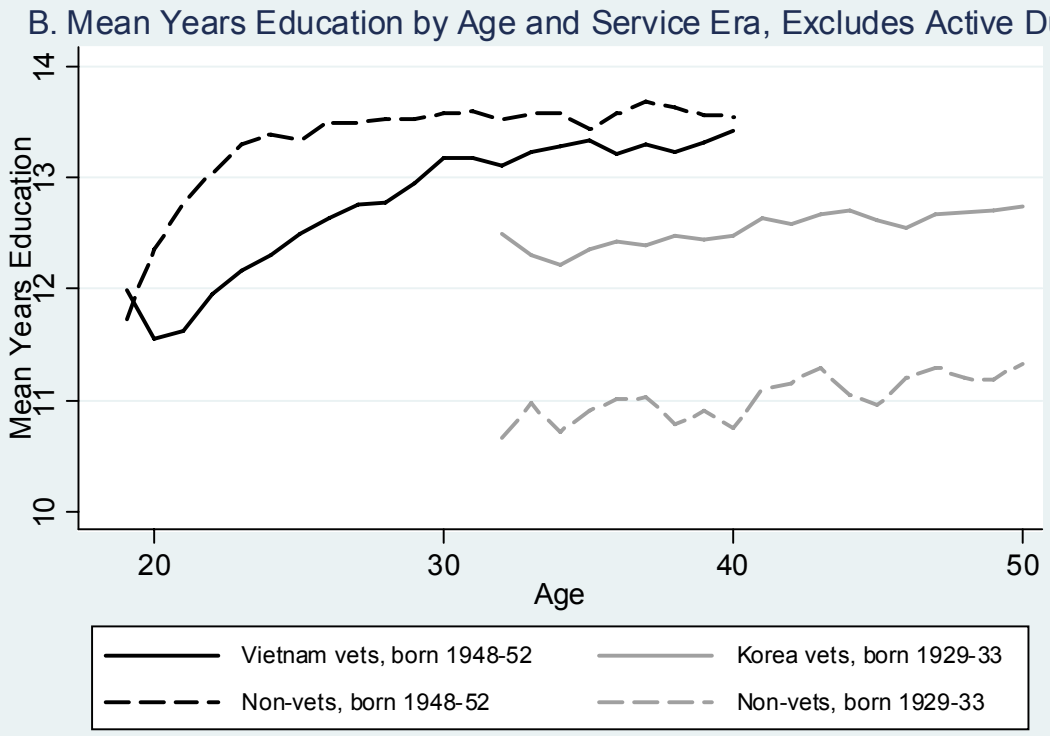


Figure 4. Average Schooling by Age, Veteran Status, and Era - Whites

Note: Education data from the March CPS.

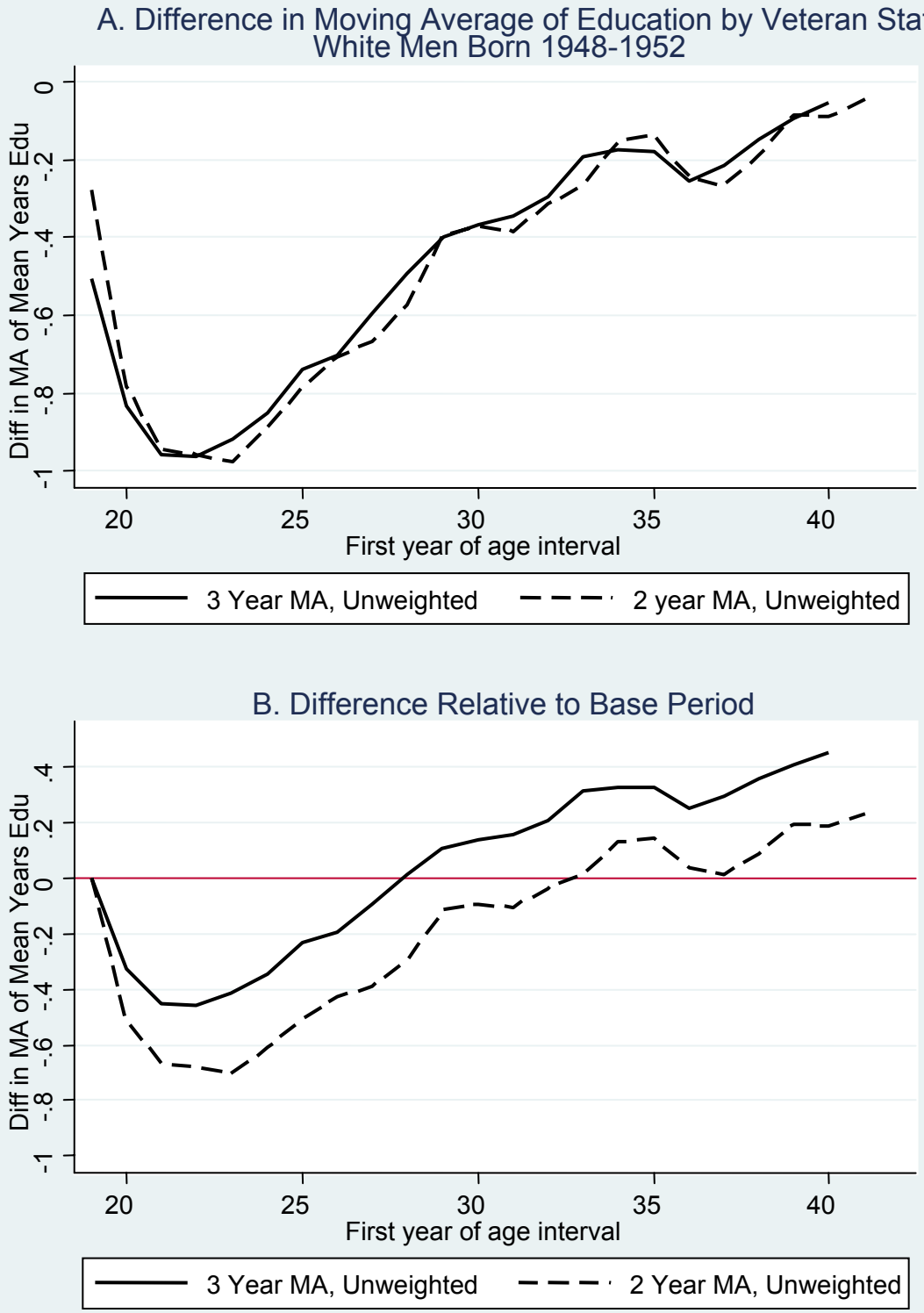


Figure 5. Schooling Differentials by Veteran Status - White Men Born 1948-1952

Note: Mean years education constructed from March CPS.

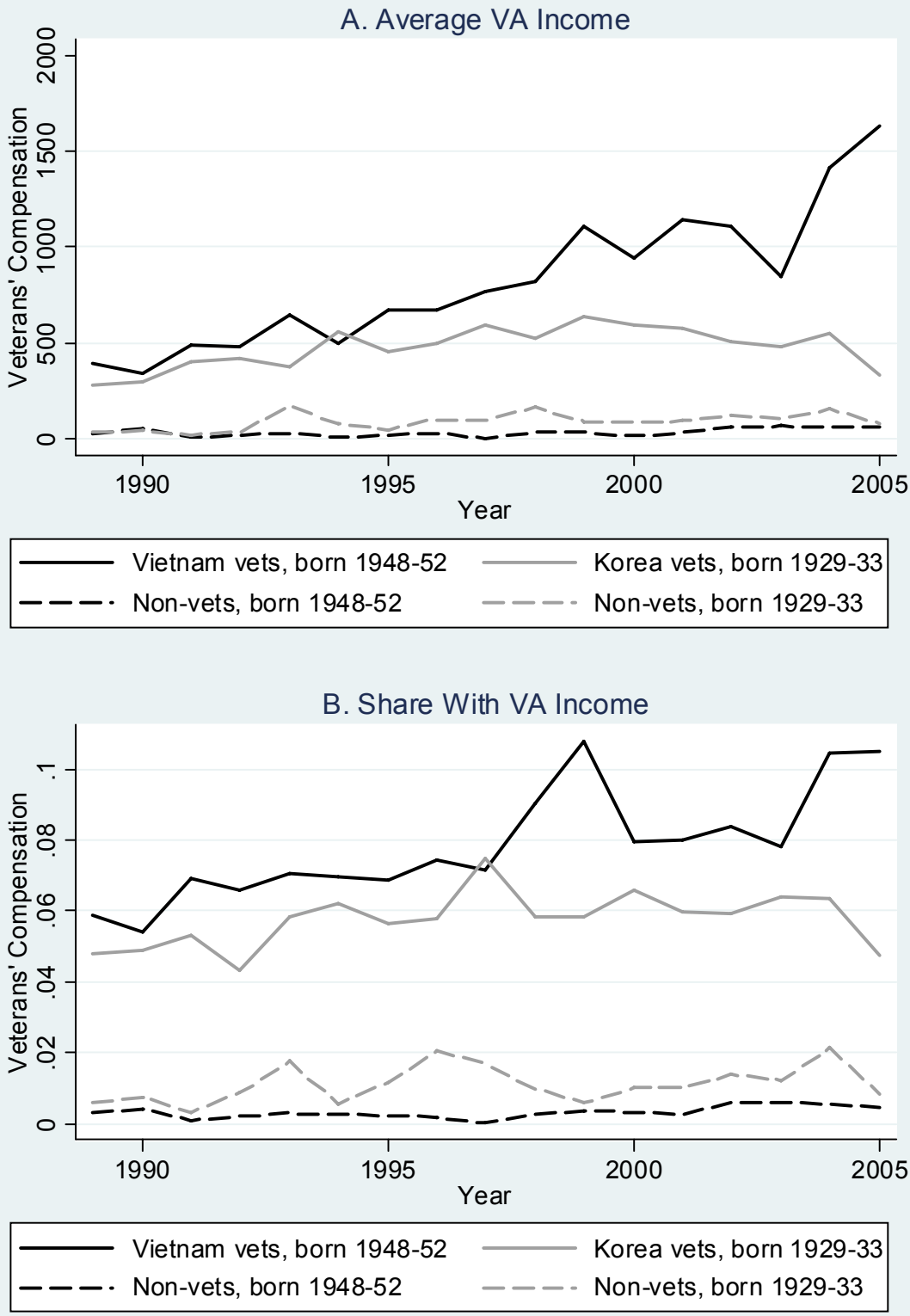


Figure 6. VA Income (Including VDC) by Year and Service Era - Whites

Note: Amounts in 2005 Dollars. Year is year before March CPS survey

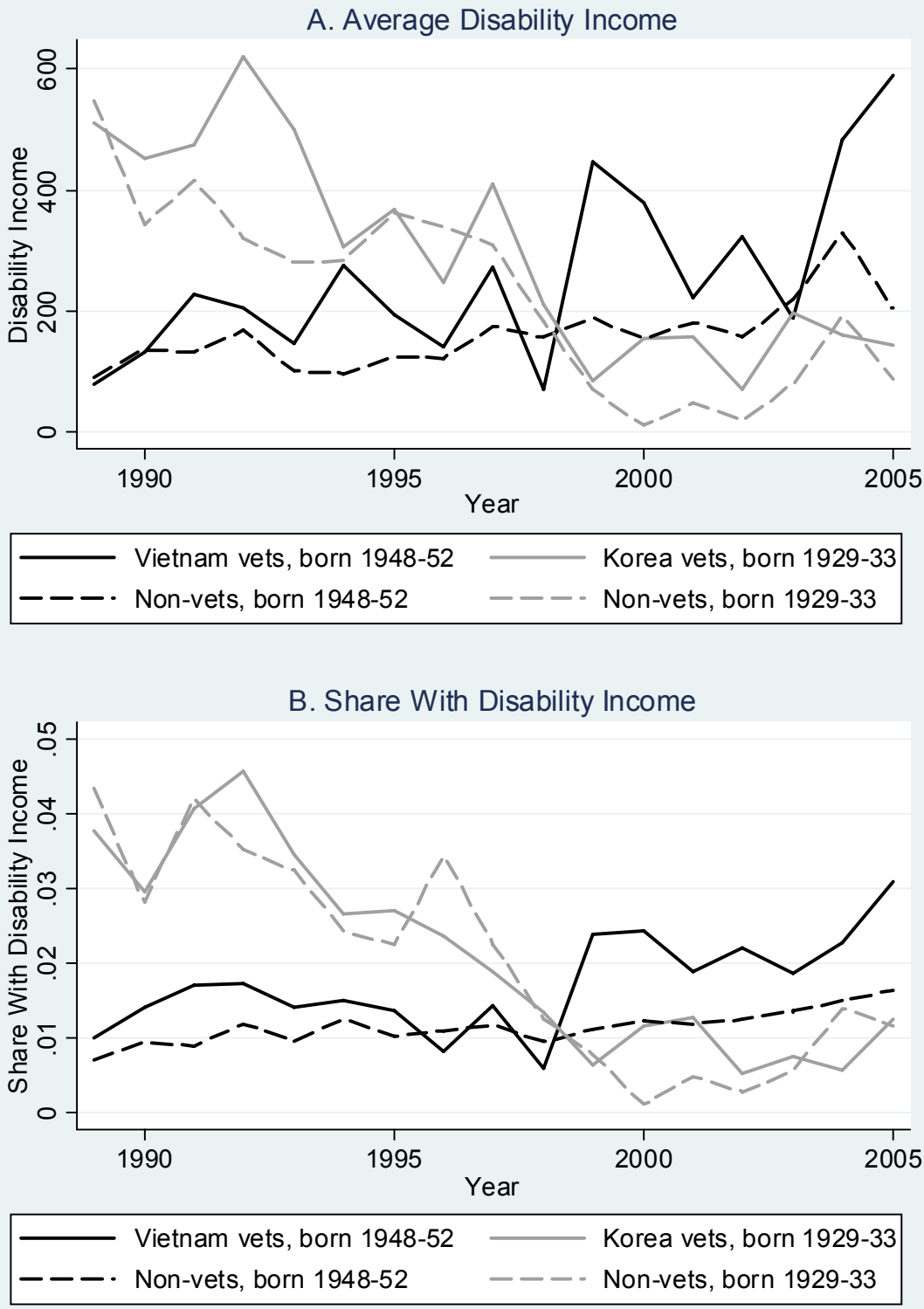


Figure 7. Non-SSA Disability Income (Including MDP) by Year and Service Era - Whites

Note: Amounts in 2005 Dollars. Year is year before March CPS survey.

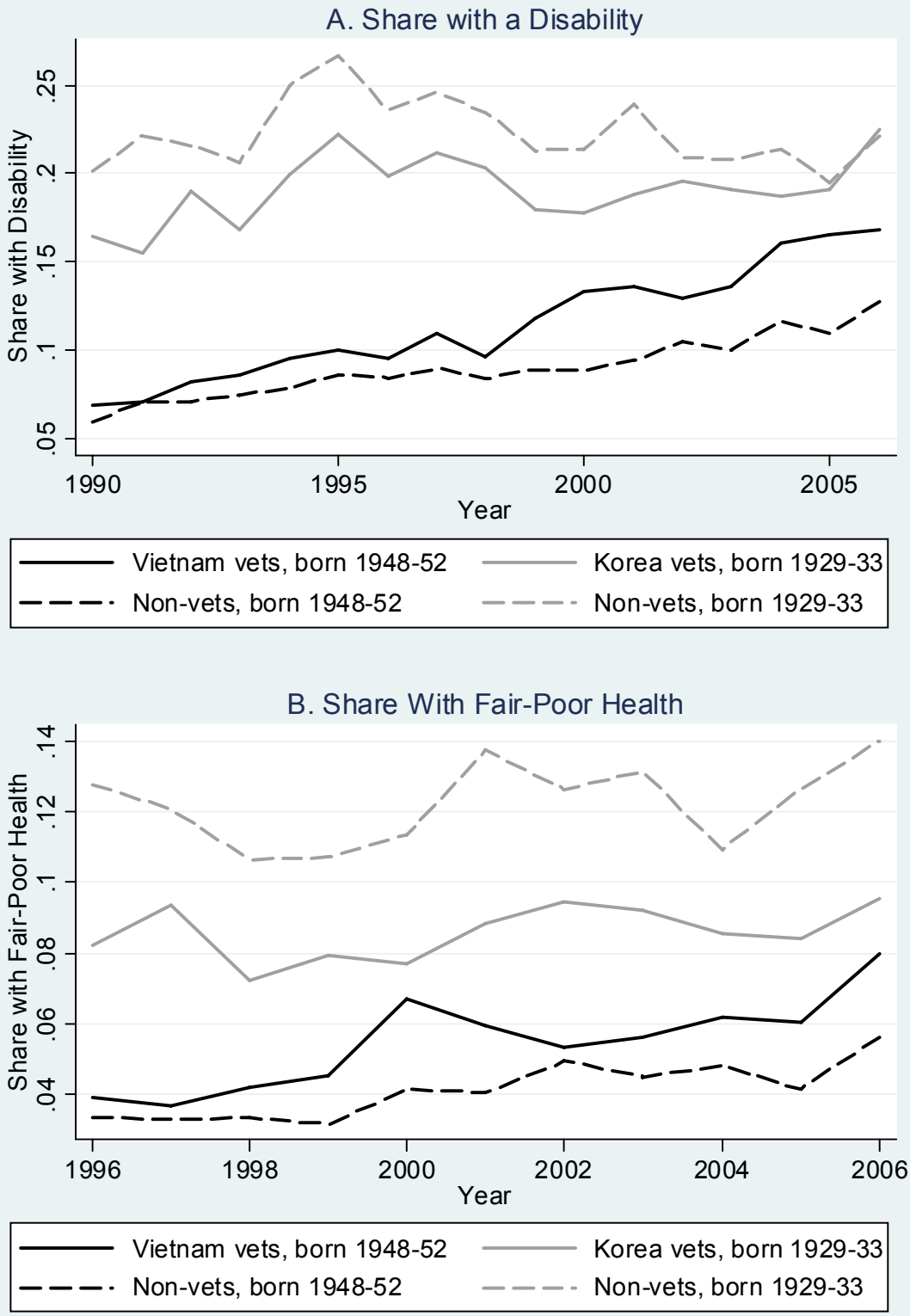


Figure 8. CPS Disability and Health Status by Year and Service Era - Whites

Note: The figure plots self-reported work disability and share with fair-poor health in the March CPS.

APPENDIX

A. Figures 4 and 5

Figures 4 and 5 use data from the 1964, 1965, and 1967-1991 CPS March Demographic Supplements (the 1966 supplement does not contain veteran status). All data were downloaded from the Minnesota Population Center's Integrated Public Use Microdata Series, accessible at www.ipums.org. We include Vietnam veterans and non-veterans born 1948-1952 in both figures, as well as Korea veterans and non-veterans born 1929-1933 in Figure 4. The CPS does not report quarter of birth, so we constructed birth year as if all men in the sample were born after the survey date. We categorized Vietnam veterans as all men born between 1948-1952 who were either veterans, as reported by the variable VETSTAT, or currently serving in the military, as reported by the variable EMPSTAT. Use of VETSTAT instead of period-of-service recodes adds a few veterans with post-Vietnam service, including some still in the military. Korea veterans were identified in an identical manner, except we used the 1929-1933 birth cohort.

Figure 4 reports mean years of education, derived from the variable HIGRADE, for veterans and non-veterans. Unlike CPS supplements from 1992 or later, the earlier supplements in our sample report years of education instead of highest degree obtained. The data are weighted using the person level weight PERWT, and collapsed over age rather than year, so at any given age, the average is derived from multiple years of data. We selected the sample so that at least three birth cohorts (i.e., three years of data) contribute to any given age-education observation.

Figure 5 collapses the education data by age in the same way described above. We then constructed two and three year moving averages of mean years of education. The moving averages are unweighted in that each age-education observation enters with equal weight in the moving average. Panel A reports the difference between the value of the moving average for veterans and non-veterans. The X-axis reports the first year of the age interval included in each moving average observation. (For example, the age 20 three-year moving average observation is the educational attainment of those aged 20, 21, and 22.) The same data

were used to construct panel B, except that this panel shows the difference between the moving average at age 19 and subsequent values.

B. Figures 6, 7, and 8

Figures 6, 7, and 8 use data from the 1990-2006 CPS March Demographic Supplements, again obtained from www.ipums.org. We constructed birth year and selected birth cohorts in the same way as described for Figures 4 and 5. Here, however, instead of assigning service era based on birth year, we used the variable VETLAST, which reports an individual's most recent period of service. All active duty servicemen were excluded from this sample.

The disability-related income variables most relevant for veterans in the CPS are Income from Veteran Benefits (INCVET) and Income from Disability Benefits (INCDISAB). INCVET captures any income from the VA., including service related disability payments (VDC), non-disability pension payments, and educational allowances. INCDISAB information is collected only for respondents who indicate the presence of a household member with a disability. This variable includes U.S. military retirement disability pensions (MDP) but excludes disability payments from the VA or Social Security. It also covers worker's compensation, company, union, federal government civil service, state, or local government disability programs, U.S. Railroad Retirement disability, private accident or disability insurance, black lung miner's disability, and state temporary sickness payments. Amounts are in 2005 dollars.

Men who reported a disability that limits or prevents work were identified from the variable DISABWRK. This is the screening variable for INCDISAB. Men with fair or poor health were identified using the variable HEALTH, which gives self-reported health status. This variable is only available from 1996-2006.

All plots show weighted means collapsed by year using PERWT. Since income amounts refer to the previous year in the March CPS, Figures 6 and 7 run from 1989-2005. Disability and health measures refer to the time of the survey, so Panel A of Figure 8 runs from 1990-2006 and Panel B from 1996-2006. Finally, all figures discussed in this appendix include both imputed and non-imputed values.

Table A1. Descriptive statistics for all White cohorts

	1950-52	1948-52	1948-53	1948	1949	1950	1951	1952	1953
Draft eligibility (by RSN)	.376	.437	.405	.530	.536	.538	.339	.260	.259
Veteran status (served in Vietnam Era)	.236	.305	.276	.446	.384	.300	.221	.193	.139
Post-Vietnam service	.038	.034	.037	.027	.030	.033	.037	.044	.050
Group quarters	.016	.015	.015	.014	.014	.015	.016	.016	.017
Now in military	.0027	.0024	.0026	.0019	.0022	.0024	.0026	.0030	.0032
Now in school	.028	.026	.027	.023	.024	.026	.028	.030	.031
Age	48.2	49.2	48.7	51.3	50.2	49.2	48.2	47.2	46.2
Married, spouse present	.709	.715	.713	.732	.721	.711	.711	.704	.701
A. Labor market variables									
Employment	.861	.855	.857	.843	.850	.855	.861	.865	.867
Unemployment	.027	.027	.027	.026	.027	.027	.027	.027	.028
Not in labor force	.112	.118	.116	.131	.124	.118	.112	.107	.105
Usual hours worked	41.5	41.2	41.3	40.5	40.9	41.2	41.5	41.7	41.8
Weeks worked	44.8	44.5	44.6	43.9	44.2	44.4	44.8	45.0	45.1
Wage and salary income	46406	46595	46521	46830	46957	46293	46592	46331	46176
Log weekly earnings (positive values)	6.75	6.75	6.75	6.77	6.77	6.75	6.75	6.74	6.73
Self employment income (positive values)	5261	5285	5244	5400	5249	5226	5182	5369	5048
B. Disability and disability income variables									
Work disability	.123	.124	.123	.128	.125	.125	.122	.121	.119
Other disabilities	.143	.149	.146	.163	.153	.149	.142	.138	.133
Non-work disabilities	.070	.074	.072	.082	.077	.074	.070	.068	.065
Any disabilities	.193	.198	.196	.211	.202	.199	.192	.189	.184
Mobility	.082	.086	.084	.097	.090	.087	.082	.077	.074
Self care	.021	.022	.022	.024	.022	.022	.021	.020	.020
Physical	.051	.052	.052	.055	.053	.053	.051	.050	.049
Mental	.044	.045	.045	.048	.047	.046	.044	.042	.042
Vision or hearing	.036	.038	.037	.043	.041	.038	.035	.034	.032
Other income (e.g.,VA, UI, child support, alimony; incl. VDC)	392	440	420	546	486	444	377	357	328
Other income >0	.054	.059	.057	.070	.064	.058	.053	.050	.048
Retirement/Dis. income (Retirement, survivor, disability pensions; incl. MDP)	691	855	800	1223	1007	787	673	621	541
Retirement income >0	.043	.051	.048	.068	.059	.049	.043	.038	.036
Social Security income (incl. SSDI)	293	316	306	363	341	321	290	271	260
Social Security income >0	.033	.035	.034	.039	.037	.035	.033	.031	.031
Supplemental Security income (SSI)	110	113	112	121	115	112	110	108	110
Supplemental Security income >0	.017	.017	.017	.017	.017	.017	.016	.016	.017
C. Education									
Imputed highest grade completed	13.8	13.8	13.8	13.9	13.8	13.8	13.8	13.7	13.6
7th or 8th grade +	.990	.990	.990	.989	.990	.989	.991	.990	.991
9th grade +	.977	.975	.976	.971	.974	.975	.978	.978	.979
10th grade +	.965	.963	.963	.958	.961	.963	.966	.966	.966
11th grade +	.948	.946	.946	.942	.943	.945	.948	.949	.948
12th grade (no diploma) +	.931	.930	.930	.927	.928	.930	.932	.932	.930
High school graduate +	.910	.908	.908	.906	.907	.908	.910	.910	.907
Some college (less than 1 year) +	.655	.659	.654	.667	.667	.662	.657	.646	.629
1 or more years of college (no degree) +	.582	.588	.582	.599	.598	.591	.584	.571	.551
Associate's degree +	.411	.419	.413	.433	.428	.420	.411	.402	.387
Bachelor's degree +	.333	.341	.335	.358	.350	.342	.333	.324	.309
Master's degree +	.135	.140	.137	.151	.145	.139	.135	.131	.122
Professional degree +	.051	.052	.051	.054	.053	.051	.051	.050	.047
Doctorate	.016	.017	.016	.018	.018	.016	.016	.015	.014
N (log earnings)	573728	934666	1134983	178349	182315	183435	191559	198734	200267
N (all other variables)	696530	1141905	1382708	220891	224130	223984	232348	240198	240736

Note: Sample weights are used in all estimates and statistics.

Table A2. Descriptive statistivs for all Nonwhite cohorts

	1950-52	1948-52	1948-53	1948	1949	1950	1951	1952	1953
Draft eligibility (by RSN)	.382	.440	.408	.538	.537	.544	.343	.265	.265
Veteran status (served in Vietnam Era)	.293	.293	.274	.404	.353	.285	.231	.216	.183
Post-Vietnam service	.058	.058	.066	.039	.042	.050	.071	.083	.101
Group quarters	.064	.064	.066	.056	.060	.064	.066	.071	.076
Now in military	.0025	.0025	.0028	.0020	.0019	.0020	.0027	.0038	.0039
Now in school	.043	.043	.044	.038	.039	.045	.044	.048	.050
Age	49.2	49.2	48.6	51.3	50.2	49.3	48.2	47.3	46.2
Married, spouse present	.502	.512	.509	.535	.522	.511	.504	.492	.495
A. Labor market variables									
Employment	.665	.662	.663	.657	.654	.662	.666	.669	.670
Unemployment	.056	.054	.055	.047	.055	.053	.056	.057	.059
Not in labor force	.279	.284	.282	.296	.291	.285	.279	.274	.270
Usual hours worked	32.8	32.6	32.7	32.1	32.3	32.6	32.8	33.1	33.0
Weeks worked	35.9	35.7	35.7	35.4	35.4	35.7	35.8	36.1	35.9
Wage and salary income	27584	27711	27561	28395	27490	27569	27508	27670	26874
Log weekly earnings (positive values)	6.41	6.43	6.42	6.46	6.44	6.42	6.41	6.40	6.38
Self employment income (positive values)	1709	1708	1694	1734	1682	1775	1702	1653	1632
B. Disability and disability income variables									
Work disability	.210	.212	.211	.214	.216	.213	.210	.207	.205
Any other disabilities	.247	.254	.251	.267	.264	.252	.249	.241	.237
Non-work disabilities	.116	.120	.118	.130	.125	.119	.115	.114	.110
Any disabilities	.326	.332	.329	.343	.342	.331	.325	.321	.314
Mobility	.132	.139	.135	.152	.147	.138	.135	.125	.118
Self care	.040	.042	.041	.046	.044	.041	.041	.038	.038
Physical	.121	.122	.122	.125	.124	.123	.122	.117	.119
Mental	.073	.076	.075	.080	.079	.073	.075	.072	.074
Vision or hearing	.045	.048	.047	.054	.052	.046	.046	.043	.044
Other income (e.g.,VA, UI, child support, alimony; incl. VDC)	566	628	609	763	708	627	559	513	520
Other income >0	.072	.078	.076	.091	.084	.075	.070	.070	.068
Retirement income (Retirement, survivor, disability pensions; incl. MDP)	848	973	929	1258	1117	916	867	765	731
Retirement income >0	.065	.071	.068	.085	.077	.068	.065	.062	.058
Social Security income (incl. SSDI)	424	464	451	547	515	445	449	382	391
Social Security income >0	.057	.060	.059	.068	.064	.058	.059	.053	.052
Supplemental Security income (SSI)	273	276	274	273	288	276	274	269	264
Supplemental Security income >0	.044	.044	.044	.044	.046	.045	.045	.043	.042
C. Education									
Imputed highest grade completed	12.6	12.6	12.6	12.5	12.5	12.6	12.6	12.6	12.6
7th or 8th grade +	.973	.971	.972	.967	.967	.971	.973	.975	.977
9th grade +	.948	.944	.946	.936	.936	.946	.948	.951	.953
10th grade +	.923	.918	.920	.908	.908	.920	.923	.927	.930
11th grade +	.882	.876	.878	.865	.866	.880	.882	.884	.887
12th grade (no diploma) +	.832	.826	.828	.818	.817	.829	.831	.835	.833
High school graduate +	.770	.766	.767	.759	.758	.768	.771	.772	.770
Some college (less than 1 year) +	.468	.468	.467	.470	.464	.466	.472	.466	.461
1 or more years of college (no degree) +	.400	.400	.399	.406	.398	.399	.404	.397	.392
Associate's degree +	.226	.228	.227	.235	.229	.231	.226	.221	.221
Bachelor's degree +	.160	.163	.162	.170	.164	.165	.162	.154	.156
Master's degree +	.057	.060	.058	.068	.062	.061	.059	.051	.052
Professional degree +	.018	.019	.019	.021	.019	.020	.019	.017	.017
Doctorate	.0061	.0065	.0063	.0078	.0069	.0073	.0058	.0051	.0052
N (log earnings)	71045	113194	137938	20286	21863	23383	23004	24658	24744
N (all other variables)	96217	154810	188023	28272	30321	31942	31162	33113	33213

Note: Sample weights are used in all estimates and statistics.

Table A3. Theoretical and empirical proportion draft eligible

Cohort	Theoretical Eligibility	Differential		
		All	White	Nonwhite
	(1)	(2)	(3)	(4)
1948	195/366	-0.0015	-0.0025	0.0048
	(.533)	(.0011)	(.0012)	(.0022)
1949	195/365	0.0018	0.0017	0.0028
	(.534)	(.0011)	(.0012)	(.0033)
1950	195/365	0.0049	0.0041	0.0097
	(.534)	(.0011)	(.0012)	(.0032)
1951	125/365	-0.0025	-0.0030	0.0002
	(.342)	(.0011)	(.0011)	(.0031)
1952	95/366	0.0008	0.00003	0.0055
	(.260)	(.0010)	(.0010)	(.0028)
1953	95/365	-0.0002	-0.0011	0.0050
	(.260)	(.0010)	(.0010)	(.0028)
F(6,∞)		5.07	4.37	3.24
N		1570310	1382287	188023

Notes: For each cohort the table reports the theoretical proportion draft eligible in column 1. Ratios are followed by their numerical equivalents in parentheses. Columns 2 -4 report the difference between this and the empirical proportion draft-eligible, with robust standard errors in parentheses. The F-stsatistic is the joint test across cohorts.

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