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Schooling and the Vietnam-Era GI Bill: Evidence from the Draft Lottery

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Abstract

Draft-lottery estimates of the consequences of Vietnam-era service using 2000 census data show marked schooling gains for veterans. We argue that these gains can be attributed to Vietnam veterans' use of the GI Bill rather than draft avoidance behavior. At the same time, draft lottery estimates of the earnings consequences of Vietnam-era service are close to zero in 2000. These results can be reconciled by a flattening of the age-earnings profile in middle age and a modest economic return to the schooling subsidized by the GI Bill. Other long-run consequences of Vietnam-era service include increased migration and public-sector employment.

Economists have long argued that compulsory military service amounts to a hidden tax on soldiers. American conscripts were paid poorly while in the military and lost valuable labor-market experience relative to their civilian counterparts (Walter Oi, 1967). On the other hand, some social scientists see military service as a possible leg up, even for draftees, primarily because of the generous GI Bill schooling benefits available to

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veterans. It's hard to exaggerate the role played by the GI Bill in contemporary social history (see, e.g., Edward Humes 2006). Consistent with this positive view, World War II (WWII) veterans typically earn somewhat more than same-age nonveterans, though white Vietnam-era veterans, who had access to a similarly generous wartime GI Bill, do a little worse (see, for example, studies of veteran effects cited in Joshua D. Angrist and Alan Krueger 1994).

This paper presents new estimates of the long-term causal effects of Vietnam-era service on veterans. As in Angrist (1989, 1990), the problem of selection bias is solved here using the Vietnam-era draft lotteries to construct instrumental-variables (IV) estimates. However, our study differs from earlier draft-lottery studies in a number of ways. First and most importantly, we use recently available 2000 Census data (which includes exact date of birth in non-public use files) to look at the long-term effects of Vietnam-era service on several outcomes. Motivated by the historical and economic importance of the GI Bill, our inquiry begins with the educational consequences of Vietnam-era service. Post-service schooling is especially interesting in this context because Vietnam veterans had access to GI Bill benefits similar to those offered to veterans of WWII and Korea.

We also estimate effects on veteran earnings in 1999, and reconcile the earnings results with our estimated GI Bill effects using a Mincer-style wage equation. The earnings investigation is of special interest in this case because the 2000 Census catches Vietnam veterans around the time they were turning 50, an age when experience profiles are relatively flat and earnings losses due to lost experience should have dissipated. Finally, we look briefly at a number of other channels through which Vietnam-era service many have changed veterans' lives.

Our study builds on a large body of research on the effects of military service on veterans' schooling and earnings. One of the earliest estimates of the economic returns to veterans' post-service schooling appears in Zvi Griliches and William M. Mason (1972), which reports results for a sample of WWII veterans from the 1964 CPS. The notion that time spent on active duty military service should be seen as lost civilian labor market experience appears in Mason (1970). Saul Schwartz (1986) estimated the returns to schooling for Vietnam and Korean-era veterans, arguing that the GI Bill probably lowers returns. Angrist (1993) estimated the impact of GI Bill subsidies on schooling and the economic return to schooling for Vietnam veterans, while Thomas Lemieux and David Card (2001) used instruments derived from cohort-province differences in enlistment rates to estimate the effects of the Canadian WWII GI Bill. As far as we know, however, ours is the first study to use the draft lottery to identify GI Bill effects.

Our investigation uncovers several important findings. First, Vietnam-era conscription increased schooling, with effects of a magnitude similar to those reported in studies of the WWII and Korean-era GI Bills by John Bound and Sarah Turner (2002) and Marcus Stanley (2003). The estimated economic returns to the GI Bill schooling increment are about 7 percent, markedly smaller than the corresponding OLS estimates. Based on estimates of a Mincer-style wage equation, the resulting earnings gains are roughly offset by modest earnings loss due to lost experience, producing a net earnings impact of zero. Finally, we find a large veteran effect on public-sector employment and a moderate decrease in probability of living in the state of birth. On the other hand, we find little evidence of lasting changes in employment rates, average earnings, or marital status.[1]

The remainder of the paper is organized as follows. Section I describes the non-public-use 2000 Census file and reports the draft-lottery first-stage estimates generated by these data. Section II reports our estimates of effects on schooling and explains why we think these can be attributed to the GI Bill. Section III discusses effects on work and earnings and reconciles the schooling and earnings results using a simple Mincer framework. Section IV looks briefly at other consequences of Vietnam-era service and Section V concludes.

I 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. 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 in 1948-53 or in subsets of these birth years. The cohorts of 19-year-olds most at risk of conscription in the draft lotteries were born in 1950-52, so our analysis looks at the sample of men in this group. This sample includes about 700,000 whites and 96,000 nonwhites. There is a smaller but non-negligible draft-lottery impact on men born in 1948-49, so we report estimates for an expanded sample of men born in 1948-52 as well. The 1948-52 sample includes more than 1.14 million whites and about 155,000 nonwhites. We also report first-stage estimates for men born in

[1]See Angrist, Stacey H. Chen, and Brigham Frandsen (2010) for a detailed investigation of the health and disability consequences of Vietnam-era service.

1953, since they were assigned lottery numbers though no one in this cohort was drafted (additional data notes appear in the appendix).

Roughly 24 percent of men born in 1950-52 served in the Vietnam-era and about 38 percent were draft-eligible, as the first column of Table 1 shows. Average schooling is 13.8 years for whites and 12.6 years for nonwhites. The average years of college, an important variable in our investigation of GI Bill effects, is 1.76 for whites and 1.05 for nonwhites.[2] The contrast in average educational attainment by veteran status shows white veterans with less schooling and non-white veterans with more schooling, on average, than their non-veteran counterparts. On the other hand, white veterans are less likely than white nonveterans to have attended college but *more* likely to have graduated from high school, while nonwhite veterans are more likely than nonveterans to have either attended college or graduated from high school. Similar patterns for men born in 1948-52 are documented in Appendix Table A1.

Statistics for labor-market variables are summarized in Panel B of Table 1. Among whites, veterans have lower employment rates and earnings than nonveterans, while the pattern is reversed for nonwhites. The annual 1999 earnings of white veterans were about $39,500, while white nonveterans earned $48,600 that year. Unemployment rates were low in both the veteran and non-veteran groups, but many men, especially nonwhites, were out of the labor force. Means of other outcome variables are reported in Panel C. Veterans are more likely than nonveterans to work in the public sector, especially in the federal government. Regardless of race, veterans are slightly more likely than nonveterans to have married and moderately more likely to live outside their state of birth.

B The Draft-Lottery First Stage

The first draft lottery, held in December 1969, affected men born 1944-50 who were at risk of conscription in 1970, while subsequent draft lotteries involved 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 the lottery year. In each draft lottery, numbers from 1-366, known as random

[2] We imputed years of schooling with a modification of the the scheme in David A. Jaeger (1997). See the appendix for details. Years of college ranges from 0-4 and was constructed from imputed schooling using the formula Min[Max(Years of schooling -12,0), 4], as in Bound and Turner (2002).

sequence numbers (RSNs), were randomly assigned to all dates of birth in the relevant cohorts.

Each lottery was associated with a draft-eligibility ceiling or cut-off. Men with an RSN below the ceiling were draft-eligible while men with an RSN above the ceiling were draft-exempt. Draft-eligibility ceilings were 195 in the 1970 lottery, 125 in the 1971 lottery and 95 in the 1972 lottery. Draft eligibility is 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 through an occupational or educational deferment, or because of 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 is .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 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, with 1953 "draft-eligibility" coded using the 1972 lottery cutoff of 95). Because the effect on men born in 1953 is small, we omit this cohort from the main empirical analysis. 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 relationship 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 probabilities smoothed across 5-RSN cells by single year of birth, but the smoothing does not straddle the draft-eligibility cutoff in each cohort.[3] Like Table 2, the figure 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

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

could expect more choice regarding terms of service (e.g., choice of branch of service), while draftees mostly served in the Army. Another important feature of Figure 1 is the muted relationship between veteran status and lottery numbers for nonwhites. Angrist (1991) argues that this pattern reflects the fact that nonwhites were more likely than whites to consider military service an attractive career option. An econometric consequence of the weaker first stage for nonwhites is the relative imprecision of 2SLS using draft lottery estimates for this group.

Expanded Instrument Sets

Motivated by Figure 1, we constructed instruments from a set of five lottery-group dummies. These were 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 for individual i is $\{z_{1i}, z_{2i}, z_{3i}, z_{4i}, z_{5i}\}$ where

$$\begin{aligned} z_{1i} &= I[RSN_i \leq 95], \\ z_{2i} &= I[95 < RSN_i \leq 125], \\ z_{3i} &= I[125 < RSN_i \leq 160], \\ z_{4i} &= I[160 < RSN_i \leq 195], \\ z_{5i} &= I[195 < RSN_i \leq 230], \end{aligned}$$

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

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

where YOB_i is i's year of birth. This shows that $elig_i$ is a function of both lottery-number main effects and interactions with year of birth.

The first two columns in Table 2 report estimates of the $5z$ first stage in pooled samples.[4] 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

[4]The estimates in Table 2 and the second-stage estimates that follow control for year of birth, state of birth, and month of birth.

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 .0067 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 138 for nonwhites to over 2403 for whites.

As it turns out, the $5z$ instrument set does not produce more precise 2SLS estimates than $elig_i$ alone, though partial F-statistics measuring the relative contribution of $5z$ in a first-stage that includes $elig_i$ are highly significant (e.g., $F = 91$ for whites in the 1950-52 sample). We therefore report estimates using an instrument set, labeled $5zx$, that interacts $5z$ with year of birth. The $5zx$ set includes 15 instruments for the 1950-52 sample and 25 instruments for the 1948-52 sample. The $5zx$ first stage for the 1950-52 sample appears in columns (5)-(7) of Table 2. This first stage documents a modest role for draft-motivated enlistment. 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. Partial F-statistics for the marginal contribution of $5zx$ in a model that includes $5z$ are on the order of 150 for whites and 10 for nonwhites.

II Schooling and the Vietnam-era GI Bill

A Empirical Framework

The results reported here are 2SLS (and OLS) estimates of parameter β in the equation

$$Y_i = W_i'\alpha + \beta VET_i + u_i, \tag{1}$$

where Y_i is an outcome variable for individual i, the dummy variable VET_i indicates veteran status, and W_i is a vector of covariates that includes year of birth dummies, month of birth dummies, and state of birth dummies. 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 an RSN 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 are also reported.[5]

The 2SLS estimates in (1) capture the effect of military service for draft-lottery compliers, a group that includes men who were drafted and those who volunteered in the face of draft risk. The average causal effect for compliers is the local average treatment effect (LATE) generated by draft-lottery instruments (Guido Imbens and Angrist, 1994). The assumptions required for a LATE interpretation of draft-lottery estimates are that (a) draft lottery numbers are independent of potential outcomes in the treated and non-treated state, and (b) draft-eligibility can only make military service more likely for any given individual.

The independence assumption is supported in part by random assignment: draft lottery numbers are uncorrelated with ability or family background. The causal interpretation of our 2SLS estimates also turns on an exclusion restriction which states that the only channel by which draft lottery numbers affected outcomes is military service. Effects of military service on schooling do not necessarily signal a violation of the exclusion restriction if any extra schooling caused by draft-eligibility is itself a consequence of military service (e.g., via the GI Bill). But we might worry that schooling effects reflect draft-avoidance behavior (via student deferments) and not military service *per se*. We argue below, however, that student deferments were probably of little importance for the draft-lottery cohorts.

It's noteworthy that most soldiers who served in the draft-lottery period were not compliers; rather, they were true volunteers who were not drafted and did not volunteer simply to avoid conscription.[6] Estimates using draft-lottery instruments need not gen-

[5] A potential problem with the second-stage estimates reported here is selection bias in the sample that survives to the 2000 Census interview date. In a pioneering study of draft-lottery effects, Norman Hearst, Thomas Newman, and Stephen Hulley (1986) found excess mortality among draft-eligible men from California and Pennsylvania. More recently, however, Conley Dalton and Jennifer A. Heerwig (2009) find no excess mortality in a larger national sample of death records. In an investigation reported in the appendix to Angrist, Chen, and Frandsen (2010), we also find little evidence of elevated mortality among draft-eligible men in the 2000 Census. It therefore seems likely that any selection bias due to differential mortality is modest.

[6] The proportion of veterans who were 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 $\{v_{1i}, v_{0i}\}$. Veteran status is $v_i = v_{0i} + elig_i(v_{1i} - v_{0i})$ and compliers have $v_{1i} - v_{0i} = 1$. Given monotonicity, $v_{1i} \geq v_{0i}$, so the proportion of 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 $E[v_{1i} - v_{0i}|v_i = 1] = P[v_i = 1|v_{1i} - v_{0i} =$

eralize to the population of true volunteers. Nevertheless, the effects of military service on men compelled to serve against their will reflect the consequences of conscription in the Vietnam period, a historical legacy that continues to be debated and is relevant for contemporary veterans compensation policies, especially as compensation costs for Vietnam veterans have continued to grow (e.g., Mark Duggan, Robert Rosenheck, and Perry Singleton, 2010; Joseph Stiglitz and Linda Bilmes, 2008). Moreover, draft lottery estimates may also be relevant for contemporary discussions of military manpower policy, since compliers under any future draft are likely to be similar to those from the draft-lottery period. The Selective Service System web site states that "if a draft were held today," it would involve a lottery over 19-year olds and there would be few deferments as in the lottery years beginning in 1971 (See http://www.sss.gov/viet.htm). Conscripts would, by definition, be men who were otherwise unlikely to volunteer. It also seems likely that a future draft would come in wartime (the possibility of a new draft was raised by politicians and commentators in the run-up to the Iraq War).

B Schooling Estimates

Compulsory military service appears to have increased the educational attainment of white Vietnam-era veterans, a result documented in Table 3. The 2SLS estimates using $elig_i$ in the 1950-52 sample suggest that white veterans got .332 more years of schooling than nonveterans. The corresponding results are slightly lower in the 1948-52 sample, and change little when estimated with an expanded instrument set ($5zx$). Both samples generate precise estimates with standard errors of about .05. The precision gains from estimation with the larger $5zx$ instrument set are exceedingly modest. In contrast to the results for whites, the estimates for nonwhites are smaller and not significantly different from zero, but also relatively imprecise. Given this imprecision, results for nonwhites are reported in an online appendix.

The remainder of Table 3 shows that the increase in years of schooling for white veterans results primarily from more years of college, with precisely estimated effects ranging from .24-.27. Specifically, military service is estimated to have increased the likelihood a veteran attended college or earned an Associate's degree by 6 to 9 percentage points . The increase in the likelihood of completing a BA is smaller though still marked,

$1]P[v_{1i} - v_{0i} = 1]/P[v_i = 1] = P[elig_i = 1]P[v_{1i} - v_{0i} = 1]/P[v_i = 1]$. For white men born 1950-52, this is .376(.145/.236) = .231.

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.[7]

C GI Bill Benefits vs. Draft Avoidance

The schooling shifts documented in Table 3 are most likely a consequence of the Vietnam-era GI Bill, which offered stipends similar in generosity to those available to veterans of WWII and Korea.[8] Vietnam veterans were especially likely to have used the GI Bill for education and training. Data from the 2001 Survey of Veterans (SOV) show that among whites, 44 and 42 percent of WWII and Korean-era veterans used benefits for education and training, while the usage rate was 50 percent for Vietnam-era veterans. Vietnam-era veterans were also more likely than earlier cohorts to have used their benefits for college course work: 63 percent of Vietnam-era GI Bill beneficiaries used benefits for college courses, while the corresponding figures for WWII and Korean-era benefit users are 53 and 56 percent.[9]

The notion that the GI Bill increased schooling is supported by a number of earlier studies. For example, Bound and Turner's (2002) IV estimates of the effects of WWII service on college completion by white men are around 5-6 percentage points while their estimates of effects on years of college range from .23-.28. Stanley's (2003) estimates of the effects of the Korean-era GI Bill eligibility on college completion are also on the order of 5-6 percentage points and his estimates of effects on years of college range from .20-.33. The college completion effects reported in Table 3 are a little over 5 percentage points and

[7] Angrist and Krueger (1992) found a mostly insignificant relation between lottery numbers and education using data from the 1979-85 CPS's. But these results are too imprecise to detect effects on schooling of the size reported here. Moreover, some of the Vietnam veteran schooling advantage seems to have accumulated after Angrist and Krueger's (1992) sample period.

[8] The WWII GI Bill included a $500 tuition benefit and a monthly stipend. In the 1970s, the Vietnam-era GI Bill paid full-time students a stipend almost identical in value to the WWII package (adjusting for inflation) and more generous than the Korean-era full-time stipend. These benefit levels were almost double the average cost of tuition, room, and board at 4-year public universities in this period. The real value of the Vietnam-era GI Bill declined in the 1980s, but remained above the cost of tuition, room, and board (Data from authors' tabulations and Bound and Turner, 2002).

[9] The pattern for nonwhite veterans is similar, though the levels are lower. GI Bill statistics in this paragraph are from the authors' tabulation of responses to the 2001 SOV. For purposes of this comparison, 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).

range from .24 to .27 for years of college, remarkably similar to the estimates reported in these earlier studies. Our estimates are also similar to those reported by Turner and Bound (2003) in that they show larger effects of the GI Bill on whites than nonwhites (see the appendix tables for results for nonwhites). Finally, Lemieux and Card (2001) report effects of a similar magnitude in cohorts that benefitted from the Canadian GI Bill, while Angrist (1993) estimated large post-service schooling increases due to use of the Vietnam-era GI Bill.[10]

The leading alternative explanation for schooling effects estimated using draft-lottery instruments is draft-avoidance 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 sharp decline in educational deferments during the draft-lottery period. President Nixon announced a college-deferment phase-out in April 1970. In 1971 new deferments ended, and existing deferments were extended only one term or to graduation for seniors. The declining importance of college deferments is reflected in the cohort- and sex-specific enrollment rates analyzed by Card and Lemieux (2001). Their analysis shows relatively little deviation from trend in the male-to-female college graduate ratio or the proportion with some college in cohorts born after 1950.[11]

Estimates of schooling effects for whites by single year of birth, reported in Table 4, also weigh against draft deferment as the primary force behind the schooling effects in Table 3. In particular, Table 4 shows that in spite of the decreasing availability of college deferments from 1970 onwards, the estimated effects on years of schooling and years of college are substantial for white men born in 1951 and 1952. The largest effects of military service on these two schooling variables are for men in the 1951 cohort, few of whom would have been deferred for long. Estimates of effects on years of schooling and on years of

[10] The BEOG program (Pell grants) also played an important role in expanding college attendance for adult students in the 1970s (see, e.g., Neil S. Seftor and Turner 2002), but Vietnam veterans were not especially likely to have received Pell grants. Among male Vietnam veterans aged 35-39 in the SOV of 1987 (roughly the cohorts of the 2000 Census), 54 percent had used the GI Bill, while only 7.4 percent reported having received any federal (non-Veteran) aid, and only 2.3 percent received federal grants (including Pell grants). The overlap with Pell grants is small because Pell was means-tested while the GI Bill was not and because half of the GI Bill benefit amount was counted as income when determining Pell grant eligibility (U.S. Congressional Budget Office 1978, p.24).

[11] For institutional background related to draft deferments, see the chronology in Selective Service System Office of Public Affairs (1986) and Semiannual Reports of the Director of the Selective Service System from the early 1970s.

college for the 1952 cohort (which had no access to college deferments) are smaller than those for the 1951 cohort, but similar or larger in magnitude than the estimates for white men born from 1948 to 1950.

Differences across cohorts in the 2SLS estimates of effects on some-college dummies mirror the cohort pattern for on years of schooling and years of college. As shown in Table 4, the estimated effect on an indicator for one or more years of college falls from .105 for the 1951 cohort to .068 for the 1952 cohort. On the other hand, the BA effect is larger for the 1952 cohort than for the 1950 cohort, in spite of the younger cohort's loss of access to college deferments. It's also worth noting that the estimates by single year of birth for nonwhites, reported in Online Table 2, though imprecise, are typically larger for younger cohorts. On balance, therefore, our results point away from draft deferment as the primary explanation for the estimates in Table 3.[12]

III Labor Market Effects

A Effects on Work and Earnings

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 Table 5, which reports estimates of effects on labor market status and earnings using different instrument sets. Using draft eligibility as an instrument for conscription, 2SLS estimation in the sample of white men born 1950-52 generates effects 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 also small and insignificant. Estimates of effects on log weekly wages, computed for the sample of men with positive earnings, are similarly small. In contrast, the OLS estimates in columns 2 and 6 show that veteran status is associated with worse labor market outcomes and lower employment rates. The OLS estimates, about -7,900 to -8,600 for annual earnings and -11 percent to -12 percent of weekly wages, are outside the 2SLS confidence intervals.[13]

As in Table 3, we report IV estimates using the $5zx$ instrument set (5 lottery-number

[12]See also the figures in our working paper (Angrist and Chen, 2008), which uses the 1972-89 CPS to show that although they started out behind nonveterans, veterans experienced substantial post-service schooling gains relative to nonveterans beginning a few years after they were likely to have been discharged.

[13]The corresponding results for nonwhite labor market outcomes, not shown here but reported online, are inconclusive.

group dummies with a full set of year-of-birth interactions) as well as draft-eligibility instruments. Again, the $5zx$ instrument set produces estimates that are only slightly more precise than those using $elig_i$ alone. The clearest precision gains appear in the 1948-52 sample. In Table 5, the standard error for the estimated effect on earnings in the sample of whites born 1948-52 falls from 1243 to 1133. For the estimated effect on log weekly wage, the change in standard errors is small (from .16 to .15).

As a partial check on the underlying identifying assumptions, we computed over-identification test statistics for the results in Tables 3, 5, and 7 (these are not reported in the tables). All test statistics come out with p-values of at least .4. In the LATE framework, the over-identification test is as much an exploration of treatment effect heterogeneity from one instrument set to another as a test of instrument validity. These test results therefore suggest that the treatment effects identified by changes in draft-eligibility are statistically indistinguishable from treatment effects identified by changes in draft-motivated enlistment on either side of the eligibility cutoff. Conditional on a constant causal effect, we can also take high over-identification p-values as empirical support for the underlying exclusion restrictions that motivate draft-lottery instruments.

B Reconciling Schooling and Earnings Effects

The small earnings effects in Table 5 contrast with the substantial earnings losses reported for white veterans in Angrist (1990). The latter ranges from 10 percent to 15 percent of FICA-taxable earnings in 1981-84. In this section, we argue that results from the 2000 Census can be reconciled with earlier results showing losses if the costs of conscription are due primarily to lost labor-market experience. By 2000, the draft lottery cohorts had reached middle age, when experience profiles are fairly flat, so the veteran penalty should have faded to a level that is roughly offset by the earnings gains due to GI Bill-funded schooling.[14]

To formalize this claim, let y_i denote the log weekly wage of individual i in the draft lottery cohorts, s_i his years of schooling, and x_i his potential work experience. A Mincer-

[14] An alternative explanation for Vietnam-era-related earnings losses is the mid-1970s recession. Lisa B. Kahn (2010), Phil Oreopoulos, Till von Wachter, and Andrew Heisz (2010), and Molly Dalh, Thomas DeLeire, and Jonathan Schwabish (2010) find that workers who transition from school to work in a down labor market suffer an earnings penalty. Weighing against this interpretation of service-related earnings losses is the fact that the first draft-lottery cohorts entered the civilian labor market during relatively good times, while the draft-lottery estimates of earnings losses reported in Angrist (1990) are remarkably consistent across cohorts.

style human capital earnings equation is

$$y_i = \beta_0 + \beta_1 x_i + \beta_2 x_i^2 + \rho s_i + u_i, \tag{2}$$

where u_i is a residual that captures random variation in the earnings function across individuals. Although stylized, equation (2) is a workhorse of empirical labor economics that has repeatedly been found to describe essential features of the relationship between schooling, experience, and earnings.[15]

To model veteran effects in the Mincer framework, we write years of schooling (s_i) and potential work experience (x_i) as:

$$\begin{aligned} s_i &= s_{0i} + \delta VET_i, \\ x_i &= age_i - s_i - 6 - \ell VET_i = x_{0i} - (\delta + \ell) VET_i, \end{aligned} \tag{3}$$

where s_{0i} and $x_{0i} \equiv age_i - s_{0i} - 6$ denote i's potential schooling and work experience if he doesn't serve. We expect ℓ to be about two years for Vietnam-era draftees. Volunteers usually served longer, but most of the men who were compelled to serve by the draft lottery did so as conscripts. In addition, the estimated schooling gain reported in Table 3 suggests that δ ranges from 0.294 to 0.336 (with a standard error of about 0.053). Taking account of the effect of military service on schooling, Vietnam veterans lose $(\delta + \ell)$ years of experience relative to nonveterans.

The Mincer equation generates veteran status interactions with x_{0i}, the level of potential work experience in the absence of military service. Specifically, re-arranging equation (2) gives:

$$y_i = \beta_0 + \beta_1 x_{0i} + \beta_2 x_{0i}^2 + \rho s_{0i} + \pi_{xi} VET_i + u_i,$$

where $\pi_{xi} \equiv \pi_0 + \pi_1 x_{0i}$ and

$$\pi_0 \equiv -[\beta_1(\delta + \ell) - \beta_2(\delta + \ell)^2] + \rho\delta, \tag{4}$$

$$\pi_1 \equiv -2\beta_2(\delta + \ell). \tag{5}$$

[15] A number of studies evaluate the functional form assumptions of the simple Mincer equation. Two landmark contributions are Kevin M. Murphy and Finis Welch (1990), which focuses on the shape of the experience profile, and James J. Heckman, Lance Lochner, and Petra E. Todd (2005), which explores the robustness of schooling returns in the Mincer model. Although this work shows the traditional Mincer equation can be improved upon, the strong assumptions of the traditional Mincer model appear to matter little for our purposes. This is probably because our sample is limited to middle-aged men and because the changes in experience and schooling induced by military services are small enough for linearity to be a reasonable approximation.

The veteran main effect, π_0, reflects changes in labor market experience due to lost experience while in the military or in school, plus a term, $\rho\delta$, that captures the economic return to the service-induced schooling increment. The results in Table 3 suggest that the extra schooling fueled by the GI Bill comes to about 0.3 years. Assuming, as the results reported below suggest, that the return to one additional year of schooling (ρ) is roughly 7 percent, the GI Bill adds $\rho\delta$ or about 2 percent to veterans' earnings.

The draft lottery cohorts in the 2000 census are of an age where experience profiles are nearly flat and well-approximated by a linear profile ($\beta_2 \simeq 0$). Thus, the veteran/experience interaction term, π_1, is negligible. It therefore seems reasonable to think of reduced-form veteran effects in 2000 data as estimates of

$$\pi_{xi} \simeq -\beta_1(\delta + \ell) + \rho\delta. \tag{6}$$

As it turns out, the experience profile in our data is such that given a 2-year loss of experience, the two terms in (6) above are virtually off-setting, adding up to a net veteran effect of about zero.

OLS and IV estimates of equation (2) are reported in Panel A of Table 6 for the sample of white men born 1948-52. The instruments for schooling and potential experience consist of a draft-eligibility dummy and an age quadratic or a full set of year-of-birth dummies. 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. We focus on whites because the estimated impact of military service on the schooling of nonwhites is smaller and not significantly different from zero. As a benchmark, column (1) reports OLS estimates treating all variables as exogenous. With potential experience defined as in equation (3), the estimated returns to schooling are about .12. The estimated experience profile in this case does not have the usual concavity because the profile in this age range is fairly flat (the experience derivative is small, about .009 with s.e.=.001).

Instrumental-variables estimates of the return to schooling are markedly smaller than the corresponding OLS estimates. This can be seen in columns (2)-(4) of Table 6, which report 2SLS and limited information maximum likelihood (LIML) estimates of equation (2). As shown in column (2), estimates from a just-identified model using age_i, age_i^2 and draft-eligibility ($elig_i$) as instruments for the three endogenous variables, x_i, x_i^2 and s_i, generate a return of .068 (s.e.= .029). Swapping year-of-birth dummies for age_i and age_i^2 generates a 2SLS estimate of .076 (s.e.=.029), reported in column (3). The first-stage F-statistic for schooling, calculated in a manner that takes account of multiple endogenous variables, is about 16 or more. This is outside the range where bias in 2SLS estimates is usually a concern.[16] The LIML estimates in column (4) are close to the corresponding 2SLS estimates in column (3), not surprisingly since the degree of over-identification for this model is only two. The experience derivative is about 0.009 for OLS and 0.007 for both 2SLS and LIML, whether estimated using a quadratic or linear profile. The fact that this implies a rough 2 percent earnings loss is the basis for our claim that the earnings and schooling effects combined in (6) add up to something close to zero.

The robustness of the finding of a small IV (i.e., below OLS) estimate of the returns to schooling is also of interest. One obvious explanation for this result is positive ability bias in the OLS estimates, but it's worth noting many other IV estimates of the returns to schooling have come out larger than OLS (see, e.g., the estimates summarized in Card [1995]). A simple economic model with heterogeneous effects can be used to see why the returns to college attendance for GI Bill users in particular might be below the average return for all men who have attended college (see our working paper [Angrist and Chen, 2008] for details). At the same time, the GI Bill, which we see as the main force driving changes in schooling due to veteran status, affects post-secondary schooling but has little to do with either primary or secondary schooling (an institutional fact that is reflected in our estimates). Differences in OLS and IV estimates might therefore be driven by nonlinearity if the returns to college differ from the returns to high school. We can allow for some degree of nonlinearity in returns by treating years of primary and years of secondary schooling as exogenous covariates, while instrumenting years of college in a two-stage least squares (2SLS) procedure based on (2).

[16] The multivariate first-stage F statistic 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 is Z, with projection matrix $P_z = Z(Z'Z)^{-1}Z'$. The second stage equation is

$$y = P_z W_1 \delta_1 + P_z W_2 \delta_2 + [\epsilon + (W_1 - P_z W_1)\delta_1 + (W_2 - P_z W_2)\delta_2],$$

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 $M_{1z} = [I - P_z W_1 (W_1' P_z W_1)^{-1} W_1' P_z]$. 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

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

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

Estimates of the returns to college, reported in Panel B of Table 6, are somewhat higher than the returns estimated in the simpler years-of-schooling model. The OLS estimate increases to 0.13, while the corresponding 2SLS estimates range from 0.076 to 0.089 depending on the instrument list and whether the experience profile is taken to be linear or quadratic. Importantly, however, a substantial gap between the OLS and 2SLS estimates remains even after focusing on the returns to a college-specific schooling increment. Our working paper (Angrist and Chen, 2008) shows that the findings in Table 6 are robust to other changes in the functional form of the experience profile beyond the linear and quadratic results reported in the table. It's also noteworthy that adjusting for possible omitted health effects of Vietnam-era service has little impact on the overall picture in this context.

IV Other Consequences of Vietnam-Era Service

To round out our investigation of the long-term effects of Vietnam-era conscription, we look briefly at other outcomes. This investigation complements our primary focus on experience and earnings. We show here that while veterans are more likely to work in the public sector and to have moved since childhood, there is little evidence of marriage effects.

The federal government and many state and local governments have a policy of preferring veterans over similarly qualified non-veteran applicants (federal preferences are explained at the US Office of Personnel Management's website http://fedshirvets.gov). The Vietnam-Era Veterans' Readjustment Assistance Act of 1974 also requires that employers with Federal contracts or subcontracts of $25,000 or more "take affirmative action to employ and advance in employment" veterans of the Vietnam-era, among other veteran groups. Consistent with government hiring policies, especially at the federal level, the 2SLS estimates reported in Table 7 show that veterans in the draft-lottery cohorts born in 1950-52 are 5-6 percentage points more likely to work in the public sector, and that this effect is due almost entirely to an increased likelihood of working for the federal government. The corresponding set of OLS estimates understates the impact of military service on public-sector employment, though not dramatically.

Also shown in Table 7, 2SLS estimates of effects on mobility indicate a 3 percentage points decrease in the probability of living in one's state of birth. In contrast, OLS estimates exaggerate the negative impact of military service on mobility by 1-2 percentage points - apparently veterans are somewhat more mobile than nonveterans even in the absence of military service. The modest effects on mobility reported here seem likely due to the fact that the military often relocates soldiers who serve in the US armed forces, though this effect might also be driven by relocation for college, as Ofer Malamud and Abigail Wozniak (2008) suggest.

Finally, the absence of an effect on marital status is documented at the bottom of Table 7. Although OLS estimates show white veterans are more likely to have be married ever or currently, the corresponding 2SLS estimates are statistically insignificant and small (no more than one percentage point). To sum up, government hiring preferences and state-of-birth mobility effects, while identifiable in and of themselves, do not seem important enough to have changed employment or earnings. The lack of a marriage effect weighs against the view that Vietnam-era service contributed to social pathology among Vietnam veterans.[17] We therefore see these findings as supportive of our view that experience and earnings are the primary channels by which military service affected Vietnam veterans' long-term economic position.

V Summary and Conclusions

Draft-lottery IV estimates using data from the 2000 Census show a strong positive connection between schooling and military service. This schooling gain is very likely due to the Vietnam-era GI Bill. Overall, the schooling effects estimated here are similar to those reported in earlier evaluations of the impact of the WWII and Korean-era GI Bills by Bound and Turner (2002) and Stanley (2003). In this case, however, we have the advantage of quasi-experimental random assignment via the draft lottery and evidence from the equally generous but less-studied Vietnam-era GI Bill. Interestingly, the results reported here are also broadly consistent with Norman Frederiksen and William B. Schrader's (1951) pioneering investigation of the impact of the WWII GI Bill in the immediate post-war period. This study surveyed enrolled veterans in an attempt to determine how many would not have gone to college but for the GI Bill. The GI Bill was found to be important but not revolutionary: while many veterans cited the GI Bill as key to their decision to attend college, 60 percent reported that they definitely would have gone to

[17]For contradictory evidence, see Chris Rohlfs (2010) and Jason M. Lindo and Charles Stoecker (2010), who use draft lottery instruments to explore the link between Vietnam-era service and crime. These studies use smaller and arguably more idiosyncratic data sets than the large Census files analyzed here.

college without GI Bill funding.

Our results also suggest that the Vietnam veteran earnings penalty has largely faded. Seen through the lens of a Mincer-style wage equation, the near-zero veteran wage penalty can be explained by the combination of lost experience on a flat portion of the experience profile and the economic return to additional schooling funded by the GI Bill. IV estimates from a variety of specifications point to an annualized return to schooling on the order of .07, with somewhat larger estimates coming out of models that allow for nonlinearities in the earnings function. Although not precise enough to be statistically different from the OLS estimates (as is common for IV estimates), the IV estimates are consistently below the corresponding OLS estimates in all specifications, a finding that differs from many previous IV estimates. As conjectured by Mark C. Berger and Barry T. Hirsch (1983), a simple economic explanation for low returns to schooling among veterans is the large subsidy to schooling provided by the GI Bill.

Other effects of Vietnam-era service seem unremarkable. White Vietnam veterans are less likely to live in their state of birth, a result that is probably due to the forced mobility that is part of the American military experience. White Vietnam veterans also appear to enjoy an increased probability of employment in the public sector, especially in the federal government. But public sector hiring preferences did not translate into earnings or employment gains, at least not into gains large enough to offset other factors such as lost experience. We found no evidence that Vietnam-era service affected veterans' marriage rates.

A final observation regarding the long-term consequences of Vietnam-era military service seems in order. Although the earnings penalty for white Vietnam veterans has largely disappeared, and veterans ultimately benefitted from their service as far as schooling goes, the lifetime earnings consequences of conscription for white Vietnam veterans have almost surely been negative. To substantiate this claim, we added the (percentage) earnings loss due to lost experience reported in Angrist (1990) to the earnings gain attributable to the schooling differential estimated here. We then applied returns and losses to the annual earnings of high school graduates in the CPS and calculated the present discounted value over the period 1972-2000. From the point of view of lottery-cohort soldiers discharged at age 21, the present value of lost earnings amounts to about 10 percent of earnings through the year 2000, so that even after accounting for GI Bill benefits, conscription reduced veterans lifetime earnings. The GI Bill made this loss about 15 percent smaller than it otherwise would have been, but it did not come close to offsetting the full costs of conscription suffered by draftees.

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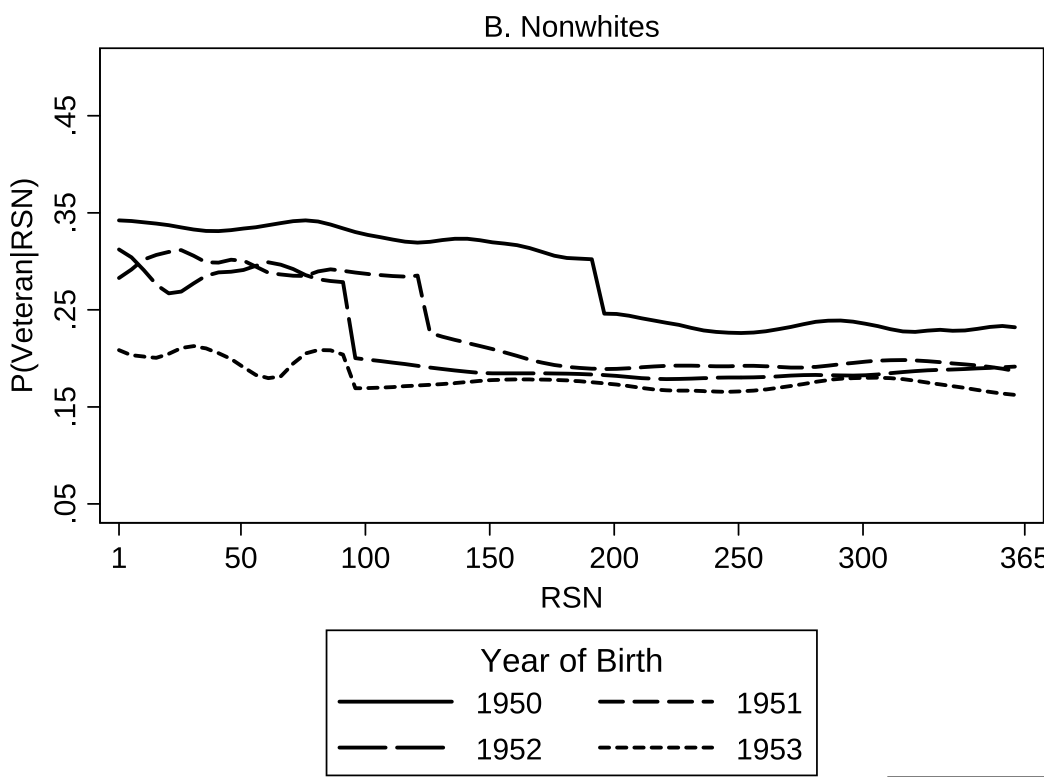
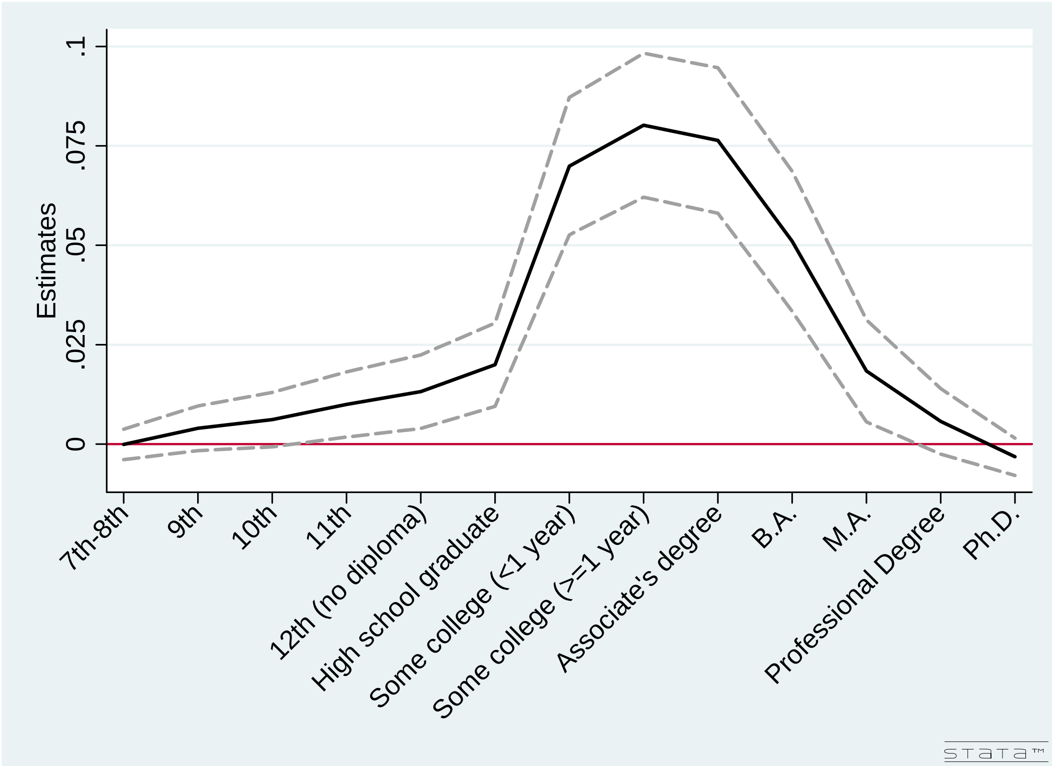
A. Whites
P(Veteran|RSN)
.05
.15
.25
.35
.45
1
50
100
150
200
250
300
365
RSN
Year of Birth
1950
1951
1952
1953



B. Nonwhites
P(Veteran|RSN)
.05
.15
.25
.35
.45
1
50
100
150
200
250
300
365
RSN
Year of Birth
1950
1951
1952
1953



Estimates
.1
.075
.05
.025
0
7th-8th
9th
10th
11th
12th (no diploma)
High school graduate
Some college (<1 year)
Some college (>=1 year)
Associate's degree
B.A.
M.A.
Professional Degree
Ph.D.

Table 1. Descriptive Statistics for 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
Age	48.2	48.4	48.2	48.2	48.3	48.2
A. Education variables						
Imputed highest grade completed	13.8	13.4	13.9	12.6	13.0	12.4
Years of college (0-4)	1.76	1.36	1.88	1.05	1.14	1.010
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
B. 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
Self employed	.156	.114	.169	.067	.056	.071
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
Supplemental Security Income >0	.017	.013	.018	.044	.029	.049
Social security income >0	.033	.036	.032	.057	.053	.058
C. Other outcomes						
Works in public sector	.153	.184	.144	.198	.257	.178
Works in federal government	.042	.077	.031	.062	.118	.044
Lives in state of birth	.602	.560	.615	.639	.586	.656
Currently single	.095	.064	.104	.176	.123	.193
Currently married	.725	.726	.725	.557	.580	.549
Ever married	.905	.936	.896	.824	.877	.807
N	696530	166652	529878	96217	23246	72971

Notes: The table shows sample means from the 2000 Census, 1:6 file, weighted by census sampling weights.

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)	(.0025)	(.0024)	(.0023)	(.0022)	(.0024)	(.0020)
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	.0085	.00002
	(.0020)	(.0017)	(.0045)	(.0043)	(.0041)	(.0036)	(.0031)	(.0029)
RSN 161-195	.040	.044	.044	.054	.102	.024	-.0012	.0017
	(.0020)	(.0017)	(.0044)	(.0043)	(.0041)	(.0034)	(.0030)	(.0029)
RSN 196-230	.0067	.0061	.0043	.0062	.013	-.0012	.0087	.0008
	(.0019)	(.0017)	(.0043)	(.0042)	(.0038)	(.0032)	(.0031)	(.0029)
F-statistics	2403	2295	111	202	731	861	1029	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)	(.012)	(.011)	(.0095)	(.0093)
RSN 126-160	.044	.041	.027	.042	.093	.034	.0054	.0018
	(.0057)	(.0047)	(.012)	(.012)	(.011)	(.010)	(.0092)	(.0086)
RSN 161-195	.022	.021	.012	.027	.066	-.0047	.0056	.0023
	(.0055)	(.0046)	(.012)	(.011)	(.010)	.0092	(.0092)	(.0087)
RSN 196-230	-.0031	.0008	-.004	.018	.008	-.010	-.0049	.0021
	(.0054)	(.0046)	(.012)	(.011)	(.010)	.0093	(.0089)	(.0090)
F-statistics	138	134	4.98	14.3	48.9	55.1	47.3	4.51

Notes: The table reports draft-eligibility effects and RSN group effects estimated in separate regressions for each column. Robust standard errors are reported in parentheses. All models include a full set of dummies for year of birth, state of birth, and month of birth. The estimates were constructed using census sampling weights.

Table 3: Veteran Effects on Education for Whites

	1950-52				1948-52			
			2SLS				2SLS	
	Mean	OLS	elig	5zx	Mean	OLS	elig	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of schooling (imputed)	13.8	-.551	.332	.336	13.8	-.550	.294	.314
		(.0074)	(.053)	(.052)		(.0057)	(.053)	(.049)
Years of college	1.73	-.512	.265	.261	1.76	-.534	.240	.248
		(.0050)	(.035)	(.034)		(.0038)	(.034)	(.032)
9th grade +	.977	.015	.0056	.0061	.975	.020	.0021	.0040
		(.0004)	(.0031)	(.0030)		(.0003)	(.0031)	(.0028)
10th grade +	.965	.018	.0080	.0083	.963	.025	.0042	.0061
		(.0005)	(.0037)	(.0036)		(.0004)	(.0038)	(.0034)
11th grade +	.948	.021	.012	.013	.946	.029	.0071	.010
		(.0007)	(.0045)	(.0044)		(.0005)	(.0045)	(.0041)
12th grade (no diploma) +	.931	.024	.015	.016	.930	.033	.009	.013
		(.0008)	(.0051)	(.0049)		(.0006)	(.0050)	(.0046)
High school graduate or higher +	.910	.025	.023	.023	.908	.034	.017	.020
		(.0009)	(.0057)	(.0056)		(.0006)	(.0057)	(.0052)
Some college (less than 1 year) +	.655	-.050	.079	.079	.659	-.049	.064	.070
		(.0015)	(.009)	(.0093)		(.0011)	(.0094)	(.0086)
1 or more years of college (no degree) +	.582	-.082	.090	.089	.588	-.083	.074	.080
		(.0016)	(.010)	(.010)		(.0012)	(.010)	(.0090)
Associate's degree +	.411	-.126	.081	.079	.419	-.133	.074	.076
		(.0015)	(.010)	(.010)		(.0011)	(.010)	(.0091)
Bachelor's degree +	.333	-.168	.053	.051	.341	-.176	.051	.051
		(.0014)	(.010)	(.0094)		(.0010)	(.010)	(.0088)
Master's degree +	.135	-.082	.016	.017	.140	-.090	.019	.018
		(.0009)	(.0070)	(.0068)		(.0007)	(.0070)	(.0064)
Professional degree+	.051	-.043	.0047	.0037	.052	-.046	.010	.0057
		(.0005)	(.0045)	(.0044)		(.0004)	(.0045)	(.0041)

Note: All regressions include a full set of dummies for state of birth, year of birth and month of birth. Columns 3-4 and 7-8 report 2SLS estimates with the instrument sets listed. Robust standard errors are reported in parentheses. Estimates were computed using census sampling weights. A corresponding set of results for nonwhites is available online.

Table 4: Veteran Effects on Education by Year of Birth for Whites

	1948 (1)	1949 (2)	1950 (3)	1951 (4)	1952 (5)
Years of schooling (imputed)	.179	.122	.254	.460	.321
	(.232)	(.173)	(.099)	(.093)	(.085)
Years of college	.045	.188	.232	.357	.218
	(.146)	(.111)	(.063)	(.061)	(.054)
1 or more years of college (no degree) +	.005	.019	.088	.105	.068
	(.041)	(.031)	(.018)	(.017)	(.016)
Associate's degree +	.004	.080	.072	.102	.067
	(.042)	(.032)	(.018)	(.018)	(.016)
Bachelor's degree +	.015	.061	.038	.075	.044
	(.041)	(.031)	(.018)	(.017)	(.015)
Master's degree +	.030	.021	-.004	.029	.024
	(.031)	(.023)	(.013)	(.012)	(.011)

Note: The table reports 2SLS estimates of schooling effects by single year of birth, constructed 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 are reported in parentheses. Estimates were computed using census sampling weights. A corresponding set of results for nonwhites is available online.

Table 5. Veteran Effects on Labor Market Outcomes for Whites

	1950-52				1948-52			
			2SLS				2SLS	
	Mean	OLS	elig	5zx	Mean	OLS	elig	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Employment	.861	-.020	-.0043	-.0026	.855	-.010	-.0047	-.0033
		(.0012)	(.0072)	(.0070)		(.0009)	(.0072)	(.0066)
Unemployment	.027	.0043	.0028	.0017	.027	.0028	.0022	.0014
		(.0005)	(.0033)	(.0032)		(.0004)	(.0033)	(.0030)
Not in labor force	.112	.016	.0014	.0009	.118	.0074	.0025	.0018
		(.0011)	(.0066)	(.0064)		(.0008)	(.0066)	(.0060)
Self employed	.156	-.055	-.007	-.005	.156	-.053	.0002	-.002
		(.001)	(.007)	(.007)		(.001)	(.007)	(.007)
Usual hours worked	41.5	-.888	-.101	-.230	41.2	-.544	.055	-.136
		(.054)	(.334)	(.325)		(.040)	(.335)	(.305)
Weeks worked	44.8	-.752	-.133	-.192	44.5	-.243	-.120	-.175
		(.054)	(.330)	(.321)		(.040)	(.331)	(.301)
Wage and salary income	46406	-8616	-517	-873	46595	-7936	-115	-546
		(161)	(1240)	(1209)		(128)	(1243)	(1133)
Log weekly wage	6.75	-.121	-.0038	-.0094	6.75	-.110	.009	-.0030
		(.0026)	(.016)	(.016)		(.0019)	(.016)	(.015)
Self employment income	5261	-2772	855	867	5285	-2846	487	670
		(77.8)	(616)	(606)		(62.3)	(616)	(567)

Note: All regressions include a full set of dummies for state of birth, year of birth and month of birth. Columns 3-4 and 7-8 report 2SLS estimates with the instrument sets listed. Robust standard errors are reported in parentheses. Estimates were computed using census sampling weights. A corresponding set of results for nonwhites is available online.

Table 6. Schooling, Experience, and Earnings (White Men Born 1948-52)

	Quadratic experience effect				Linear experience effect			
		Elig+age	Elig+yob			Elig+age	Elig+yob	
	OLS	2SLS	2SLS	LIML	OLS	2SLS	2SLS	LIML
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Years of schooling models								
Years of Schooling	.117	.068	.076	.072	.117	.068	.074	.071
	(.0007)	(.0293)	(.029)	(.030)	(.0007)	(.0294)	(.029)	(.030)
Experience	-.059	-.005	-.0167	-.0163	.0093	.0068	.0066	.0067
	(.0048)	(.0267)	(.034)	(.035)	(.0006)	(.0016)	(.002)	(.002)
Experience squared	.0012	.0002	.0004	.0004	-	-	-	-
	(.0001)	(.0005)	(.0006)	(.0006)				
experience derivative	.0089	.0069	.0066	.0068	-	-	-	-
	(.0006)	(.0016)	(.0016)	(.0017)				
Reduced-form veteran effect	-.013	-.013	-.012	-.012	-.019	-.014	-.013	-.013
	(.0012)	(.0036)	(.0039)	(.0040)	(.0011)	(.0032)	(.0032)	(.0033)
B. Years of college models								
Years of college	.131	.076	.089	.085	.132	.077	.085	.081
	(.0008)	(.0386)	(.038)	(.040)	(.0008)	(.0389)	(.038)	(.040)
Years of secondary education	.101	.152	.1441	.1470	.082	.149	.1426	.1461
	(.0020)	(.0300)	(.030)	(.031)	(.0017)	(.0309)	(.030)	(.031)
Years of primary education	.092	.060	.069	.067	.049	.055	.055	.055
	(.0041)	(.0110)	(.014)	(.014)	(.0035)	(.0047)	(.005)	(.004)
Experience	-.119	-.008	-.0243	-.0237	-.007	.008	.0074	.0076
	(.0062)	(.0272)	(.035)	(.036)	(.0005)	(.0019)	(.002)	(.002)
Experience squared	.0020	.0003	.0006	.0005	-	-	-	-
	(.0001)	(.0005)	(.0006)	(.0006)				
Experience derivative	-.0048	.0079	.0074	.0076	-	-	-	-
	(.0005)	(.0018)	(.0018)	(.0019)				
Reduced-form veteran effect	.018	-.015	-.013	-.013	.014	-.016	-.015	-.015
	(.0011)	(.0042)	(.0043)	(.0045)	(.0011)	(.0037)	(.0036)	(.0038)

Notes: The table reports estimates of the human capital earnings function described in the text. The schooling and experience variables were treated as endogenous, instrumented by draft eligbility and year of birth dummies, controlling for month of birth and state of birth dummies. First-stage F-statistics for years of schooling and years of college (adjusted for multiple endogenous variables) range from 15-45. Robust standard errors are reported in parentheses. Estimates were computed using census sampling weights.

Table 7. Veteran Effects on Other Outcomes for Whites

	1950-52				1948-52			
			2SLS				2SLS	
	Mean	OLS	elig	5zx	Mean	OLS	elig	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Works in public sector	.153	.040	.064	.064	.159	.034	.054	.059
		(.001)	(.007)	(.007)		(.001)	(.007)	(.007)
Works in federal government	.042	.047	.055	.055	.045	.044	.049	.052
		(.001)	(.004)	(.004)		(.001)	(.004)	(.004)
Lives in state of birth	.602	-.057	-.034	-.034	.599	-.043	-.027	-.030
		(.002)	(.010)	(.010)		(.001)	(.010)	(.009)
Currently single	.095	-.039	-.009	-.008	.089	-.038	-.006	-.007
		(.001)	(.006)	(.006)		(.001)	(.006)	(.005)
Currently married	.725	.001	.009	.009	.732	.008	.008	.009
		(.001)	(.009)	(.009)		(.001)	(.009)	(.008)
Ever married	.905	.039	.009	.008	.911	.038	.006	.007
		(.001)	(.006)	(.006)		(.001)	(.006)	(.005)

Note: This table reports estimates of veteran effects on the outcomes listed at left. The notes to Table 3 apply here as well.

Table A1. Descriptives for Whites and Nonwhites Born 1948-52

	Whites			Nonwhites		
	All	Vietnam vets	Non-vets	All	Vietnam vets	Non-vets
Draft eligibility (by RSN)	.437	.552	0.386	0.440	.520	.406
Veteran status (served in Vietnam Era)	.305	1.000	.000	0.293	1.000	.000
Post-Vietnam service	.034	.050	0.028	0.058	.060	.057
Age	49.2	49.6	49.0	49.2	49.5	49.0
A. Education variables						
Imputed highest grade completed	13.8	13.4	14.0	12.551	13.0	12.4
Year of college (0-4)	1.79	1.43	1.94	1.05	1.70	1.00
9th grade +	.975	.988	0.970	0.944	0.98	0.93
10th grade +	.963	.978	0.956	0.918	.970	.896
11th grade +	.946	.963	0.938	0.876	.950	.845
12th grade (no diploma) +	.930	.950	0.921	0.826	.924	.786
High school graduate +	.908	.930	0.899	0.766	.883	.717
Some college (less than 1 year) +	.659	.627	0.674	0.468	.585	.419
1 or more years of college (no degree) +	.588	.534	0.612	0.400	.491	.363
Associate's degree +	.419	.331	0.457	0.228	.250	.219
Bachelor's degree +	.341	.224	0.392	0.163	.146	.170
Master's degree +	.140	.081	0.166	0.060	.047	.065
Professional degree +	.052	.021	.066	.019	.011	.022
Ph.D. degree	.017	.007	.021	.007	.004	.007
B. Labor market variables						
Employment	.855	.846	0.859	0.662	.705	.644
Unemployment	.027	.028	0.026	0.054	.049	.056
Not in labor force	.118	.126	0.115	0.284	.247	.300
Self employed	.156	.120	0.171	0.068	.057	.072
Usual hours worked	41.2	40.7	41.401	32.594	34.2	31.9
Weeks worked	44.5	44.2	44.626	35.701	37.5	34.9
Wage and salary income	46595	41047	49034	27711	29187	27098
Self employment income	5285	3369	6128	1708	1352	1856
C. Other outcomes						
Works in public sector	.159	.184	0.148	0.200	.257	.176
Works in federal government	.045	.075	0.031	0.064	.117	.042
Lives in state of birth	.599	.570	0.612	0.631	.588	.649
Currently single	.089	.061	0.102	0.166	.118	.186
Currently married	.732	.740	0.728	0.564	.592	.553
Ever married	.911	.939	0.898	0.834	.882	.814
Sample size	1141905	348777	793128	154810	45425	109385

Notes: The table shows sample means from the 2000 Census, 1:6 file, weighted by census sampling weights.

Schooling and the Vietnam-Era GI Bill: Evidence from the Draft Lottery

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25th October 2010

Web Appendix

I Sample Notes

We worked with a non-public-use version of the 1-in-6 long-form sample that includes date of birth. The long-form sample is the basis for the publicly available Integrated Public Use Microdata Series (IPUMS) files. These files are simple random samples drawn from 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 (Census Bureau 2005). Adjustment for variation in sampling rates is made here using the weighting variables that are included in the long-form file. These weights adjust for non-response and for non-random sampling, and are designed to match external population totals by age, race, sex, and Hispanic origin. In practice, weighting matters little for our results. We confirmed that the means from the 1-in-6 file are close to those from the 5 percent file distributed via IPUMS. The original 2000 long-form sample includes Puerto Rico and island territories; residents of these areas are omitted from our study.

II Schooling Imputation

Using a matched CPS file with responses to both old (highest grade completed) and new (categorical) schooling questions, Jaeger (1997) calculates average and median highest grade completed conditional on categorical school values. He finds that the conditional median gives a better fit than the mean. We therefore use median highest grade completed for most categorical values. A drawback of this scheme, however, is that the categories in the new CPS schooling variable differ slightly from those on the 2000 Census long-form. Specifically, the Census allows for an additional some-college category: "some college, but less than one year." Because some veterans appear to have used the GI Bill to start a college program which they then left, we would like to distinguish this group from other veterans when imputing years of schooling. This may matter for our draft-lottery estimates of linear-in-schooling human capital earnings functions. A second drawback of the Jaeger scheme for our purposes is that it assigns the same value to those who report finishing 12th grade with no diploma and those who received a diploma.

In view of these concerns, we used Jaeger's finer conditional mean imputation to assign values to the census categories "grade 12 no degree" and "one or more years of college". Finally, we estimated a fractional year for the census category "some college but less than one year", by assuming that time in college is exponentially distributed with a fixed dropout hazard each month. This hazard rate was estimated from the ratio of those with at least 13 years completed to those with at least 13 years enrolled in the 1980 Census (for men aged 26-36), assuming a fixed hazard for 8 months of school. The exponential parameter was then used to estimate expected months in school for those ever enrolled in grade 13 college who drop out after one year. The result is an imputed value of 12.55 years. The resulting imputation scheme is: no schooling (0); nursery school through 4th grade (2.5); 5th-6th grade (5.5); 7th-8th grade (7.5); 9th (9); 10th grade (10); 11th grade (11); 12th grade no diploma (11.38); high school graduate (12); some college less than 1 year (12.55); 1 or more years of college no degree (13.35); associate degree (14); bachelors degree (16); masters, professional or doctoral degree (18).

A direct application of Jaeger's formula generates results almost identical to those reported in the paper. Note also that estimates of effects of military service on discrete schooling variables (e.g., an indicator for college graduation status) are unaffected by the choice of imputation scheme.

III Additional Tables

This section includes three additional tables for nonwhites. Table A1 shows the effect of Vietnam-era veteran status on education, Table A2 reports the effect on education by birth year, and Table A3 shows the effect on labor market variables and other outcomes.

Online Table 1: Veteran Effects on Education for Nonwhites

	1950-52				1948-52			
			2SLS				2SLS	
	Mean	OLS	elig	5zx	Mean	OLS	elig	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Years of schooling (imputed)	12.6	.512	.203	.090	12.6	.645	.184	.196
		(.020)	(.230)	(.226)		(.016)	(.235)	(.211)
Years of college	1.02	.118	.192	.173	1.02	.150	.159	.163
		(.0116)	(.118)	(.115)		(.0088)	(.119)	(.108)
9th grade +	.948	.043	.0013	.0003	.944	.055	-.009	-.0018
		(.0016)	(.019)	(.019)		(.0013)	(.020)	(.018)
10th grade +	.923	.063	-.0056	-.0044	.918	.079	-.015	-.0050
		(.0019)	(.023)	(.022)		(.0015)	(.023)	(.021)
11th grade +	.882	.090	.019	.019	.876	.110	.016	.025
		(.0023)	(.027)	(.027)		(.0018)	(.028)	(.025)
12th grade (no diploma) +	.832	.122	-.0021	-.0028	.826	.144	-.014	.0038
		(.0027)	(.032)	(.031)		(.0021)	(.032)	(.029)
High school graduate or higher +	.770	.147	.055	.055	.766	.170	.045	.058
		(.0032)	(.035)	(.034)		(.0024)	(.035)	(.032)
Some college (less than 1 year) +	.468	.158	.080	.083	.468	.171	.094	.092
		(.0042)	(.041)	(.040)		(.0031)	(.041)	(.037)
1 or more years of college (no degree) +	.400	.117	.070	.068	.400	.132	.054	.065
		(.0042)	(.040)	(.040)		(.0032)	(.041)	(.037)
Associate's degree +	.226	.024	.055	.051	.228	.031	.042	.051
		(.0036)	(.035)	(.034)		(.0027)	(.035)	(.032)
Bachelor's degree +	.160	-.032	.028	.020	.163	-.026	.012	.010
		(.0030)	(.031)	(.030)		(.0023)	(.031)	(.028)
Master's degree +	.057	-.020	.0080	.0060	.060	-.021	.020	.011
		(.0018)	(.019)	(.019)		(.0014)	(.020)	(.018)
Professional degree+	.018	-.012	-.0028	-.0027	.019	-.012	.0086	.0018
		(.0010)	(.011)	(.011)		(.0008)	(.011)	(.010)

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 sampling weights.

Online Table 2: Veteran Effects on Education by Single Year of Birth for Nonwhites

	1948	1949	1950	1951	1952
	(1)	(2)	(3)	(4)	(5)
Years of schooling (imputed)	1.002	-.226	-.014	.358	.134
	(1.144)	(.714)	(.400)	(.386)	(.418)
Years of college	.010	-.056	.057	.067	.275
	(.589)	(.355)	(.205)	(.200)	(.209)
1 or more years of college (no degree) +	.015	.040	.003	.052	.122
	(.198)	(.119)	(.069)	(.068)	(.072)
Associate's degree +	.018	.031	-.004	.050	.066
	(.172)	(.104)	(.060)	(.058)	(.062)
Bachelor's degree +	-.073	-.087	.021	-.022	.0330
	(.152)	(.091)	(.053)	(.051)	(.054)
Master's degree +	.112	.022	.023	-.029	.027
	(.107)	(.060)	(.034)	(.032)	(.032)

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 sampling weights.

Online Table 3. Other Veteran Effects for Nonwhites

	1950-52				1948-52			
			2SLS				2SLS	
	Mean	OLS	elig	5zx	Mean	OLS	elig	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Labor market variables								
Employment	.665	.049	.018	.033	.662	.063	.0013	.020
		(.0040)	(.040)	(.039)		(.0030)	(.040)	(.037)
Unemployment	.056	-.0035	-.047	-.048	.054	-.0063	-.027	-.036
		(.0019)	(.019)	(.019)		(.0014)	(.019)	(.018)
Not in labor force	.279	-.045	.029	.015	.284	-.057	.026	.016
		(.0039)	(.039)	(.038)		(.0029)	(.039)	(.036)
Self employed	.067	-.015	.007	.002	.068	-.016	.012	.009
		(.0020)	(.021)	(.020)		(.0015)	(.021)	(.019)
Usual hours worked	32.8	1.97	3.58	4.12	32.6	2.33	3.68	3.77
		(.171)	(1.71)	(1.68)		(.129)	(1.73)	(1.57)
Weeks worked	35.9	2.14	2.84	3.15	35.7	2.73	2.41	2.72
		(.186)	(1.86)	(1.82)		(.141)	(1.88)	(1.70)
Wage and salary income	27584	1324	3476	4948	27711	2109	1006	3294
		(313)	(3231)	(3199)		(239)	(3255)	(2968)
Log weekly wage	6.41	.028	-.037	.011	6.43	.042	-.0090	.018
		(.0074)	(.067)	(.065)		(.0057)	(.067)	(.061)
Self employment income	1709	-616	328	489	1708	-511	1750	1167
		(108)	(1177)	(1134)		(82.4)	(1167)	(1065)
Other outcomes								
Works in public sector	.198	.080	.127	.129	.200	.083	.085	.111
		(.0036)	(.033)	(.033)		(.0027)	(.033)	(.030)
Works in federal government	.062	.074	.101	.099	.064	.075	.080	.088
		(.0025)	(.020)	(.020)		(.0018)	(.020)	(.019)
Lives in the state of birth	.639	-.072	-.038	-.040	.631	-.061	-.051	-.053
		(.004)	(.039)	(.039)		(.003)	(.040)	(.036)
Currently single	.176	-.068	.007	.009	.166	-.063	-.023	-.008
		(.003)	(.032)	(.031)		(.002)	(.031)	(.029)
Currently married	.557	.030	-.017	-.020	.564	.036	.027	.001
		(.004)	(.041)	(.041)		(.003)	(.042)	(.038)
Ever married	.824	.068	-.007	-.009	.834	.063	.023	.008
		(.003)	(.032)	(.031)		(.002)	(.031)	(.029)

Note: All regressions include a full set of dummies for state of birth, year of birth and month of birth. Columns 3-4 and 7-8 report 2SLS estimates with the instrument sets listed. Robust standard errors are reported in parentheses. Estimates computed using sampling weights.

Schooling and the Vietnam-Era GI Bill: Evidence from the Draft Lottery: Replication Kit

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I Additional Tables

This section includes three additional tables for nonwhites. Table A1 shows the effect of Vietnam-era veteran status on education, Table A2 reports the effect on education by birth year, and Table A3 shows the effect on labor market variables and other outcomes.

II Replication Kit

This section includes programs that can be used to reproduce the results in the paper. These programs are meant to be run on non-public-use files archived at the Boston Census Data Center (BRDC). Researchers interested in working with files at the BRDC must be approved for access. See

`http://www.ces.census.gov/index.php/ces/contact_us_ces`

for details.

Four data steps before replication:

- getdata4.sas: Extract data from the raw census data set.
- vardef4.sas: Label variables and record the variable definition
- link_rsn_expanded_final.sas: Link the extracted data (vardef4) to the lottery numbers file (dobrsn20091215), by date of birth. We include cohorts born between 1944

3. Replication of table 3: t3white_final.do
4. Replication of table 4: t4white_final.do
5. Replication of table 5: t5white_final.do
6. Replication of table 6: t6white_final.do and t6fest.do
7. Replication of table 7: t7white_final.do
8. Replication of table a1: ta1_final.do
9. Replication of Online table 1: ot1_final.do
10. Replication of Online table 2: ot2_final.do
11. Replication of Online table 3: ot3_final.do

Replication of figures:

1. Replication of figure 1 (figure1.eps): figure1.do using data file cellavgstograph.csv
2. Replication of figure 2 (figure2.eps): figure2.do using data file Brigham_Graphnew_SC2010March.csv, based on estimates in online table 1.