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ABSTRACT

Long-Term Economic Consequences of Vietnam-Era Conscription: Schooling, Experience and Earnings*

Military service reduces civilian labor market experience but subsidizes higher education through the GI Bill. Both of these channels are likely to affect civilian earnings. New estimates of the effects of military service using Vietnam-era draft-lottery instruments show post-service earnings effects close to zero in 2000, in contrast with earlier results showing substantial earnings losses for white Vietnam veterans in the 1970s and 1980s. The recent estimates also point to a marked increase in post-secondary schooling that appears to be attributable to the Vietnam-era GI Bill. Seen through the lens of a Mincer wage equation, the wage effects observed in 2000 data can be explained by a flattening of the experience profile in middle age and a modest return to the additional schooling funded by the GI Bill. In particular, IV estimates of the returns to GI Bill-funded schooling are well below OLS estimates. Wage equations that allow for nonlinearities in the returns to schooling and a possible negative effect of military service on health, leave the main findings unchanged.

JEL Classification: J31, I22, I28, H56

Keywords: veterans, returns to schooling, instrumental variables

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1 Introduction

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 (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 benefits available to veterans. It's hard to exaggerate the role played by the GI Bill in contemporary social history (see, e.g., Humes, 2006). Consistent with this positive view, World War II (WWII) veterans typically earn somewhat more than same-age non-veterans, though white Vietnam era veterans, who had access to a similarly generous wartime GI Bill, do a little worse.[1]

A fundamental difficulty with simple comparisons by veteran status is selection bias. The process of screening for military service generates a pool of veterans who differ in important ways from non-veterans. In 1970, for example, half of those screened in the pre-induction physical were disqualified, while 20 percent of those screened at induction time were disqualified (Selective Service System, 1970). 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 seems likely to be an artifact of health-related selection bias. 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 high school graduates, and screens out those with very low test scores (see, e.g., Eitelberg *et al.*, 1984). As a result, men with very low earnings potential were unlikely to end up as soldiers. At the same time, some recruits found military service attractive precisely because their prospects in the civilian labor market were poor, while those with the highest earnings probably found it worthwhile to work hard to escape the draft. The net selection bias in this case is unclear.

The investigation in this paper begins with new estimates of the long-term causal effects of Vietnam-era service. As in Angrist (1989, 1990), the problem of selection bias is solved by using the Vietnam-era draft lotteries to construct instrumental variables (IV) estimates. However, this paper goes beyond earlier work using the draft lottery in a number of ways. First, because of newly available data from the 2000 Census, we are able to look at the consequences of Vietnam-era conscription as the draft-lottery cohorts

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

approach age 50.[2] Second, our inquiry is guided by a simple Mincer-style human capital earnings function. This framework highlights two of the most important channels whereby military service might affect earnings, loss of experience and subsidized higher education, and leads naturally to an empirical strategy where the returns to veteran-induced changes in experience and schooling can be estimated jointly. 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.

Our empirical framework builds on a long tradition of research on the effects of military service on veterans' schooling and earnings. The first attempt to estimate the economic returns to veterans' post-service schooling is Griliches and Mason (1972), who report results for a sample of WWII veterans from the 1964 CPS. The idea that time spent on active duty military service should be seen as lost civilian labor market experience is also discussed by Griliches and Mason (1972) and appears to originate with Mason (1970). Schwartz (1986) similarly estimated the returns to schooling for Vietnam and Korean-era veterans, arguing that the GI Bill probably lower returns. More recently, Angrist (1993) estimated the impact of GI Bill subsidies on schooling and the economic return to schooling for Vietnam veterans, while Lemieux and Card (2001) study Canadian veterans of WWII. The Lemieux and Card (2001) paper reports IV estimates (using instruments derived from cohort-province differences in enlistment rates) as well as OLS estimates. As far as we know, however, ours is the first attempt to use a human capital framework to provide a complete account of the causal effects of veteran status on earnings.

Our investigation generates a number of clear findings. First, the estimated effects of Vietnam-era service on earnings (and the estimated effects on other labor market variables such as employment) are nearly zero. This is roughly consistent with the experience profiles from Social Security data estimated by Angrist (1990). Second, the 2000 Census data show a marked impact of Vietnam-era conscription on schooling, with effects of a magnitude similar to those reported in studies of the WWII and Korean-era GI Bills by Bound and Turner (2002) and Stanley (2003). Finally, we put these pieces together by simultaneously instrumenting schooling and experience in a human-capital earnings function. The estimated returns to schooling that come out of this analysis are on the order of 7 percent, markedly smaller than the corresponding OLS estimates, as we might expect given the large subsidies to higher education provided by the GI Bill.

[2]We used confidential birthday information in the 2000 file through an agreement with the Census Bureau's Center for Economic Studies.

2 Empirical framework

A Mincer-style human capital earnings function highlights important channels through which military service might affect civilian earnings. 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. The Mincer equation is

$$y_i = \beta_0 + \beta_1 x_i + \beta_2 x_i^2 + \rho s_i + u_i, \tag{1}$$

where u_i is a residual that captures random variation in the earnings function across individuals. Although stylized, equation (1) is a workhorse of empirical labor economics that has repeatedly been found to describe essential features of the relationship between schooling, experience, and earnings. As a robustness check, we also report results for somewhat more general models, where the returns to schooling are nonlinear and the experience profile includes higher order terms.[3]

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

$$\begin{aligned} s_i &= s_{0i} + \delta v_i, & (2)\\ x_i &= a_i - s_i - 6 - \ell v_i = x_{0i} - (\delta + \ell) v_i, & (3) \end{aligned}$$

where v_i is veteran status, a_i is age, s_{0i} denotes i's schooling if he doesn't serve and $x_{0i} \equiv a_i - s_{0i} - 6$ is potential experience in the absence of service. 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.

The effect of veteran status on schooling, δ, can be positive or negative. On one hand, veterans were eligible for education subsidies through the GI Bill. On the other, veterans cannot usually attend school at traditional college-going ages. College attendance at older ages may be more costly, a result of higher foregone earnings or liquidity constraints, especially for veterans with families. The effect of compulsory military service on potential experience, however, is almost certainly negative. For conscripts, military experience is

[3] A number of studies evaluate the functional form assumptions of the simple Mincer equation. Two landmark contributions are Murphy and Welch (1990), which focuses on the shape of the experience profile, and Heckman, Lochner, and 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.

likely to be a poor substitute for the experience that these soldiers would have obtained, had they not been forced to serve. We therefore see military service as delaying entry into the civilian labor market. Specifically, veterans are assumed to lose ℓ years of civilian experience relative to men the same age and with the same educational attainment. Taking account of the effect of military service on schooling, Vietnam veterans lose $(\delta+\ell)$ years of experience relative to non-veterans.

The Mincer equation leads to a model with a veteran effect that interacts with x_{0i}, the level of potential work experience in the absence of military service. We focus initially on a scenario where military service affects earnings solely through lost experience; that is, $\delta = 0$, $s_i = s_{0i}$, and $x_i = x_{0i} - v_i\ell$. Using these assumptions and re-arranging equation (1), gives:

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

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

$$\pi_0 \equiv -[\beta_1\ell - \beta_2\ell^2], \quad (4a)$$

$$\pi_1 \equiv -2\beta_2\ell. \quad (4b)$$

Fitting a similar model to log social security earnings profiles for 1978-1984 (ignoring any causal effects of military service on education), Angrist (1990) estimated ℓ=2.08 (s.e.=.38), with an experience profile such that $\pi_0 = -.225$, and $\pi_1 = .011$. In other words, veterans start out at a -.225 wage disadvantage, but the gap closes by .011 each year. In this specification, the veteran earnings gap is zero when the mean of $x_i \approx 20.5$ or $age_i \approx 39$ for high school graduates.[4]

The effect of military service on schooling, summarized by the parameter δ, has downstream consequence for earnings via the returns to schooling, ρs_i, and the experience terms, $\beta_1 x_i$ and $\beta_2 x_i^2$. When δ is nonzero, the parameters determining the net veteran effect become:

$$\pi_0^* = -[\beta_1(\delta+\ell) - \beta_2(\delta+\ell)^2] + \rho\delta, \quad (5a)$$

$$\pi_1^* = -2\beta_2(\delta+\ell). \quad (5b)$$

In this case, the veteran intercept, π_0^*, also reflects changes in labor market experience due to schooling plus a term, $\rho\delta$, which captures the economic return to the service-

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

induced schooling increment. More generally, we can think of ρ and δ as varying across individuals, a point we return to, below. The veteran/experience interaction term, π_1^*, is also adjusted for experience lost while in school, but this adjustment should be small since δ turns out to be small relative to ℓ. Thus, any additional schooling due to the GI Bill should have a non-negligible impact on the level of veteran earnings with little impact on the rate of veteran catch-up.

The pure loss-of-experience model generates a restriction linking π_0 and π_1. To see this, note that the coefficients β_1 and β_2 are separately identified as the coefficients on experience and experience squared in (1), leaving only ℓ unknown in π_0 and π_1 (similarly, π_0^* and π_1^* are linked by equations 5a and 5b). A somewhat more general and econometrically unrestricted model allows the linear potential experience term to vary with veteran status according to $\beta_{1i} = \beta_{10} + \beta_{11}v_i$, where β_{11} is most likely negative. This formulation can be motivated by the Ben-Porath (1967) model of continuous human capital investment, since military service shortens the horizon for returns to post-service on-the-job training. In this specification, the human capital earnings function can be written:

$$y_i = \beta_0 + \beta_{10}x_{0i} + \beta_2 x_{0i}^2 + \rho s_{0i} + \pi_{xi}v_i + u_i,$$

where π_{xi} becomes $\pi_{xi} \equiv \tilde{\pi}_0 + \tilde{\pi}_1 x_{0i}$, with

$$\begin{aligned} \tilde{\pi}_0 &= -[(\beta_{10} + \beta_{11})(\delta + \ell) - \beta_2(\delta + \ell)^2] + \rho\delta, && \text{(6a)} \\ \tilde{\pi}_1 &= -[2\beta_2(\delta + \ell) - \beta_{11}]. && \text{(6b)} \end{aligned}$$

This model is characterized by a reduced initial earnings loss, with a slower rate of catch-up than in the simpler, constant-slope models. Fitting a version of the reduced-slope model with no schooling effects, Angrist (1990) estimated ℓ=1.84 (s.e.=.43), $\tilde{\pi}_0 = -.189$, and $\tilde{\pi}_1 = .006$. In this case, the veteran earnings gap disappears when the mean of $x_i = .189/.006 \approx 31.5$ or $age_i \approx 50$ for those with a high school diploma. Thus, allowance for an additional free parameter generates earnings profiles with somewhat slower veteran catch-up.

The results reported below suggest that the extra schooling fueled by the GI Bill comes out to about .3 years. Assuming, as earlier work and our results below suggest, that the returns to this additional schooling are roughly .07, the GI Bill adds about 2 percent to veterans' earnings. The extra schooling also reduces experience, generating somewhat more complicated terms involving $(\delta + \ell)$. But in the 2000 census, the draft lottery cohorts are of an age where experience profiles are nearly flat and well-approximated by a linear profile. It therefore seems reasonable to think of reduced-form veteran effects in 2000 data as estimates of

$$\pi_{net} = -\beta_{10}(\delta + \ell) + \rho\delta, \tag{7}$$

obtained by setting $\beta_{11} = \beta_2 = 0$. Since we estimate the linear profile to have a slope equal to about .007, equation (7) with an assumed loss of experience of 2 years (the service obligation of draftees) accounts for a net causal impact of veteran status on earnings close to zero.

The Mincer equation is a highly stylized model with no direct effect of military service on earnings. In the empirical work, we also consider a version that allows for direct effects of military service on earnings through health. Still, the simple Mincer model has important implications that can be checked empirically. A key implication of both the restricted and unrestricted loss-of-experience models is that by the time of the 2000 Census, when average age in the 1950-52 cohorts reached 48, the veteran earnings gap should have closed. Moreover, if the GI Bill is important, we should look for a modest return to schooling that partially or entirely offsets any residual earnings gap due to lost experience.

Two final econometric points are worth mentioning. First, 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). We can therefore allow for some degree of nonlinearity in the returns to schooling by treating years of primary and years of secondary schooling as exogenous covariates, while treating years of college as endogenous in a two-stage least squares (2SLS) procedure based on (1).

Second, our reduced-form estimates of the causal effects of veteran status, i.e., causal effects estimated by instrumental variables without imposing the structure of the Mincer equation, are local average treatment effects (LATEs) for draft-lottery compliers in the sense of Angrist, Imbens, and Rubin (1996). Compliers in this case are men who served in the Vietnam era because they were assigned a low lottery number but would not have served otherwise. We can link the compliers idea with the more structural interpretation outlined in this section using random coefficients notation. Specifically, in this context local average treatment effects (LATE) can be seen as estimating

$$E[-\beta_{10i}(\delta_i + \ell_i) + \rho_i\delta_i | v_{1i} > v_{0i}],$$

where v_{1i} denotes i's potential veteran status when draft-eligible, v_{0i} denotes i's potential veteran status when ineligible (so compliers have $v_{1i} > v_{0i}$), and the i subscripts on β_{10i}, ρ_i, and δ_i represent cross-sectional heterogeneity in the returns to experience, schooling, and the effects of military service on schooling. This link recognizes, for example, that terms of service differed for draftees and volunteers, and that other veteran groups might be affected differently by the GI Bill. As it turns out, however, our estimates of the effects of the GI Bill are remarkably close to those reported by Bound and Turner (2002) and Stanley (2003) for World War II and Korean-era veterans.

3 Data and First-Stage

3.1 The 2000 Census 1-in-6 File

The 2000 Census long form sample includes approximately one-sixth of US households.[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. Because the cohorts of 19-year-olds at risk of conscription in the draft lotteries were born from 1950-52, our analysis looks at the sample of men in this group. This sample includes about 700,000 whites and 96,000 nonwhites. There was a smaller but non-negligible draft-lottery impact on men born in 1948 and 1949, so estimates are also reported for an expanded sample of men born 1948-52. The 1948-52 sample includes more than 1.14 million whites and about 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 the 1953 cohort.

Roughly 24 percent of men born 1950 to 1952 served in the Vietnam era and about 38 percent were draft-eligible. These and other descriptive statistics appear in Table 1, which

[5]The 1-in-6 long form sample is the basis for the publicly available PUMS files. These files, documented in US Census Bureau (2005), 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. Adjustment for variation in sampling rates is made here by using the weighting variables that are included in the long-form 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. In practice, weighting matters little for our results. We also confirmed that the means from publicly available data from the 1-in-6 file are close to those from the 5 percent 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.

reports means by veteran status and race for the 1950-52 sample (Descriptive statistics for the 1948-52 sample and means by single year of birth appear in Appendix Tables A1 and A2). Descriptive statistics for labor market variables are collected in Panel A. Among whites, veterans have lower employment rates and earnings than 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.

Overall, the average schooling level in the sample is 13.8 years for whites and 12.6 years for nonwhites. The average years of college is 1.76 for whites and 1.05 for nonwhites. These statistics can be seen in Panel B.[6] The contrast in average educational attainment by veteran status parallels the contrast in earnings, with white veterans obtaining less schooling and non-white veterans obtaining more schooling than their non-veteran counterparts. On the other hand, although white veterans are less likely than white nonveterans to have attended or completed one or more years of college, they are more likely than non-veterans to be high school graduates. Among nonwhites, veterans are more likely than nonveterans to have attended college or graduated from high school. However, nonwhite nonveterans are more likely than nonwhite veterans to have earned a BA.

3.2 The Draft-Lottery First Stage

The first draft lottery, held in December 1969, affected men born in 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.

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

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

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 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, 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.[7] 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 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) shows that this can be explained by the fact that nonwhites were more likely than whites to consider military service an attractive career option.

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

3.2.1 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 breaking 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.[8] 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.

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

The $5z$ instrument set does not produce more precise 2SLS estimates than $elig_i$ alone. This is in spite of the fact that 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 appears in columns 3-7 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.[9]

4 Labor-Market Effects

We look first at employment and earnings. The results reported here are 2SLS estimates of the parameter α in the equation

$$Y_i = w_i'\beta + \alpha v_i + \varepsilon_i, \tag{8}$$

where Y_i is an outcome variable; v_i is veteran status; and w_i is a vector of covariates that includes year of birth dummies, state of birth dummies, and month 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 of equation (8) are also reported.[10]

[9]A larger instrument set with dummies for RSN 1-30 and RSN 31-60 adds little to the precision obtained with $5zx$. Likewise, a non-parametric first stage using the fitted values from Figure 1 fails to generate a meaningful gain in precision relative to $5zx$.

[10]A potential problem with the second-stage estimates is the possibility of selection bias due to excess mortality among draft-eligible men. There are two likely channels for this. The first is war-related deaths, since civilian samples are limited to those who survived the war. The second is elevated post-service mortality due to physical injury, PTSD, or other long-term consequences of military service such as an increased likelihood of cigarette smoking (as suggested by Bedard and Deschenes, 2006, for WWII veterans). For reasons discussed in the appendix, however, mortality-related selection is unlikely to be important for the draft-lottery cohorts.

As discussed in the previous section, 2SLS estimates of equations like (8) capture the effect of service on those who were drafted or who volunteered in the face of draft risk, in other words, *draft-lottery compliers*. The average causal effect for compliers is the local average treatment effect (LATE) generated by draft-lottery instruments (Imbens and Angrist, 1994). The assumptions required for a LATE interpretation of draft-lottery estimates are (a) that draft lottery numbers are independent of potential outcomes in the treated and non-treated state and (b) monotonicity of the first-stage relation (here, monotonicity means draft-eligibility can only make military service more likely for any given individual, as seems plausible).

The independence assumption is supported in part by random assignment. Lottery numbers should be uncorrelated with ability or family background. Part of this assumption is also 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 also worth noting that most soldiers who served in the lottery period were not compliers; rather, they were true volunteers who were not drafted and did not volunteer simply to avoid conscription.[11] Estimates using draft-lottery instruments need not generalize to the population of true volunteers. Nevertheless, the effects of military service on men compelled to serve against their will reflect the historical consequences of conscription. These estimates may also be relevant for contemporary discussions of military manpower policy, since compliers in the future are likely to be similar to those from the draft-lottery period.[12] Moreover, given an economic mechanism such as the Mincer

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

[12]The Selective Service System web site states that "if a draft were held today," it would involve a lottery over 19-year olds. There would be few deferments, as in the Vietnam-era lottery, with at most a one-semester deferment for enrolled students. And it seems likely that any future draft would come

equation of Section 2, which explains the effects of Vietnam-era service, we might draw broader conclusions as to how conscription affects soldiers. Not surprisingly, however, these conclusions require stronger assumptions than a reduced-form "treatment-effects-style" analysis of causal effects.

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 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 -.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. 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.

The pattern of OLS estimates is reversed for nonwhites, with veterans more likely to be working and earning more than non-veterans. But the 2SLS estimates in Panel B of Table 3 offer little evidence of an impact on the employment or earnings of nonwhites: the estimated earnings effects for nonwhites are positive but insignificant. It should be noted, however, that the 2SLS estimates for nonwhites are considerably less precise than those for whites, due both to a smaller sample and a weaker first-stage. 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. Some of the estimated effects on weeks and hours worked by nonwhites are positive and significantly different from zero, e.g., an increase of 3.7 hours per week in column 7 (s.e.=1.7). There is also some evidence of reduced unemployment for nonwhites in the 1948-52 sample. On the other hand, the estimated effects on employment and weeks worked by nonwhites are positive but insignificant. On balance, therefore, the results for nonwhites seem inconclusive, though perhaps leaning towards positive long-run effects.

It's noteworthy that the $5zx$ instrument set (5 lottery-number dummies with a full

in wartime. Finally, as in the Vietnam era, those conscripted would be men who do not find GI Bill education benefits enough of an inducement to volunteer. See http://www.sss.gov/viet.htm.

set of year-of-birth interactions) produces only slightly more precise estimates than $elig_i$ alone. The clearest precision gains appear in the 1948-52 sample. For example, the standard error for the effect on earnings in the sample of whites born 1948-52 falls from 1243 to 1133, with similar coefficient estimates. The standard error for the effect on log wages changes by only .01 in this sample, from .16 to .15. This reflects the fact that although the $5zx$ interactions terms are highly significant in the first stage, they are not very big.

As a partial check on the underlying identifying assumptions, we computed over-identification test statistics for the key earnings and wage results in Table 3, and for the key schooling results in Table 4 (years of schooling and years of college), discussed below. For whites, all eight test statistics come out with p-values of at least .4. A couple of the p-values for nonwhites are between 1-5 percent, but there are no decisive rejections. 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.

The 2SLS estimates in Table 3 contrast with the earnings losses reported for white veterans in Angrist (1990). The latter range from 10-15 percent of FICA-taxable earnings in 1981-84. As suggested by the framework outlined in Section 2, however, results from the 2000 Census can be reconciled with the earlier results 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.

5 Effects on Schooling

Compulsory military service appears to have increased the educational attainment of Vietnam-era veterans, a result documented in Table 4. For example, the 2SLS estimates using $elig_i$ in the 1950-52 sample suggest that white veterans got .345 more years of schooling than nonveterans. The corresponding results are slightly lower in the 1948-52 sample, but change little when estimated with an expanded instrument set. Both samples generate precise estimates with standard errors of about .05. In contrast to the results for whites, however, the estimates for nonwhites (reported in Panel B) are smaller and not significantly different from zero.

The remainder of Table 4 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. More specifically, veterans were more likely to attend college (including partial years) or to earn an associate's degree. These effects are on the order of .06-.09. The increase in the likelihood of completing a BA degree is smaller though still marked, 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.[13]

5.1 GI Bill Benefits vs. Draft Deferments

The schooling shifts documented in Table 4 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.[14] 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.[15]

[13] 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.

[14] 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).

[15] 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,

The notion that the GI Bill increased schooling is supported by a number of earlier studies. For example, Bound and Turner's (2002) preferred IV estimates of the effects of WWII service on college completion by white men are around 5-6 percentage points while their preferred 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 while his estimates of effects on years of college range from .20-.33. The college completion effects reported in Table 4 are a little over 5 points for whites and range from .24-.27 for years of college, remarkably similar to the Bound and Turner (2002) and Stanley estimates. The estimates in Table 4 also echo Turner and Bound (2003) in that they show larger effects of the GI Bill on whites than nonwhites. Finally, Lemieux and Card (2001) report effects of a similar magnitude in cohorts that benefitted from the Canadian GI Bill, while Angrist (1993) finds large post-service schooling increases associated with the use of the Vietnam-era GI Bill.[16]

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 fact that the importance of educational deferments declined sharply 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 no deviation from trend in the male-to-female college graduate ratio or the proportion with some college in cohorts born 1950 or later.[17]

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).

[16]The BEOG program (Pell grants) also played an important role in expanding college attendance for adult students in the 1970s (see, e.g., 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).

[17]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.

5.2 Additional Evidence on the GI Bill Hypothesis

Estimates of schooling effects by single year of birth, reported in Table 5, also weigh against draft deferment as the primary force behind the schooling effects in Table 4. In particular, Table 5 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 years of college for the 1952 cohort (which had no access to college deferments) are smaller than for the 1951 cohort, but similar in magnitude or larger than the estimated effects on white men born from 1948-50.

Differences across cohorts in the 2SLS estimates of effects on some-college dummies mirror the differences in estimates of effects on years of schooling and years of college. For example, the estimated effect on a dummy variable indicating 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 latter's wider access to college deferments. It's also worth noting that the estimates by single year of birth for nonwhites, though imprecise, are typically larger for younger cohorts. On balance, therefore, Table 5 points away from draft deferment as the primary explanation for the results in Table 4.

Schooling Trends in the CPS

A second piece of evidence supporting the GI Bill explanation of increased schooling among Vietnam veterans comes from the schooling trends of veterans as observed in the Current Population Surveys (CPS). Our interpretation of these trends is based on a model that divides total educational attainment into three parts: pre-service schooling for veterans or schooling completed as of the typical entry age for non-veterans (s_i^A); schooling acquired between the typical entry and discharge ages (Δs_i^B); and the difference between completed schooling and the schooling completed at the typical discharge age (Δs_i^C). Completed education is the sum of these components:

$$s_i = s_i^A + \Delta s_i^B + \Delta s_i^C. \tag{9}$$

We think of s_i^A as schooling at age 19, $s_i^A + \Delta s_i^B$ as schooling at ages 22-24, and s_i as schooling completed by age 40, when GI Bill eligibility expired for the cohorts studied here.

In principle, military service can have a causal effect on either Δs_i^B and Δs_i^C or both. In contrast, s_i^A is a "pre-treatment" variable that might be correlated with veteran status but should not be caused by veteran status. To make this explicit, let $\Delta s_i^B(v)$ denote the potential schooling acquired during the service period, where $v = 0, 1$ indexes veteran status. Similarly, let $\Delta s_i^C(v)$ denote the potential schooling acquired in the post-service period. Veteran and non-veteran potential schooling increments are defined for all i, regardless of realized veteran status.

To highlight key features of the causal connection between military service and schooling, we make the not unrealistic assumption that soldiers get no schooling while in the military ($\Delta s_i^B(1) = 0$). Therefore, we have,

$$\Delta s_i^B = \Delta s_i^B(0)(1 - v_i).$$

We also assume that non-veterans complete their education by the time most veterans are discharged, so that $\Delta s_i^C(0) = 0$ and we can write:

$$\Delta s_i^C = \Delta s_i^C(1) v_i.$$

These two assumptions can be linked to the potential outcomes notation in Section 2 by observing that in the absence of military service, potential schooling is

$$s_{0i} = s_i^A + \Delta s_i^B(0)$$

while men who serve in the military get

$$s_{1i} = s_i^A + \Delta s_i^C(1).$$

Observed schooling is therefore

$$s_i = s_{0i} + (s_{1i} - s_{0i})v_i = s_i^A + \Delta s_i^B(0) + [\Delta s_i^C(1) - \Delta s_i^B(0)]v_i.$$

Thus, the causal effect of veteran status on an individual veteran's schooling is $[\Delta s_i^C(1) - \Delta s_i^B(0)]$. In other words, the causal effect of military service on individual schooling is the veteran post-service schooling increment, $\Delta s_i^C(1)$, net of the schooling gains foregone while in the military, $\Delta s_i^B(0)$.

In practice, individual causal effects are not observable so we try to estimate average effects. The average causal effect of military service on veterans' schooling is

$$E[\Delta s_i^C(1) - \Delta s_i^B(0)|v_i = 1] = E[\Delta s_i^C(1)|v_i = 1] - E[\Delta s_i^B(0)|v_i = 1]. \tag{10}$$

Military service increases average education when the average post-discharge increase in veterans' schooling is enough to overcome the education veterans lost while serving. The quantity $E[\Delta s_i^C(1)|v_i = 1]$ has a sample counterpart (assuming we can get the timing right). But the quantity $E[\Delta s_i^B(0)|v_i = 1]$ is counterfactual: we have to make some assumptions - other than those of the IV framework - to get an independent handle on it.

As a first step towards the identification of $E[\Delta s_i^C(1) - \Delta s_i^B(0)|v_i = 1]$, note that the observed veteran/nonveteran difference in expected schooling growth from entry age to completion is

$$E[s_i - s_i^A|v_i = 1] - E[s_i - s_i^A|v_i = 0] = E[\Delta s_i^C(1)|v_i = 1] - E[\Delta s_i^B(0)|v_i = 0]. \tag{11}$$

The sample analog of this expression contrasts veteran and non-veteran schooling growth. This is not quite what we want since the observed $E[\Delta s_i^B(0)|v_i = 0]$ is subtracted instead of the counterfactual $E[\Delta s_i^B(0)|v_i = 1]$. But assuming $E[\Delta s_i^B(0)|v_i = 0] = E[\Delta s_i^B(0)|v_i = 1]$, that is, the schooling veterans lost while in the military is equal to the schooling non-veterans obtained at the same ages, equation (11) is the average causal effect of veteran status on schooling expressed in equation (10). In practice, the schooling non-veterans obtained during the service period probably exceeded the schooling veterans lost while in the military, so the empirical counterpart of (11) is, if anything, an underestimate of (10).

We estimated the difference in schooling increments by veteran status using a sample of white men in the 1964-1991 CPS. This covers the period from the beginning of the Vietnam era to just beyond the expiration of Vietnam-era GI Bill entitlements in 1989. The underlying conditional means can be seen at the top of Figure 2, which plots educational attainment by age and veteran status for the Vietnam-era cohorts.[18]

[18] A drawback of the CPS for our purposes is that most active duty soldiers are not in the sampling frame so we miss many veterans (the CPS includes only soldiers stationed in the US, living off-base or with their families). The absence of most active-duty soldiers probably tends to bias the veteran average upwards at young ages since some of those counted as veterans will have returned to school while active-duty soldiers have not yet had the chance to do so. Hence, the baseline veteran deficit is probably even larger than shown in the figure. A detailed description of the data and methods used to construct Figures 2 appears in the appendix.

Panel A of Figure 2 shows that the educational attainment of Vietnam veterans born from 1948 to 1952 increased little when these men were in their early twenties, while the schooling of non-veterans the same age was rising sharply. On the other hand, while the age-schooling profile of non-veterans flattened early, the schooling of Vietnam veterans continued to increase when these men were in their thirties.

Panel B of Figure 2 focuses on the evolution of the difference in average education by veteran status at each age. For the purposes of this figure, differences for single years of age were smoothed using either a two-year or three-year moving average. This panel documents the rapidly increasing and then shrinking veteran/non-veteran schooling differential. The change in the schooling differential by veteran status is another version of equation (11) since

$$\begin{aligned} E[s_i - s_i^A|v_i &= 1] - E[s_i - s_i^A|v_i = 0] \\ &= \{E[s_i|v_i = 1] - E[s_i|v_i = 0]\} - \{E[s_i^A|v_i = 1] - E[s_i^A|v_i = 0]\}. \end{aligned} \tag{12}$$

This expression highlights the differences-in-differences nature of the identification strategy outlined in this section.

The empirical counterpart of the right-hand side of (12) appears at the bottom of Figure 2. Specifically, Panel C plots the veteran/nonveteran difference in the moving average of schooling, relative to the average over the first two or three years of age in Panel B. The corresponding difference-in-differences estimates of the effect of veteran status on schooling range from 0 to .4 years depending on the moving average window and the width of the age range used to estimate completed schooling (the older the group the larger the effect). For example, taking age 38 as the terminal point gives an increase of .2 years using a two-year moving average and .4 years using a three-year moving average. Thus, our analysis of CPS data on schooling trends comes down close to the 2SLS estimates of the effect of veteran status on schooling using draft lottery instruments.

6 Schooling, Experience and Earnings

Here, we bring the experience and schooling channels together using the framework outlined in Section 2. In this framework, veteran status affects wages by reducing potential experience x_i and increasing schooling s_i, but with no direct effects. For purposes of estimation, 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. We start with a human capital earnings function with three endogenous variables: x_i, x_i^2 and s_i. Age and cohort effects are assumed to be captured by the potential-experience quadratic so that age or year of birth are available as instruments.

Estimates of equation (1) are reported in Table 6 for the sample of white men born 1948-52. 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 returns to schooling are about .12. The estimated experience profile in this case does not have the usual concavity, reflecting the fact that the profile in this age range is fairly flat (the experience derivative is small, about .009 (s.e.=.001)). The veteran earnings loss due to lost experience, constructed from equations (4a) and (4b), is equal to -.015 (s.e.=.0006).

Instrumental variables estimates of the return to schooling are considerably 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 (1). In over-identified models, LIML provides a check for possible finite-sample bias in 2SLS.[19] 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.= .034). Swapping year-of-birth dummies for age_i and age_i^2 generates a 2SLS estimate of .075 (s.e.=.033), reported in column 3. The first-stage F-statistic for schooling, calculated in a manner that takes account of multiple endogenous variables, has a value of 16. This is outside the range where bias in 2SLS estimates

[19]The finite-sample behavior of LIML is discussed in, e.g., Anderson, Kunitomo, and Sawa (1982). 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. In some cases, however, heteroscedasticity biases LIML; see, Hausman, *et al.* (2007).

is usually a concern.[20] 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.

In an attempt to increase the precision of the estimated schooling coefficients, we used the $5zx$ instrument set constructed from five RSN dummies interacted with year of birth. This generates somewhat smaller schooling coefficients (not reported here). But the multivariate F-statistic for for the schooling first stage in this model is low, about 3.6, and the LIML estimates fall to zero with standard errors much larger than those for the corresponding 2SLS estimates. Since the just-identified or moderately over-identified estimates reported in columns 2-4 of Table 6 appear to be more reliable than the estimates coming out of heavily over-identified models, we focus on the former.

The fact that the experience profile is close to linear with a modest slope is confirmed in columns 5-8 of Table 6, which report the results of estimating models similar to those reported in columns 1-4, but with a linear experience profile. The experience derivative is given by the linear experience term in this case and equal to .009 for OLS and about .007 for 2SLS and LIML. The schooling coefficients estimated in models with a linear experience profile are virtually identical to those reported in columns 1-4. As a further check on the sensitivity of these estimates to the functional form of the experience profile, Appendix Table A4 reports a set of estimates with cubic and quadratic experience controls. Here too, the estimated returns to schooling are virtually unchanged.[21]

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[20] 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 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 Bekker (1994) and Angrist and Krueger (1995).

[21] Paralleling the original specifications, the extra experience terms are treated as endogenous while adding age^3 and/or age^4 to the instrument list. Because age, age^2, age^3, age^4 are close to collinear in our sample of men born 1948-52, we rescale age and experience into an interval from -1 to +1, a modification

6.1 Disability Effects

The empirical framework motivating Table 6 allows for indirect effects of veteran status via schooling and experience. In practice, however, changes in veterans' health provide an additional avenue whereby military service may have affected earnings. For example, Hearst, Newman and Hulley's (1986) pioneering draft-lottery study found elevated civilian mortality risk among draft-eligible men, mostly due to an excess of suicide and motor vehicle accidents. On the other hand, 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 (see Section A of the Appendix for details). A number of recent studies using the draft lottery also find little evidence of adverse health consequences for Vietnam-era draftees.[22]

Although empirical results to date have been mixed, 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). Veterans also have health concerns related to the Agent Orange defoliant used by American forces. Finally, the loss of earnings associated with Vietnam-era conscription may itself have been debilitating. Consistent with this view, estimates in our working paper show an impact of Vietnam-era veteran status on non-work-related disability rates for whites (Angrist and Chen, 2007). At the same time, our 2SLS estimates generate no effect on *work-related* disability rates.[23]

To explore the impact of possible disability effects on the Mincer equation, we esti-

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that leaves the theoretical schooling parameter unchanged. Hausman and Newey (1995) use a similar rescaling to overcome collinearity when working with a nonparametric series estimator.

[22]Goldberg, Richards, Anderson, and Rodin (1991) found no significant increase in 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. Bedard and Deschenes (2006) suggest that WWII service increased smoking and smoking-related disease, probably because WWII veterans were given free cigarettes. Eisenberg and Rowe (2007), using draft-lottery instruments, find increased smoking in the immediate post-Vietnam period, but the effects are imprecise and disappear in later data. They also find no evidence of effects on other health outcomes.

[23]Given these inconsistencies, the estimated impact on disability rates for veterans may reflect, at least in part, the financial incentives in the veterans' compensation system. Autor and Duggan (2007) note that veterans disability compensation is not taxed to offset earnings. Duggan, Rosenheck and Singleton (2006) show that enrollment in the veterans compensation program seems highly sensitive to changes in program rules and to unemployment rates. A recent VA study investigates a surge in compensation claims from 1999-2004 and the large variation in these claims across states (VA, 2005).

mated a model that allows disability rates to increase with Vietnam-era service:

$$y_i = \beta_{0\gamma} + \beta_{1\gamma} x_i + \beta_{2\gamma} x_i^2 + \rho_\gamma s_i + \gamma d_i + u_i. \tag{13}$$

In this equation, d_i indicates non-work-related disability status (the disability variable that appears to have been most affected by veteran status in our earlier paper), with coefficient γ. The addition of d_i to the list of endogenous variables generates highly imprecise results, but we can get a sense of the consequences of higher disability rates for 2SLS estimates of equation (13) by inserting plausible values of γ in the following model

$$y_i^* \equiv y_i - \gamma^* d_i = \beta_{0\gamma} + \beta_{1\gamma} x_i + \beta_{2\gamma} x_i^2 + \rho_\gamma s_i + \epsilon_i. \tag{14}$$

As a benchmark, we set $\gamma^* = -.2$, slightly larger in magnitude than the OLS estimate of the wage loss associated with non-work disabilities using equation (13).

Adjusting for disability status in this manner increases the 2SLS estimates of the returns to schooling by .005-.007, as Panel B of Table 6 shows. Variations on these results for alternative choices of γ^* can be obtained by observing that $\widehat{\rho}_\gamma$, the 2SLS estimate of the schooling coefficient in equation (14), is related to $\widehat{\rho}_0$, the 2SLS estimate of the schooling coefficient imposing $\gamma^* = 0$, as follows:

$$\widehat{\rho}_\gamma = \widehat{\rho}_0 - \widehat{\lambda}\gamma^*,$$

where $\widehat{\lambda}$ is the 2SLS estimate of the coefficient on s_i in a regression of d_i on the right-hand-side variables in equation (14), again, treating all variables as endogenous. Because $\widehat{\lambda}$ in this adjustment is only about .03, the difference between $\widehat{\rho}_\gamma$ and $\widehat{\rho}$ is small for any plausible value of γ^*.

6.2 Nonlinearity and Heterogeneity in the Returns to Schooling

The 2SLS estimates in Table 6 reflect both the range of variation induced by the draft lottery and the fact that not everyone is a draft-lottery complier. Specifically, because the draft lottery affects schooling through veteran status, which in turn works through the GI Bill, the 2SLS estimates capture the return to a college-level schooling increment for GI Bill users. With nonlinear and heterogeneous returns, this complicates the comparison of 2SLS to OLS estimates or to IV estimates using other instruments. Although the 2SLS and OLS estimates reported here were constructed using linear constant-effects models, both types of estimates can be understood as weighted average effects. The weighting schemes for the two estimation strategies differ and therefore the estimated returns may differ due to nonlinearities in the causal relation between schooling and earnings, even if there is no omitted variables bias in the OLS estimates.

This 2SLS weighting scheme is easiest to describe for IV estimates in a nonlinear model without covariates. Let $f_i(s)$ denote the potential (or latent) earnings that person i would receive after obtaining s years of education. Note that the function $f_i(s)$ has an "i" subscript on it while s does not. This function tells us what i would earn for *any* value of schooling, s, and not just for the realized value, s_i. In other words, $f_i(s)$ answers causal "what if" questions for multinomial s_i. A linear random coefficients model sets $f_i(s) = \beta + \rho_i s$, but here we allow $f_i'(s)$ to vary with both s and i.

Suppose that s_i takes on values in the set $\{0, 1, ..., \bar{s}\}$, so there are $\bar{s}$ incremental causal effects, $f_i(s) - f_i(s-1)$, for $s = 1, ..., \bar{s}$. The 2SLS estimator is a computational device that generates a weighted average of these incremental effects, with a weighting function we can estimate, so as to learn where the action is with a particular instrument. Draft lottery instruments for schooling put the most weight on years of college.

To flesh this out, assume that a draft-eligibility dummy is used to estimate the returns to schooling in a model with no covariates, so 2SLS is a Wald estimator. Let s_{1i} denote the schooling that i would get if $elig_i = 1$, and let s_{0i} denote the schooling that i would get if $elig_i = 0$. The formula below, adapted from Angrist and Imbens (1995), shows how the Wald estimator captures an average causal response:

$$\frac{E[y_i|elig_i = 1] - E[y_i|elig_i = 0]}{E[s_i|elig_i = 1] - E[s_i|elig_i = 0]} = \sum_{s=1}^{\bar{s}} \omega_s E[f_i(s) - f_i(s-1)|s_{1i} \geq s > s_{0i}] \tag{15}$$

where

$$\omega_s \equiv \frac{P[s_{1i} \geq s > s_{0i}]}{\sum_{j=1}^{\bar{s}} P[s_{1i} \geq j > s_{0i}]} \tag{16}$$

is a positive weighting function that sums to one. The assumptions that lay behind this formula are: that draft-eligibility is randomly assigned and affects earnings only through schooling (the independence and exclusion restrictions), that draft-eligibility affects schooling for at least some people (existence of a first stage), and that schooling can only increase as a consequence of draft-eligibility (monotonicity).[24]

[24]Formally, these assumptions are (a) Independence and Exclusion: $\{f_i(0), f_i(1), ..., f_i(\bar{s}); s_{0i}, s_{1i}\} \amalg elig_i$, (b) First-stage: $E[s_{1i} - s_{0i}] \neq 0$, and (c) Monotonicity: $s_{1i} \geq s_{0i}\ \forall i$ (or vice versa). In this illustrative bivariate example, the exclusion restriction implies that the experience profile is flat and that there are no other veteran effects.

Formula (15) says that the Wald estimator is a weighted average of $E[f_i(s) - f_i(s-1)|s_{1i} \geq s > s_{0i}]$, the average difference in potential earnings for *compliers at point* s. In this case, compliers are men driven by draft eligibility from a level of schooling less than s to at least s. By virtue of monotonicity, the size of the complier group, $P[s_{1i} \geq s > s_{0i}]$, is given by the difference in the CDF of s_i conditional on $elig_i$ at point s.[25] There is another link of interest here: The 2SLS weighting function for a 2SLS model with y_i on the left-hand side and s_i endogenous is proportional to the reduced form for a 2SLS model with s_i on the left-hand side and v_i endogenous. Thus, the 2SLS estimates of effects of veteran status on schooling reported in columns 3 and 7 of Table 4 give us a look at the (unnormalized) weighting function, (i.e., the numerator of 16) since just-identified 2SLS estimates are proportional to the corresponding reduced form.

The appropriately normalized 2SLS weighting function for white men born 1948-52 is plotted with pointwise confidence bands in Figure 3 (these estimates correspond to those in column 7 of Table 4). The weighting function jumps at the level of some college, while tapering off at the MA level and higher. The shaded bars show the histogram of schooling for veterans, characterized by a distinctive mode for high school graduates. The 2SLS estimates reported in Table 6 therefore tell us more about the returns to years of college than a histogram weighting scheme would do.

It's also worth noting that the CDF difference plotted in Figure 3 is relevant for 2SLS estimates even when schooling is independent of potential outcomes. If s_i is independent of potential outcomes, then $E[f_i(s) - f_i(s-1)|s_{1i} \geq s > s_{0i}] = E[y_i|s_i = s] - E[y_i|s_i = s-1]$, the difference in the the conditional expectation function (CEF) of log wages given schooling at $s_i = s$. In this case, the Wald estimator becomes

$$\sum_{s=1}^{\bar{s}} \omega_s(E[y_i|s_i = s] - E[y_i|s_i = s-1]),$$

where ω_s is the weighting function in (15) as before. This highlights the fact that 2SLS captures an incremental return over the range of values shifted by the instrument, whether or not selection bias is a problem.

[25]The CDF difference is

$$\begin{aligned} P[s_{1i} &\geq j > s_{0i}] = P[s_{0i} < j] - P[s_{1i} < j] \\ &= P[s_i < j|elig_i = 0] - P[s_i < j|elig_i = 1]. \end{aligned}$$

The denominator of the weighting function, $\sum_{j=1}^{\bar{s}} P[s_{1i} \geq j > s_{0i}]$, equals the Wald first stage, $E[s_i|elig_i = 1] - E[s_i|elig_i = 0]$, because the mean of a non-negative random variable is the sum-over-support of one minus the CDF.

An analogous interpretation of OLS estimates also comes from the conditional expectation function. Specifically, whether causal or not, OLS estimates produces a weighted-over-s average of $E[y_i|s_i = s]$. The formula below (adapted from Angrist and Krueger, 1999) describes the OLS weighting scheme:

$$\frac{Cov(y_i, s_i)}{V(s_i)} = \sum_{s=1}^{\bar{s}} \mu_s(E[y_i|s_i = s] - E[y_i|s_i = s-1])$$

where

$$\mu_s \equiv (E[s_i|s_i \geq s] - E[s_i|s_i < s])P[s_i \geq s](1 - P[s_i \geq s]). \tag{17}$$

Thus, OLS estimates give more weight to incremental changes in the CEF at points in the distribution of s_i closer to the median (where $P[s_i \geq s](1 - P[s_i \geq s])$ is maximized) and at points where a break induces a larger shift in the conditional mean of schooling (where $(E[s_i|s_i \geq s] - E[s_i|s_i < s])$ is maximized).

Estimates of μ_s are also plotted in Figure 3 (with dots). Like the 2SLS weighting function, the OLS weighting function tops up for years of college. Overall, however, the OLS weighting function is flatter than the 2SLS weighting function. Therefore, motivated by Figure 3, and as a specification check for the 2SLS estimates, we estimated a piecewise linear model that allows differing returns to years of college, years of secondary schooling, and years of primary schooling.[26] In practice, we don't have enough instruments to treat each of the schooling components as endogenous. But because draft eligibility mostly affects years of college, it seems reasonable to treat the years of primary and years of secondary schooling variables as exogenous controls, while instrumenting years of college with draft-eligibility status. As before, the experience profile is treated as endogenous and identified by age or year of birth.

The estimated returns to college are somewhat higher than the overall returns to schooling in the piecewise linear model. This can be seen in Table 7, which reports results from the piecewise linear model using a format similar to that of Table 6. Specifically, the OLS estimate in the first row of column 1 increases to .13, while the corresponding 2SLS estimates range from .076 to .089 depending on the instrument list and whether the experience profile is linear or quadratic. Adjustment for disability effects increases the 2SLS estimates by a small amount as before, with returns as high as .097 in column 3.

[26] The pieces were calculated as follows: years of primary = $\min(s_i, 8)$; years of secondary = $\min[\max(s_i - 8, 0), 4]$; years of college = $\min[\max(s_i - 12, 0), 4]$. These pieces sum to $\min(s_i, 16)$, i.e., to years of schooling capped at 16.

Importantly, however, a substantial gap between the OLS and 2SLS estimates remains even after focusing on the returns to a college-specific schooling increment.

Finally, a simple economic model with heterogeneous effects can be used to see why the returns to college attendance for GI Bill users might be below the average return for all men who have attended college. Because the 2SLS estimand is shaped by nonlinearity as well as by heterogeneity, it's easiest to make this point when schooling as dichotomous, so that nonlinearity is irrelevant. In particular, suppose that we are interested in the returns to college education in a world where everyone either attends college ($s_i = 1$) or finishes schooling with a high school diploma ($s_i = 0$). Since college attendance is now the only margin on which draft-eligibility operates, equation (15) simplifies to $E[f_i(1) - f_i(0)|s_{1i} = 1, s_{0i} = 0]$, the average college premium for those who go to college when draft-eligible but not otherwise.

We can dig further into the nature of heterogeneous returns using a Roy-type model where the GI Bill affects schooling by reducing costs by an amount κ and men go to college if the benefits exceed the costs.[27] Specifically, suppose that costs are $c_i = c_0 - \kappa v_i$, where c_0 is the cost of attendance for non-veterans. The veteran status first stage is:

$$v_i = \phi_0 + \phi_1 elig_i + \xi_i,$$

where ϕ_1 is the effect of draft-eligibility on veteran status and ξ_i is the first-stage residual. The schooling first stage can be derived from this by writing:

$$c_i = c_0 - \kappa[\phi_0 + \phi_1 elig_i + \xi_i] = c^*_{0i} - \kappa^* elig_i,$$

where $c^*_{0i} \equiv c_0 - \kappa[\phi_0 + \xi_i]$ and $\kappa^* \equiv \kappa\phi_1$.

Potential schooling in this model is determined by a comparison of costs and benefits for men with draft-eligibility equal to zero and one:

$$\begin{aligned} s_{0i} &= 1[f_i(1) - f_i(0) > c^*_{0i}] \\ s_{1i} &= 1[f_i(1) - f_i(0) > c^*_{0i} - \kappa^*]. \end{aligned}$$

The return to lottery-induced college enrollment is therefore the local average treatment effect on draft-eligibility compliers:

$$\begin{aligned} LATE_{elig} &= E[f_i(1) - f_i(0)|s_{1i} = 1, s_{0i} = 0] \\ &= E[f_i(1) - f_i(0)|c^*_{0i} \geq f_i(1) - f_i(0) > c^*_{0i} - \kappa^*]. \end{aligned}$$

[27]The use of the Roy model to interpret IV estimates of heterogeneous returns to schooling originates with Bjorklund and Moffitt (1987). For a recent discussion using a 0-1 example as we do here, see Heckman, Lochner, and Todd (2005).

The return to college for the college educated, $E[f_i(1)-f_i(0)|s_i=1]$, is a weighted average of $LATE_{elig}$ and the effect of college attendance on men who go to college regardless of their draft-eligibility status. In the language of Angrist, Imbens, and Rubin (1996), these men are *always-takers*. The return to college attendance for always takers is

$$\begin{aligned} E[f_i(1)-f_i(0)|s_{0i}=s_{1i}=1] &= E[f_i(1)-f_i(0)|s_{0i}=1] \\ &= E[f_i(1)-f_i(0)|f_i(1)-f_i(0)>c^*_{0i}], \end{aligned}$$

since, by virtue of monotonicity, $s_{0i}=1$ implies $s_{1i}=1$. The return to college for the college educated can therefore be written as:

$$E[f_i(1)-f_i(0)|s_i=1] = E[f_i(1)-f_i(0)|f_i(1)-f_i(0)>c^*_{0i}]p_a + LATE_{elig}(1-p_a),$$

where $p_a \equiv \Pr\{f_i(1)-f_i(0)>c^*_{0i}|s_i=1\}$ is the portion of always-takers among those who enroll in college. In this example, the effect on always-takers exceeds the effect on draft-eligibility compliers because

$$E[f_i(1)-f_i(0)|f_i(1)-f_i(0)>c^*_{0i}] > E[f_i(1)-f_i(0)|c^*_{0i} \geq f_i(1)-f_i(0) > c^*_{0i}-\kappa^*].$$

Thus, Roy-type selection provides an economic explanation for low IV estimates of the returns to schooling using draft-lottery instruments.[28]

7 Summary and Conclusions

Consistent with a flattening of age-earnings profiles in middle age, the adverse economic consequences of Vietnam-era military service appear to have faded. At the same time, 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 Frederiksen and Schrader's (1951) pioneering investigation of the impact of the

[28]Delayed college attendance followed by a shorter working life for veterans should act to increase returns, but discounting should make this second-order relative to the direct effects of GI Bill subsidies.

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 they definitely would have gone to college without GI Bill funding.

An important contribution of our study is to use variation in Vietnam veterans' experience and schooling to identify the components of a traditional human capital earnings function. 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 possible disability effects and nonlinearities in the earnings function. Although not precise enough to be statistically significantly different from the OLS estimates (as is common for IV estimates), the IV estimates are consistently below the corresponding OLS estimates in all specifications. As conjectured by Berger and Hirsch (1983), a simple economic explanation for low returns to schooling among veterans is the large subsidy to schooling provided by the GI Bill.

A low economic return to GI-Bill-subsidized schooling is not a universal finding. For example, using the Canadian WWII-era GI Bill as a source of exogenous variation, Lemieux and Card (2001) report IV estimates larger than the corresponding OLS estimates. But attenuated returns to post-service schooling are broadly in line 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 for comparably-aged non-veterans, while Angrist (1993) reported a return to Vietnam veterans' post-service schooling of .043 using the 1987 survey of veterans. Another useful benchmark comes from Heckman, Lochner, and Todd (2005), who estimate the impact of tuition and taxes on the internal rate of return to schooling under alternative assumptions. They find that tuition reduces the internal rate of return to college completion for white men in the 1990 Census by about one quarter. Thus, the GI Bill, which roughly covers tuition, room, and board at a state school, ought to reduce equilibrium returns by at least as much.

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 these veterans come out ahead 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. Although the GI Bill made this loss about 15 percent smaller than it otherwise would have been, it did not come close to offsetting the full costs of conscription.

Appendix

A. Sample Selection Due to Mortality

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.[29] It therefore seems unlikely that war-related deaths have a large effect on the composition of the sample used in our study.

An increase in civilian mortality for veterans seems more likely to affect the composition of post-Vietnam samples than combat 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 are due to suicide and motor vehicle accidents, possibly related to PTSD.

[29]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 are 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).

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 was 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, the proportion eligible is more than predicted, .4085 versus .4038.

Comparisons by single year of birth for white men born 1948-53, reported in detail in Appendix Table A3, 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, but all are small. Three out of six cohort-specific contrasts are significant for nonwhites, but these always show slightly more eligibles than predicted. Given the size and sign of these comparisons, it seems unlikely that excess civilian mortality has a substantial effect on the composition of the 2000 Census sample.

B. Construction of Figure 2

Figure 2 uses data from the 1964, 1965, and 1967-1991 CPS March Demographic Supplements (the 1966 supplement does not contain veteran status). The raw data were downloaded from the Minnesota Population Center's Integrated Public Use Microdata Series, accessible at www.ipums.org. We included white Vietnam veterans and non-veterans born 1948-1952 in the sample. Year of birth was imputed assuming men were born after the survey date. Vietnam veterans are defined as 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.

Panel A of the figure shows mean years of education, derived from the variable HIGRADE, for veterans and non-veterans. Unlike CPS supplements from 1992 or later, the pre-1992 supplements report years of education instead of highest degree obtained. Averages were constructed by weighting microdata 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., 3 years of data) contribute to any given age-education observation.

The series plotted in panels B and C were constructed by first collapsing the education data by age as for Panel A. We then constructed two- and three-year moving averages of mean years of education. The moving averages are unweighted in that each age-education cell enters with equal weight in the moving average. Panel B reports the difference in moving averages by veteran status. 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 C, except that this panel shows the difference between the moving average at age 19 and subsequent values.

C. 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 degree (18); professional degree (18); doctoral degree (18).

It's worth noting that 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.

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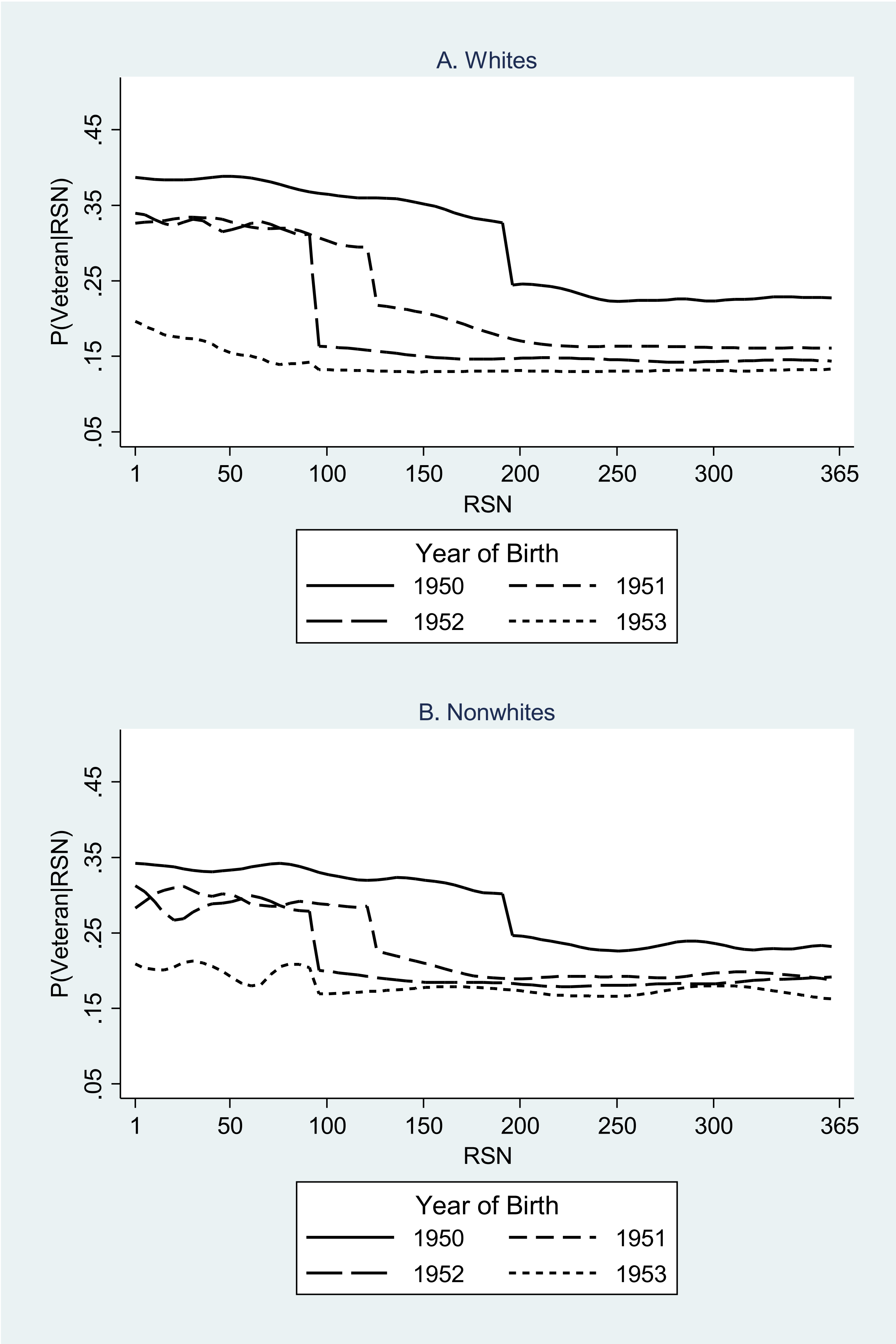

Figure 1: First Stage Plots - The relation between the probability of military service and draft lottery numbers. Notes: Data are from the 2000 Census and smoothed using a bandwidth of .4 that does not straddled the draft-eligibility cutoff.

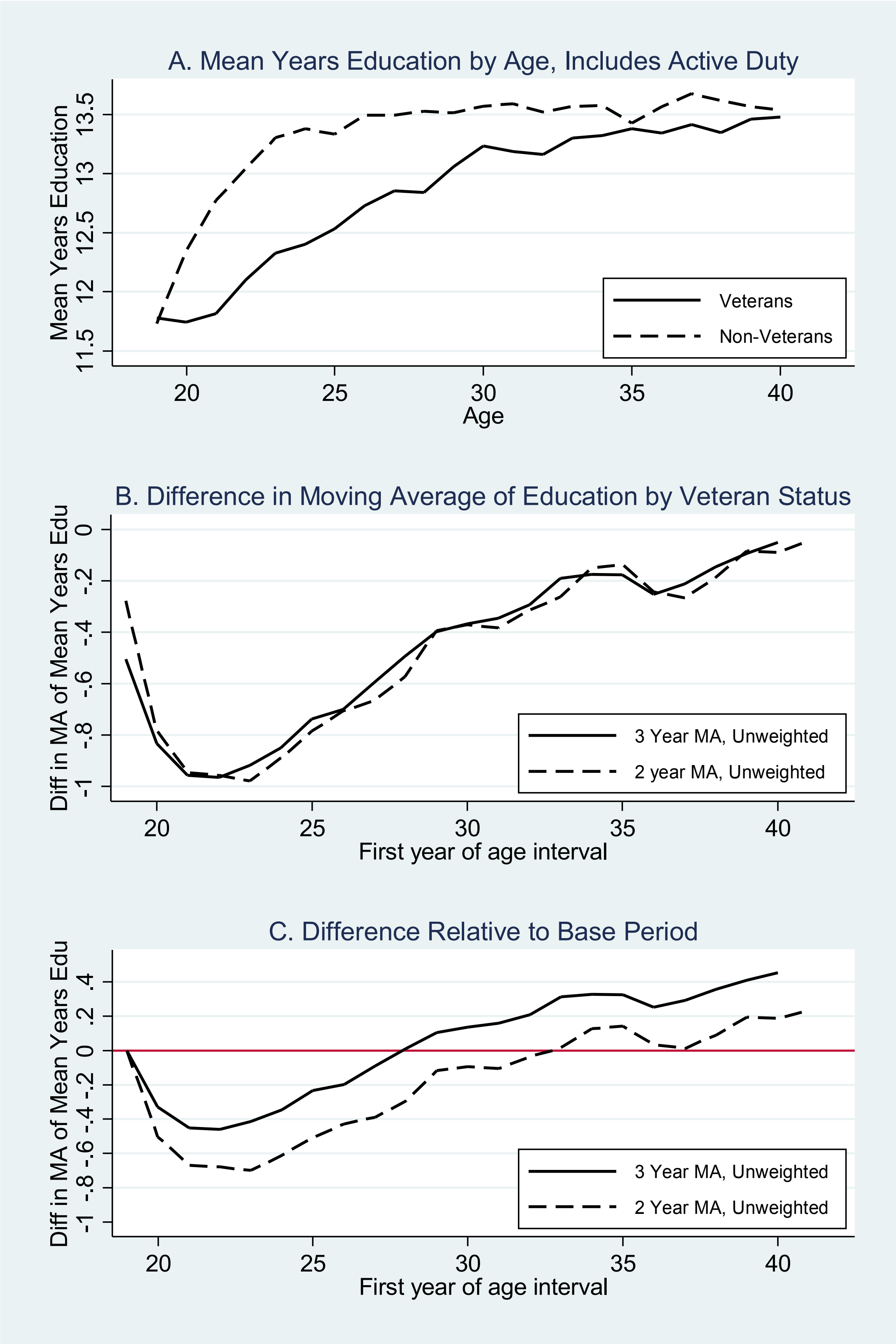

Figure 2: Average schooling by age and veteran status (for white men born 1948-1952). Notes: The figure shows averages or smoothed moving averages from the 1964-1991 March CPS (except 1966).

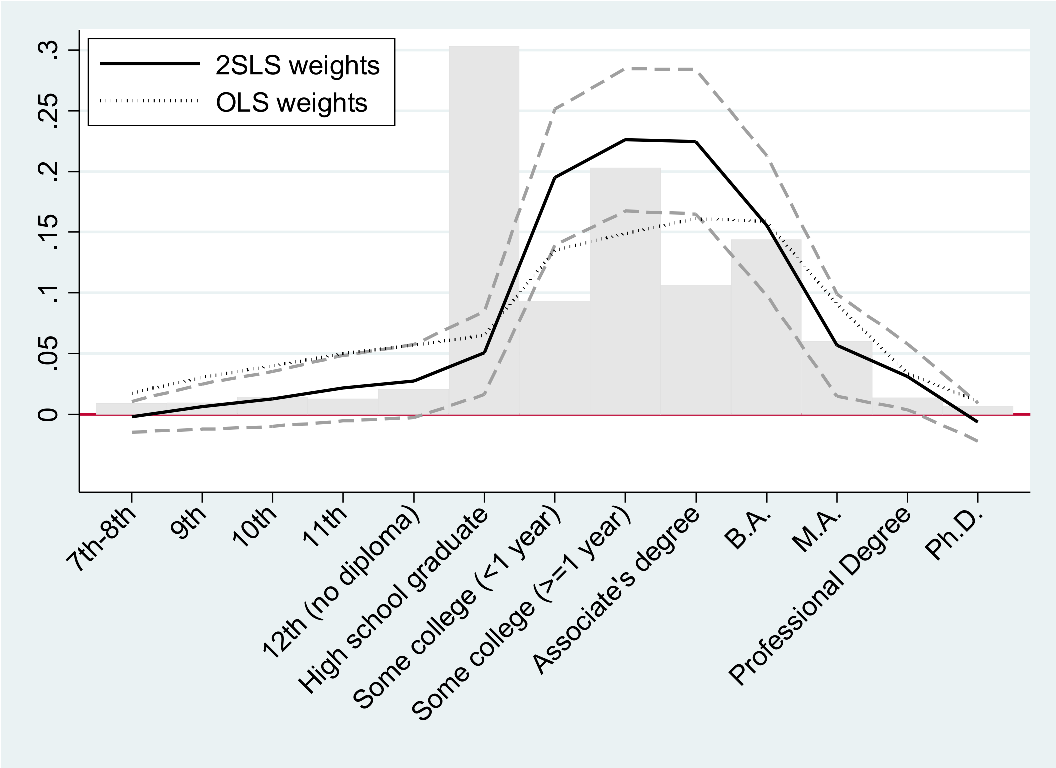

Figure 3: 2SLS and OLS weighting functions, along with standard error bands for the 2SLS weights (for white men born 1948-52). Notes: The plot also shows the schooling histogram for veterans (shaded bars).

Table 1: Descriptive statistics, by race and veteran status, 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. 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. 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	1.01
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
N	696530	166652	529878	96217	23246	72971

Note: The table shows statistics from the 2000 Census, 1:6 file, weighted by census sampling weights.

Table 2: First-stage estimates, 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: The table reports draft-eligibility effects and RSN group effects estimated in separate regressions. Robust standard errors are shown in parentheses. All models include a full set of dummies for year of birth, state of birth, and month of birth. Sampling weights were used for all estimates and statistics. (Year of birth dummies are dropped from the models used to produce columns 3-8).

Table 3: Effects of veteran status on labor market variables

	1950-52				1948-52			
			2SLS				2SLS	
	Mean	OLS	elig	5zx	Mean	OLS	elig	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Whites								
Work variables in 1999								
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	.0019
		(.0011)	(.0066)	(.0064)		(.0008)	(.0066)	(.0060)
Usual hours worked	41.5	-.888	-.101	-.230	41.2	-.544	.055	-.137
		(.054)	(.334)	(.325)		(.040)	(.335)	(.305)
Weeks worked	44.8	-.752	-.133	-.192	44.5	-.243	-.120	-.175
		(.054)	(.330)	(.321)		(.040)	(.331)	(.301)
Earnings variables in 1999								
Wage and salary income	46406	-8616	-517	-873	46595	-7936	-115	-548
		(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	668
		(77.8)	(616)	(606)		(62.3)	(616)	(567)
B. Nonwhites								
Work variables in 1999								
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)	(.035)
Usual hours worked	32.8	1.97	3.58	4.12	32.6	2.33	3.68	3.76
		(.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	2.71
		(.186)	(1.86)	(1.82)		(.141)	(1.88)	(1.70)
Earnings variables in 1999								
Wage and salary income	27584	1324	3476	4969	27711	2109	1006	3314
		(313)	(3231)	(3199)		(239)	(3255)	(2968)
Log weekly wage	6.41	.028	-.037	.012	6.43	.042	-.0090	.019
		(.0074)	(.067)	(.065)		(.0057)	(.067)	(.060)
Self employment income	1709	-616	328	436	1708	-511	1750	1115
		(108)	(1177)	(1147)		(82.4)	(1167)	(1077)

Note: All models 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 sampling weights.

Table 4: Effects on education, by race and year of birth

	1950-52				1948-52			
			2SLS				2SLS	
	Mean	OLS	elig	5zx	Mean	OLS	elig	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Whites								
Years of schooling (imputed)	13.8	-.549	.345	.348	13.8	-.547	.303	.324
		(.0075)	(.054)	(.052)		(.0057)	(.053)	(.049)
Years of college	1.76	-.511	.273	.269	1.79	-.532	.244	.254
		(.0051)	(.035)	(.034)		(.0038)	(.034)	(.031)
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	.0062
		(.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	-.048	.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)

(Continued)

Table 4 (cont.): Effects on education, by race and by year of birth

	1950-52				1948-52			
			2SLS				2SLS	
	Mean	OLS	elig	5zx	Mean	OLS	elig	5zx
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
		B. Nonwhites						
Years of schooling (imputed)	12.6	.542	.239	.224	12.5	.680	.208	.223
		(.020)	(.232)	(.227)		(.016)	(.236)	(.213)
Years of college	1.05	.133	.192	.172	1.05	.168	.146	.155
		(.0117)	(.119)	(.116)		(.0089)	(.120)	(.109)
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	-.0027	.826	.144	-.014	.0039
		(.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	.019	.163	-.026	.012	.010
		(.0030)	(.031)	(.030)		(.0023)	(.031)	(.028)
Master's degree +	.057	-.020	.0080	.0067	.060	-.021	.020	.011
		(.0018)	(.019)	(.019)		(.0014)	(.020)	(.018)
Professional degree+	.018	-.012	-.0028	-.0026	.019	-.012	.0086	.0018
		(.0010)	(.011)	(.011)		(.0008)	(.011)	(.010)

Note: All models 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 sampling weights.

Table 5: 2SLS Estimates of effects on schooling, by race and single year of birth

	1948 (1)	1949 (2)	1950 (3)	1951 (4)	1952 (5)
A. Whites					
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)
B. Nonwhites					
Years of schooling (imputed)	1.002	-.226	-.014	.358	.135
	(1.144)	(.714)	(.400)	(.385)	(.418)
Years of college	.009	-.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)	(.061)
Bachelor's degree +	-.073	-.087	.021	-.0220	.0330
	(.152)	(.091)	(.052)	(.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 and month of birth. Robust standard errors appear in parentheses. Estimates were computed using sampling weights.

Table 6: Estimates of the returns to schooling for 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. No disability adjustment								
Years of schooling	.117	.068	.075	.072	.117	.068	.074	.071
	(.0006)	(.0338)	(.033)	(.036)	(.0006)	(.0339)	(.033)	(.036)
Experience	-.061	-.005	-.0169	-.0165	.009	.007	.0066	.0067
	(.0048)	(.0308)	(.040)	(.040)	(.0006)	(.0019)	(.002)	(.002)
Experience^2	.0012	.0002	.0004	.0004				
	(.0001)	(.0005)	(.0007)	(.0007)				
Experience derivative	.0087	.0069	.0066	.0068				
	(.0011)	(.0037)	(.0018)	(.0040)				
Earnings loss due to lost experience	-.015	-.013	-.013	-.013	-.018	-.014	-.013	-.013
	(.0006)	(.0019)	(.0038)	(.0020)	(.0011)	(.0037)	(.0037)	(.0039)
B. With disability adjustment								
Education	.116	.074	.082	.079	.116	.075	.080	.077
	(.0006)	(.0336)	(.033)	(.036)	(.0006)	(.0337)	(.033)	(.036)
Earnings loss due to lost experience	-.016	-.014	-.013	-.013	-.018	-.014	-.014	-.014
	(.0006)	(.0037)	(.0018)	(.0040)	(.0011)	(.0037)	(.0036)	(.0039)
First-stage F-statistic for education (adjusted multivariate)		25.81	15.86			38.64	15.93	

Notes: The table reports estimates of the human capital earnings function described in the text. The schooling and experience terms are endogenous. The reported F-statistic is for the years of schooling first stage, adjusted for other endogenous variables.

Table 7: Years-of-college effects for 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. No disability adjustment								
Years of college	.130	.076	.089	.085	.132	.077	.085	.081
	(.0008)	(.0446)	(.044)	(.047)	(.0008)	(.0448)	(.044)	(.047)
Years of secondary	.101	.152	.1441	.1470	.082	.149	.1426	.1461
	(.0020)	(.0351)	(.035)	(.037)	(.0017)	(.0355)	(.035)	(.037)
Years of primary	.092	.060	.068	.067	.049	.055	.056	.055
	(.0041)	(.0131)	(.016)	(.016)	(.0035)	(.0056)	(.005)	(.006)
Experience	-.120	-.008	-.0245	-.0239	-.008	.008	.0074	.0076
	(.0062)	(.0314)	(.041)	(.041)	(.0005)	(.0021)	(.002)	(.002)
Experience^2	.0020	.0003	.0006	.0005				
	(.0001)	(.0005)	(.0007)	(.0007)				
Experience derivative	-.0053	.0079	.0074	.0076				
	(.0005)	(.0043)	(.0021)	(.0046)				
Earnings loss due to lost	.014	-.015	-.014	-.014	.015	-.016	-.015	-.015
experience	(.0011)	(.0021)	(.0043)	(.0022)	(.0011)	(.0043)	(.0042)	(.0045)
B. With disability adjustment								
Years of college	0.129	0.084	0.097	0.094	0.130	0.086	0.092	0.088
	-(.0008)	-(.0443)	-(.0437)	-(.0465)	(.0008)	(.0446)	(.0436)	(.0469)
Earnings loss due to lost	.013	-.016	-.014	-.015	.015	-.016	-.015	-.016
experience	(.0011)	(.0043)	(.0043)	(.0022)	(.0011)	(.0042)	(.0042)	(.0045)
First-stage F-statistic for education (adjusted multivariate)		29.69	18.08			44.35	18.08	

Notes: The table reports estimates of the human capital earnings function described in the text. The years of college and experience terms are endogenous. Other schooling variables are exogenous controls. The reported F-statistic is for the years of college first stage, adjusted for other endogenous variables.

Table A1: Descriptive statistics for white cohorts

	1950-52	1948-52	1948-53	1948	1949	1950	1951	1952	1953
A. Demographics and veteran status									
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
B. 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
C. Education variables									
Imputed highest grade completed	13.8	13.8	13.8	13.9	13.9	13.8	13.8	13.7	13.7
Years of college	1.76	1.79	1.76	1.84	1.82	1.80	1.76	1.72	1.66
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
D. Disability variables									
Non-work disabilities	.070	.074	.072	.082	.077	.074	.070	.068	.065
Any disabilities	.193	.198	.196	.211	.202	.199	.192	.189	.184
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: All estimates and statistics use census weights.

Table A2: Descriptive statistics for nonwhite cohorts

	1950-52	1948-52	1948-53	1948	1949	1950	1951	1952	1953
A. Demographics and veteran status									
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
B. Education 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
C. Education variables									
Imputed highest grade completed	12.6	12.5	12.5	12.5	12.5	12.6	12.6	12.5	12.6
Years of college	1.05	1.05	1.05	1.08	1.05	1.06	1.05	1.03	1.02
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
D. Disability variables									
Non-work disabilities	.116	.120	.118	.130	.125	.119	.115	.114	.110
Any disabilities	.326	.332	.329	.343	.342	.331	.325	.321	.314
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: All estimates and statistic use census weights.

Table A3: Theoretical and empirical proportions 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)
$F(6,\infty)$		5.07	4.37	3.24
N		1570310	1382287	188023

Notes: The theoretical proportion draft eligible is reported in column 1 for each cohort. Fractions appear in brackets. Columns 2-4 report the difference between this and the empirical proportion draft-eligible, with robust standard errors in parentheses. The F-statistic is for a joint test of theoretical and empirical equality for all cohorts.

Table A4: Earnings functions with additional experience terms

Log weekly wages, no disability adjustment, Whites born 1948-52

	Linear experience		Quadratic experience		Cubic experience		Quartic experience	
	OLS	2SLS	OLS	2SLS	OLS	2SLS	OLS	2SLS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Education	0.1172	0.0685	0.1178	0.0681	0.1189	0.0691	0.1185	0.0692
	(0.0007)	(0.0338)	(0.0007)	(0.0338)	(0.0007)	(0.0350)	(0.0007)	(0.0384)
Scaled potential Exp.	0.1163	0.0855	0.2518	0.1104	0.1980	0.0937	0.2617	0.0900
	(0.0071)	(0.0232)	(0.0119)	(0.0691)	(0.0110)	(0.2085)	(0.0158)	(0.6323)
Scaled potential Exp.^{2}			0.1849	0.0317	0.3663	-0.0376	0.4831	-0.0265
			(0.0129)	(0.0838)	(0.0219)	(0.8308)	(0.0262)	(1.7683)
Scaled potential Exp.^{3}					0.3197	-0.0625	0.1941	-0.0156
					(0.0228)	(0.7453)	(0.0384)	(7.0113)
Scaled potential Exp.^{4}							-0.2254	0.0357
							(0.0447)	(5.3823)
Returns to experience	0.0093	0.0068	0.0090	0.0069	0.0090	0.0068	0.0090	0.0068
	(0.0006)	(0.0019)	(0.0006)	(0.0019)	(0.0006)	(0.0024)	(0.0006)	(0.0037)

Notes: The table reports variations on the specifications reported in Table 6, with additional polynomial experience controls. The instrument sets include an indicator for draft eligibility and polynomial terms in age corresponding to the experience terms in the model. The age and experience terms in all models were rescaled to lie in the interval [-1,1].