Estimating the Veteran Effect with Endogenous Schooling when Instruments are Potentially Weak[1]

Saraswata Chaudhuri[2] and Elaina Rose[3]

Date: November 4, 2009

Abstract

Instrumental variables estimates of the effect of military service on subsequent civilian earnings either omit schooling or treat it as exogenous. In a more general setting that also allows for the treatment of schooling as endogenous, we estimate the veteran effect for men who were born between 1944 and 1952 and thus reached draft age during the Vietnam era. We apply a variety of state-of-the-art econometric techniques to gauge the sensitivity of the estimates to the treatment of schooling and to overcome the problem of weak instruments. We find a significant veteran penalty.

JEL Classification: C2; J24

Keywords: Veteran effect; Weak instruments

[1] We are grateful to Colin Cameron, Scott Carrell and Jim Hosek for their helpful comments.
[2] Department of Economics, University of North Carolina, CB 3305, Chapel Hill, NC 27599, USA. Tel: (919) 966-3962. Fax: (919) 966-4986. Email: saraswata_chaudhuri@unc.edu.
[3] Department of Economics, University of Washington, Box 353330, Seattle, WA 98195, USA. Tel: (206) 543-5237. Fax: (206) 685-7477. Email: erose@u.washington.edu.

I. Introduction

The costs of war include veterans' foregone civilian human capital and labor market experience as well as the health effects due to exposure to combat (Oi, 1967; Stiglitz and Bilmes, 2008). On the other hand, there are also opportunities for human capital acquisition, which, for many veterans, would not otherwise be available. Estimates of the effect of military service on subsequent civilian earnings vary widely, ranging from negative 10 percent to positive 25 percent.

A central issue in the literature is the endogeneity of military service. Young men choose whether or not to volunteer. Even during the Vietnam era that we study, many young men availed themselves of a variety of opportunities to avoid the draft. Instrumental variables (IV) can correct for the endogeneity bias in the estimates of veteran effect. The challenge is finding an instrument for military service. Angrist (1989, 1990, 1991) and Angrist and Chen (2008) show that the randomization of the Vietnam era draft provides a suitable instrument.

A second issue is the treatment of schooling. Most models of the veteran effect either omit schooling or treat it as exogenous. But military service and schooling are closely related (see, for example, Card and Lemieux, 2001; Angrist and Chen, 2008). For example, while military service provides the veterans a subsidized college education through GI Bill benefits; at the time of conscription, it also precludes some individuals from attaining their otherwise optimal level of schooling. Hence the interpretation of the estimates of military service depends on the role (and treatment) of schooling in the causal representation of its effect on subsequent civilian earnings (Rosenzweig and Wolpin, 2000).

The primary objective of our paper is to gauge the sensitivity of the estimates of military service to alternative treatments of schooling for the Vietnam era veterans, the last group of Americans who faced the draft. In our most general model, we treat schooling as well as military service as endogenous. Following Angrist (1989, 1990, 1991) and Angrist and Chen (2008) we use a set of variables capturing draft eligibility as instruments for veteran status, and following Card (1995, 1999, 2001) and Kling (2001) we use the presence of four-year accredited public and private colleges in the vicinity of the respondent's residence as instruments for schooling.

Our sample from the National Longitudinal Survey of Young Men (NLS-YM) is smaller than the Census samples used in recent IV work. This reduces the precision of the estimates. The issue is compounded by the fact that our close attention towards the exogeneity of instruments also led to the choice of instruments that are only weakly correlated with the endogenous regressors and thus reducing the precision of the estimates further. Nevertheless, we are still able to conclude that the veteran effect is negative. The effect is even more negative once we control for the individual's schooling and focus on the veteran effect net of schooling.

To help address the concern of misleading inference from the standard procedures due to the presence of the "weak instruments", we apply various weak instrument robust methods of inference and support our conclusions. We use the plug-in-based robust methods of inference on subsets of structural coefficients (associated with the endogenous regressors). These weak instrument robust methods were proposed by Stock and Wright (2000) and Kleibergen (2004, 2005); and, in this case, they accurately reflect the inadequacy of information in the data and lead to large confidence intervals for the veteran effect. Even so, we can strongly reject a zero or positive effect of the military service for the Vietnam era veterans.

That the weak instrument robust methods are still not routinely used by applied researchers is probably attributable to the fact that the validity of these methods for subsets of structural coefficients was only recently established by Kleibergen (2008) and Kleibergen and Mavroeidis

(2009a).[4] Hence a second objective of the present paper is to demonstrate the usefulness of these methods to validate conclusions from the standard procedures, if not use them as standards.

The rest of the paper is organized as follows. Section II provides background on the literature on the veteran effect and returns to schooling. Section III outlines the empirical analysis. The data are described in Section IV, and Section V presents the results. Section VI concludes.

II. **Background**

The Veteran Effect: Premium or Penalty?

It is unclear *a priori* whether military service increases or decreases earnings. On one hand, there are opportunities for human capital acquisition, which, for many, would not otherwise be available. The military provides on-the-job training, and college education is subsidized pre-service, in-service and post-service through GI Bills.[5]

Servicemen gain less measureable forms of human capital as well. For instance, the military serves as a "bridging environment" in which youths from disadvantaged backgrounds can learn less-observable skills such as an ability to function in a structured environment (Teachman and Call, 1996). Successful completion of a term of service signals favorable pre-market ability and acquired unobservable skills (DeTray, 1982). Taken together, these factors suggest that veterans will receive a premium when they return to the civilian sector.

Yet military service entails costs as well as benefits. Draftees are drawn away from their otherwise optimal human capital investment paths. So are young men who enlist in order to preempt

[4] To the best of our knowledge, these methods have, since then, found applications only in the literature on the new Keynesian Phillips curve (see, for example, Kleibergen and Mavroeidis, 2009b).
[5] Rostker (2006) outlines programs available from 1973 to 2004. New programs continue to be introduced. The best references on current offerings are the services' recruiting web pages.

being drafted into the infantry and those enlisting for non-financial motives such as patriotism or family tradition. Soldiers exposed to combat experience adverse physical and mental consequences. It is unclear whether, on net, military service increases or decreases earnings.

Estimates of the effect of military service vary by factors such as age, era, and approach to estimation. Early estimates are based on Ordinary Least Squares (OLS). Rosen and Taubman (1982) suggest a premium of 10 percent for World War II veterans and a penalty of 19 percent for Vietnam era veterans. There appears to be no effect of military service on the earnings of Korean War veterans (Schwartz, 1986). Other OLS-type studies report estimates that vary by service, rank and military occupational specialty. Air force veterans tend to earn more than veterans of other services (MacLean and Elder, 2007). Officers tend to fare better than enlisted personnel (MacLean, 2008). Technical skills transfer more readily to the civilian sector (Bryant and Wilhite, 1990; Goldberg and Warner, 1987). Blacks achieve greater premia and suffer smaller penalties than whites (Bryant, Samaranayake and Wilhite, 1993; Teachman and Tedrow, 2004). Costs of service are greatest for draftees and soldiers exposed to combat (Teachman, 2004; MacLean and Elder 2007).

Estimating the effect of military service on earnings poses an empirical challenge. As Rosen and Taubman (1982) note, military service is endogenous. The direction of the bias in the OLS estimate is ambiguous. On one hand, youths with better opportunities in the civilian sector will tend to opt out of the military. On the other hand, those with sufficiently low physical or cognitive ability will not qualify.

Instrumental variables techniques allow researchers to overcome the problem of endogeneity and obtain causal estimates of the veteran effect. Several studies exploit the randomness of draft lotteries as a valid instrument. Angrist (1989, 1990) finds a Vietnam era penalty of 15 percent for whites, but no effect for blacks. These studies suggest that OLS estimates of the losses due to service during the Vietnam era are biased away from zero. Angrist and Chen (2008) estimate long-run effects by capturing Vietnam era youths until 2000. They find that the Vietnam veteran penalty dissipates as men approach the overtaking point, where earnings profiles flatten (Mincer 1974).

Military Service and Schooling:

The interpretation of all these estimates hinges on the treatment of schooling in the estimating equation. There is an extensive literature on the returns to schooling emphasizing the fact that schooling is endogenous with respect to unobserved ability (see Card, 1999 for a survey). Those with more favorable labor market unobservables obtain more schooling, leading to an upward bias in the OLS estimates of the returns to schooling. On the other hand, measurement error in schooling will bias estimates downward. A variety of IV approaches have been used to generate (asymptotically) unbiased estimates. The literature generally reports IV estimates exceeding comparable OLS estimates.

There are several reasons to think that military service is related to schooling. First, the college tuition subsidies provide an incentive for continued schooling. Second, during the Vietnam era, potential draftees could defer their obligations by remaining in school. Card and Lemieux (2001) show that young men reaching draft age at the height of the draft were more likely to remain in college. Third, those with service-related disabilities may be less capable of returning to school; and finally, the unconstrained optimal level of schooling may exceed the optimal level subject to the constraint of service or draft eligibility. Angrist and Chen's (2008) Vietnam era veteran premium several decades beyond the military service is attributable to the additional education subsidized by the GI Bill.

The joint endogeneity of schooling and military service has implications for the estimates of the veteran effect. First, when both are treated as exogenous, OLS estimates will be biased. Second, when both are treated as endogenous, IV estimates with sufficiently large samples and appropriate instruments will be unbiased. In this case the estimated veteran effect, net of schooling, can be interpreted as the direct effect through, say, skill acquisition, loss of civilian labor market experience and adverse health outcomes. Third, when only veteran status is treated as endogenous, and schooling and veteran status are correlated, bias in the estimate of the returns to schooling can spread to the estimate of the direct effect of veteran status (Rosenzweig and Wolpin, 2000). Fourth, when schooling is omitted, the veteran effect, under some circumstances (mentioned in Section V), can be interpreted as composite effect of military service, i.e. the sum of its direct effect and also its indirect effect through schooling.

Our approach allows us to assess the sensitivity of estimates of the veteran effect to these alternative treatments of schooling. As in Angrist (1989), we use data from the National Longitudinal Survey of Young Men (NLS-YM) of men who were draft age during the Vietnam era. We use two sets of instrumental variables. The first set, following Angrist (1989, 1990, 1991) and Angrist and Chen (2008) characterize the draft eligibility of the respondents. The second, following Card (1995, 1999, 2001) and Kling (2001), capture proximity of the respondents' residence to an accredited four-year college.

Our sample is small relative to recent IV studies of the veteran effect and returns to schooling and estimates and specification tests can be sensitive to the presence of weak instruments. Therefore, we apply a variety of state-of-the-art approaches to gauge the properties of the instrumental variables estimators and guard against the perils of potential misspecification and the weak instrument problem whenever possible.

III. <u>Estimation</u>

Empirical Model:

We consider the following limited information model to estimate the net effect of a man's veteran status on his wage in the civilian market (later in his life cycle), after controlling for his years of schooling and other background characteristics. Let

$$w_i = V_i\gamma + S_i\beta + X_i\theta + \xi_i \tag{1}$$

where w_i is the logarithm of the real wage for the i-th man in the civilian labor market, V_i (=1) is a dummy variable indicating whether he ever served in the military, S_i is his years of schooling, and X_i contains an intercept term and a set of background variables including his demographic, household and locational characteristics. The error term ξ_i includes the unobservable human capital and the ability of the i-th man.

We are primarily interested in the coefficient γ. For small values of γ, this coefficient measures the net percent change in real wage attributable to veteran status, after controlling for years of schooling and other background characteristics. However, as pointed out by Halverson and Palmquist (1981), for not so small values of γ the appropriate measure should be $\delta = (e^{\gamma} - 1)$. Our estimates for γ are not small and hence we also report estimates for δ. The coefficient β measures the net returns to schooling, after controlling for the veteran status and other background characteristics, and is interesting in its own right.

The main challenges in conducting inference on γ are the endogeneity of V_i and S_i and the possible non-zero correlation between them. In the following we briefly reiterate some of the potential causes behind the mutual correlation between V_i, S_i and ξ_i that were discussed in Section II. It is quite likely that the characteristics (unobserved to the researcher) based on which the military accepted or rejected individuals, or the considerations that led individuals to volunteer for the service also affected their wages in the civilian labor market leading to a correlation between V_i and ξ_i (probably even after partialing out S_i).On the other hand, the return to schooling literature suggests that schooling is positively correlated with unobserved characteristics that affect wages positively. This, coupled with the prevalence of measurement error in schooling data, suggest a likely correlation between S_i and ξ_i (probably even after partialing out V_i).

The correlation between schooling and veteran status is also ambiguous *a priori*. While some veterans had to leave school early because they were drafted (or chose to leave school early to preempt being drafted into the infantry), there were others who went to college because their post service education was funded by the GI Bill. In fact, in our sample, empirically, veterans have slightly more schooling than the non veterans. A closer look reveals that of the 1080 veterans included in our sample, more than 61.5 percent went for additional schooling since their first (or only) term with the armed forces; and on average they got about 1.32 additional years of schooling. While it does not necessarily establish a causal effect of one's veteran status on schooling, this certainly calls for a thorough inspection of the treatment of schooling in the specification described by (1). We address this issue further in Section V while discussing our results.

Evaluating the Instrumental Variables Estimates

Likely endogeneity of veteran status and schooling and evidence of correlation between these variables suggest that simple OLS methods cannot consistently estimate the direct effect of veteran status net of schooling. IV is the most common method of inference in these situations. We use instrumental variables that can be broadly classified under two categories – (i) variables describing the draft eligibility of respondents and (ii) variables indicating the presence of an accredited four years college in the vicinity of the respondents' residence. A necessary condition for consistency of the IV estimators is that these instruments are exogenous which, in turn, implies that the variation induced by the instruments in the endogenous regressors is uncorrelated with the unobserved structural error ξ. Unfortunately, it is not possible to test exogeneity of the instruments without the prior assumption that the model is over-identified, i.e., there are at least three instruments, and at least two independent linear combinations of these instruments are exogenous. As discussed in Section V, we try to overcome this limitation of the test of exogeneity of the instruments by considering various alternative specifications while testing exogeneity (reported in Table 3 of the appendix) and also by using a joint test of hypotheses on parameter values and the exogeneity restrictions.

However, the close attention to exogeneity also leads us to be conservative in the choice of instruments and restricts us from capturing some variations in the endogenous explanatory variables veteran status and schooling. Ideally, asymptotic efficiency of the inference should be the only virtue at stake here. However, as Bound, Jaeger and Baker (1995) emphasize, this could also give rise to the so-called "weak instrument problems" in the usual asymptotic methods of inference based on the two-stage least squares (TSLS) framework. In such cases, TSLS estimates can be inconsistent and asymptotically biased, and the usual t-test and F-test tend to over-reject the true value of the parameters. These problems do not go away even with relatively large sample sizes. Hence, given the small number of observations in our sample, such problems are likely to be a major concern.[6]

---

[6] It is important to distinguish between the two types of problems that can arise due to weak instruments. The first problem is a reduction in precision; this is natural because the data do not contain enough information to precisely identify the parameters in the model. The second problem is the so-called "weak instrument problem" and this refers to the case where the conventional first-order asymptotic results provide poor approximation to the finite sample behavior of the estimators and tests; namely, the usual estimates tend to precisely report wrong values of the parameters and the usual tests tend to over-reject the true value of the parameters. Weak instrument robust methods were developed to address the second problem and overcome such misleadingly spurious precision in the usual methods of inference.

To overcome such problems we also consider the recently proposed “weak instrument robust” methods of inference. The broader aspects of our conclusion remain unchanged. The weak instrument robust methods provide a way for testing the parameters of interest, and then subsequently inverting the tests to obtain confidence regions for the parameters. Unlike the usual t-test and F-test, these tests are not over-sized even in finite samples (as long as the instruments are exogenous) and hence the asymptotic coverage probability of the corresponding confidence regions does not exceed their nominal counterparts. In particular, we report results based on the weak instrument robust subset KJ test that simultaneously tests for the individual coefficients and the over-identifying restrictions implied by the exogeneity of the instruments (see Kleibergen, 2008). Although this method can be conservative in finite samples; unlike the conventional methods, it does not, however, report incorrect parameter values with spuriously high precision or over-reject the true parameter value in the presence of weak instruments. However, even with its conservativeness, it allows us to strongly reject a zero or positive (direct) effect of veteran status.

Estimation Framework

In accordance with the limited information approach taken in this paper, we use the generalized method of moments (GMM) to infer on the parameters γ and β in (1). The moment restrictions for the inference are based on the following four instruments – (i) the lottery number assigned to the young man based on his date of birth, (ii) the lottery ceiling for the year when this young man attained draft age, (iii) a dummy variable indicating the presence of a four year accredited public college and (iv) a dummy variable indicating the presence of a four year accredited private college in the neighborhood of the young man’s residence in 1966. Denoting these four instruments generically by $\bar{Z}_i$ and letting $Z_i = [\bar{Z}_i, X_i]$, the assumption of exogeneity of the instruments and the background variables gives the moment restrictions (at the true value of the parameters)

$$E[Z_i'(w_i - V_i\gamma - S_i\beta - X_i\theta)] = 0 \text{ for all } i = 1, \dots, N. \tag{2}$$

We report the results of inference from the usual two-stage least squares methods based on the moment restrictions in (2). We also report the results of weak instrument robust inference from the Continuous Updating GMM based on the same set of moment restrictions. The particular weak instrument robust method, i.e. the subset KJ test, used here also allows us to simultaneously test for the moment restrictions specified by (2). The results are discussed in Section V.

IV. Data

We use data from the National Longitudinal Survey of Young Men (NLS-YM) to estimate the parameters in (1). The NLS-YM is a nationally representative data set of young men aged 14–24 in 1966. Respondents were followed annually until 1971, and then annually or biennially until 1981.

Men born between 1944 and 1952, who constitute about 82 percent of the survey respondents, were subject to the annual lotteries from 1969 through 1972. These men are the subject of our study. Veteran status is captured in two ways. First, there are a number of specific questions about military service. Second, the data indicate whether a respondent was unavailable because he was currently serving in the military. Schooling is measured as the highest grade completed reported on the survey.

The dependent variable, real hourly earnings, is measured in 1981 dollars at the oldest age at which the respondent appeared on the survey. In order to capture the effect of veteran status (and schooling) as late as possible in the man's life-cycle, we further restrict our attention to men whose last recorded wage was earned at the age of 29 or more.[7] Ignoring the 1.69 percent of respondents with missing wage figures, 65.75 percent of men interviewed in the survey earned their last wage at age 29 or more.[8] Lastly we ignore one respondent with an implausible birthday (04/31/1949) and three respondents with missing information on the type of area (urbanized, urban place or rural) of the respondent's residence in 1966. Our final sample consists of 2754 respondents.

In all, 1080, or 39 percent of the final sample, were veterans and 1674 were non-veterans. Sample size and reporting issues preclude us from disaggregating by rank, service or military occupation. Highest year of schooling completed was, on average, slightly higher for veterans (13.6) than non-veterans (13.4). However, the partial correlations of veteran status and schooling, controlling for the set of regressors used in the analysis is negative. These controls include race, region,[9] urbanicity[10] and the age and year at which the wage was earned.

The NLS-YM provides suitable instruments for both veteran status and schooling. Following Angrist (1989, 1990, 1991) and Angrist and Chen (2008) we use dimensions of draft status to instrument for military service; in particular we use the lottery number assigned to the individuals born between 1944 and 1952 and we use the ceiling of the draft-lottery announced for the year the individual became draft eligible. Following Card (1995, 1999, 2001) and Kling (2001), variables indicating the presence of four year accredited public and private colleges are used to instrument for schooling.

The full set of sample statistics is reported in Table 1.

---

[7] Among all the respondents born between 1944 and 1952, the wage figures are missing for 66 men and 6 reported 0 wage.

[8] 60.39 percent satisfies the stricter criterion of last recorded wage being earned at age 30 or more.

[9] The regions are northeast, mid-atlantic, east north central, west north central/mountain, east south central, west south central, pacific; and south atlantic as a default.

[10] The area is categorized as urbanized if its population is more than 125,000, as an urban place if the population is between 12,000 and 125,000, and rural otherwise.

V. Results

In order to illustrate the sensitivity of the estimates of veteran status to different treatments of schooling and treatment of potentially endogenous variables we compare five specifications of the relationship described in (1). The results are reported in Table 2. Corresponding results (estimates and standard errors) for the control variables are reported in Table 2(a).

Military service impacts earnings both directly, through, say, skill acquisition, loss of civilian labor market experience and adverse health outcomes, and indirectly through its association with schooling. Our most general IV specification, reported in column (A) separates the direct effect from the indirect effect. These results indicate a large veteran penalty. The coefficient γ of -.374 corresponds to a veteran effect of -31.2 percent ($\delta = e^{\gamma} - 1$). The standard error of this estimate is rather large, around 15 percent (obtained by the Delta method); and a 95 percent confidence region suggests that the wage reduction for veterans can vary from 1 percent to 61 percent. Nevertheless, we can safely reject that the net veteran premium is zero (or positive). On the other hand, the estimate of the returns to schooling of 16.1 percent (p-value = .078) is comparable to Card's (1995).

Specification (B) mirrors studies that estimate the direct effect of veteran status assuming that the variation in the veteran status induced by the draft lottery is uncorrelated with completed schooling. This is not true in our sample. An alternative interpretation is that the coefficient of veteran status in such models represents the composite veteran effect, i.e. the sum of the direct effect as measured in specification (A) and the indirect effect operating through schooling, for instance through the GI bill which is available only to veterans. Under this alternative interpretation, the estimate of the composite veteran effect of -15.8 percent (p-value =.14) which is greater than the estimate of the direct effect, consistent with the case in which the returns to schooling are positive and schooling and veteran status are positively correlated. However, as noted by Rosenzweig and

Wolpin (2000), schooling and unobserved ability may be correlated due to the fact that there is another effect of military service on completed schooling that arises from the interruption of schooling (for those who could not complete their schooling because they were drafted), and hence the interpretation of the coefficient of veteran status in Specification (B) is unclear.[11]

Specifications (C) and (D) are the OLS analogs to specifications (A) and (B). We would expect that, for the Vietnam era, veteran status will be negatively correlated with the earnings equation unobservables, biasing the OLS estimate downward. We would also expect the returns to schooling to be positively correlated with those unobservables, biasing the OLS coefficient upward. Instead, we find the opposite: The OLS estimate of the veteran effect is greater (i.e., more positive) than the IV estimate and the OLS estimate of the returns to schooling is smaller than the IV estimate. The estimate of the returns to schooling when schooling is treated as exogenous in Specifications (C) and (E) is 4.9 percent (p-value = .003) and is about 1.5 percentage points lower than Card's (1995).

The endogeneity tests indicate that both schooling and veteran status are endogenous. But are the OLS estimates significantly different from the IV estimates? In terms of Specification (A), estimate of the net effect, the IV estimates indicate that both schooling and veteran status are endogenous. However, using the Hausman test we cannot reject that the difference between the probability limits of the IV and OLS estimates are significant (p-value =.158).[12] In other words, while the endogeneity tests indicate that OLS is misspecified we cannot say whether the misspecification is sufficient to generate a "significant" asymptotic bias. Of course, this may be due

[11] Even if we assume that pre-service schooling is exogenous and has no relation with veteran status, and consider a simple model where veteran status only affects schooling through the post-service subsidy in college education, Specification (B) may not be adequate to estimate the composite effect of veteran status (see Joffe et.al., 2008).

[12] To see if the conclusion from the Hausman test is affected by the presence of weak instruments, we use all three forms of the statistic described in equation 3.9 (page 568) of Staiger and Stock (1997). The conclusion does not change with the other forms of the Durbin-Wu-Hausman statistic. These tests, under weak instruments, lack power.

to lack of precision caused by the low correlation of the instruments with veteran status and schooling, and relatively small sample size.

Many studies of the veteran effect control for schooling but do not have data to instrument for it. Specification (E) is that model. The point estimate of the veteran effect, - 20.9 percent, lies between the IV estimates of the "so-called" composite and the direct effect of veteran status. The estimate lacks precision and is not statistically significant.

Are these results impacted by weak instrument issues?

Yes. The first stage F-statistics for testing the relevance of the (excluded) instruments are low: 8.46 for veteran status and 2.53 for schooling. The partial R^2 statistics are .012 and .004 respectively for the two endogenous regressors (see Shea, 1997). Hence there is evidence that the instruments do not explain much variation of the endogenous regressors, especially schooling.[13] A more systematic test for weak instruments is the test proposed by Stock and Yogo (2005). This test suggests that given our model and the exogenous instruments, the maximum (asymptotic) bias of the TSLS estimators of γ and β, relative to their OLS estimators is more than 30 percent. If the instruments were strongly correlated with the endogenous regressors, one would expect this to be close to 0. The test by Stock and Yogo also suggests that the nominal size of 5 percent Wald test for jointly testing the significance of veteran status and schooling is likely to be more than 25 percent. Again, if the instruments were strongly correlated with the endogenous regressors, this would be close to 5 percent.

To gauge how seriously these problems affect our overall results, we followed the recently proposed weak-instrument-robust methods of inference. These methods are valid as long as the instruments are exogenous.

[13] The Anderson LM statistic, however, rejects the hypothesis of under-identification in the model at 5.5 percent level.

Are the instruments exogenous?

It is reassuring to observe that the over-identification test cannot reject the exogeneity of the instruments even at 97 percent level.[14] This should be hardly surprising since we ended up with weak instruments in the first place because we were too careful to ensure the exogeneity of the instruments.

Intuitive justification of the exogeneity of these instruments is provided in the original papers by Angrist (and his co-authors) and Card (and his co-authors). For example, one could argue that the lottery ceiling for the draft years were determined independently of the individuals unobserved characteristics. The lottery number assigned to each man was based on his date of birth and arguably uncorrelated with unobserved individual characteristics. It is, however, less straightforward to intuitively justify the exogeneity of the other instruments, based on the presence of four year accredited colleges in the vicinity of the respondent's residence in 1966. Nevertheless, given that the exogeneity of lottery ceiling and lottery number is more convincing, in Table 3 we have tested the exogeneity of the variables indicating the presence of colleges (individually and jointly) under the assumption that the variables involving lottery are exogenous. The minimum p-value for the over-identification test is 84 percent; and for the specification (A), that we actually use, the p-value is more than 97 percent. Hence, we conclude that the data supports the exogeneity of the instruments used in the IV regression.[15,16]

---

[14] Unlike some other studies, the p-value in this case is 97 percent and is possibly large enough to buffer for the fact that the over-identification test may lack power in certain directions.

[15] We also use the H_3 (for columns 1 and 2 of Table 3) and the H_4statistics (for all the columns) described in Hahn, Ham and Moon (2008) to test for exogeneity of the instruments. The p-value for all these tests exceeds 95 percent and hence strongly supports the exogeneity hypothesis. The results are not reported here because the weighting matrix of the quadratic form is near-singular in all cases and there may be some concern with the ill-conditioned computations.

[16] We speculate that the results of the over-identification tests using alternative sets of instruments indicate that the usual interpretation of the TSLS estimator as the local average treatment effect (on the compliers) could possibly be extended to an interpretation as the average treatment effect on the entire population under reasonable assumptions (see Angrist, 2004). However, this is beyond the scope of the present paper and is not pursued any further.

Results from weak-instrument-robust methods of inference:

We use the subset KJ test to test different hypothesized values of the parameters $\gamma$and β. This test was proposed by Kleibergen (2004, 2005) and the validity of the test for individual structural coefficients (associated with the endogenous regressors) was established by Kleibergen (2008) and Kleibergen and Mavroeidis (2009a). One important reason for using this test is the following: it simultaneously tests for the moment restrictions in (2) at the hypothesized value of the structural parameter. Recall that the moment restrictions were implied by the exogeneity of the instruments. Thus a confidence interval obtained by inverting this test will not only have the correct asymptotic coverage probability (because it is weak instrument robust) but also the exogeneity of instruments will be satisfied at each point belonging to this interval. A 95 percent confidence interval for γ, obtained by collecting all the values of the parameter that cannot be rejected by a 5 percent subset KJ test, can vary from -1 percent to -121 percent, and hence the direct veteran effect (net of schooling) can vary from a wage reduction of 1 percent to 70 percent (obtained by projection). [17] Of course, this is very imprecise; the test is conservative in the presence of weak instruments. However, it is also interesting to note that, even with such degree of imprecision, we can reject a zero or positive net effect of veteran status.

A 95 percent confidence region for β, on the other hand, shows that the increase in wage due to an additional year of schooling can vary from 3.5 percent to 54.5 percent.

It is also reassuring to note that the TSLS estimates (that are not supposed to be robust to weak instruments) are also included inside these robust confidence regions, showing that our main results based on TSLS are not terribly misleading in this context.

[17] We also tried the other plug-in based weak-instrument robust tests, such as the subset AR test and the subset conditional likelihood ratio test. However, while the former gives unbounded confidence regions (it is known to be more conservative in general), the latter gives confidence regions that are very similar to those obtained from the subset KJ test. Hence we do not report them for brevity.

VI. **Conclusion**

Estimates of the effect of military service vary by era, age and methodology. We focus on the third issue using a sample of relatively young veterans of the Vietnam era. Two methodological issues are the joint endogeneity of both military service and schooling, and the potential weakness of the instruments. The sample size (N=2754) is also relatively small for microeconometric research and results in lower precision than we would like.

Point estimates suggest a veteran penalty of about 20.9 percent when schooling is treated as exogenous and 31.2 percent when schooling is treated as endogenous. OLS estimates are positive and small. The IV estimate of the effect of military service, composite of schooling, is negative 15.8 percent. Rosenzwieg and Wolpin’s (2000) point that schooling is endogenous is validated; but, in our sample, it does not seem to cause a "statistically significant" bias in the estimate of veteran effect.

Still, our result has a substantive implication. Approximately 9-10 years after Vietnam era service, veterans suffer significant penalty.

The exercise is one of the first applications of many new techniques for evaluating properties of instrumental variables estimators and dealing with weak instruments. The focus on a model with two endogenous variables and the use of a cross-section microeconometric data set are also novel. We hope this paper will provide a guide for other researchers applying the state-of-the art approaches to instrumental variables models.

References

Angrist, J. 1989. Using the Draft Lottery to Measure the Effects of Military Service on Civilian Earning. *Research in Labor Economics* 10, Ronald Ehrenberg (ed.) Greenwich, CT, JAI.

Angrist, J. 1990. Lifetime Earnings and the Vietnam Era Draft Lottery: Evidence from Social Security Administrative Records. *American Economic Review* 80: 313 – 336.

Angrist, J. 1991. The Draft Lottery and Voluntary Enlistment in the Vietnam Era. *Journal of the American Statistical Association* 86: 584 – 595.

Angrist, J. 2004. Treatment Effect Heterogeneity in Theory and Practice. *The Economic Journal* 114: 52 – 83.

Angrist, J. and S. Chen. 2008. Long-Term Economic Consequences of Vietnam-Era Conscription: Schooling, Experience and Earnings. IZA DP No. 3628.

Bound, J., D. A. Jaeger, and R. M. Baker. 1995. Problems with Instrumental Variables Estimation when the Correlation Between the Instruments and the Endogenous Explanatory Variable is Weak. *Journal of the American Statistical Association* 90: 443 – 450.

Bryant, R. and A. Wilhite. 1990. Military Experience and Training Effects on Civilian Wages. *Applied Economics* 22: 69 – 81.

Bryant, R., V. Samaranayake and A. Wilhite. 1993. The Effect of Military Service on the Subsequent Civilian Earnings of the post-Vietnam Veteran. *Quarterly Review of Economics and Finance* 33: 15 – 31.

Card, D. 1995. Using Geographical Variation in College Proximity to Estimate the Return to Schooling. *Aspects of Labour Market Behavior: Essays in Honor of John Vanderkamp*, Christofides, L. N., E. K. Grant and R. Swidinsky (eds). Toronto: University of Toronto Press.

Card, D. 1999. The Causal Effect of Education on Earnings. *Handbook of Labor Economics* 3, Orley Ashenfelter and David Card (eds). Amsterdam: Elsevier.

Card, D. 2001. Estimating the Return to Schooling: Progress on Some Persistent Econometric Problems. *Econometrica* 69: 1127 – 1160.

Card, D. and T. Lemiux. 2001. Draft Avoidance and College Attendance: The Unintended Legacy of the Vietnam War. *American Economic Review Papers and Proceedings* 91: 97 – 107.

De Tray, D. 1982. Veteran Status as a Screening Device. *American Economic Review* 72: 133 – 142.

Goldberg, M. and J. Warner. 1987. Military Experience, Civilian Experience and the Earnings of Veterans. *Journal of Human Resources* 22: 62 – 81.

Hahn, J., J. Ham and H. R. Moon. 2008. The Hausman Test and Weak Instruments. Technical Report, Department of Economics, University of California and University of Southern California.

Halvorsen, R. and R. Palmquist. 1980. The Interpretation of Dummy Variables in Semilogarithmic Equations. *American Economic Review* 70: 474 – 475.

Kleibergen, F. 2004. Testing Subsets of Parameters in the Instrumental Variables Regression Model. *The Review of Economics and Statistics* 86: 418 – 423.

Kleibergen, F. 2005. Testing Parameters In GMM Without Assuming That They Are Identified. *Econometrica* 73: 1103 – 1123.

Kleibergen, F. 2008. Subset Statistic in the Linear IV Regression Model. Technical Report, Department of Economics, Brown University.

Kleibergen, F. and S. Mavroeidis. 2009a. Inference on subsets of parameters in GMM without assuming identification. Technical report, Department of Economics, Brown University.

Kleibergen, F. and S. Mavroeidis. 2009b. Weak Instrument Robust Tests in GMM and the New Keynesian Phillips Curve. *Journal of Business and Economic Statistics* 27: 293 – 311.

Kling, Jeffrey R. 2001. Interpreting Instrumental Variables Estimates of the Returns to Schooling. *Journal of Business and Economics Statistics* 19: 358 – 364.

Joeffe, M. M., D. Small, T. T. Have, S. Brunelli, H. I. Feldman. 2008. Extended Instrumental Variables Estimation for Overall Effects. *The International Journal of Biostatistics* 4, Article 4.

MacLean, A. and G. Elder. 2007. Military Service in the Life Course. *Annual Review of Sociology* 33: 175 – 196.

MacLean, A. 2008. The Privileges of Rank: The Peacetime Drat and Later-life Attainment. *Armed Forces and Society* 34: 682 – 713.

Mincer, J. 1974. *Schooling, Experience and Earnings*, New York: Columbia University Press.

Oi, Walter. 1967. The Economic Cost of the Draft, *American Economic Review* 67: 39 – 62.

Rosen, S. and P. Taubman. 1982. Changes in Life-Cycle Earnings: What Do Social Security Data Show? *The Journal of Human Resources* 17: 321 – 338.

Rosenzweig, M. R. and K. L. Wolpin. 2000. Natural "Natural Experiments" in Economics. *Journal of Economic Literature* 38: 827 – 874.

Rostker, B. 2006. I Want You: The Evolution of the All-Volunteer Force. *RAND Corporation*.

Schwartz, S. 1986. The Relative Earnings of Vietnam and Korean-Era Veterans. *Industrial and Labor Relations Review* 39: 564 – 572.

Shea, J. 1997. Instrument Relevance in Multivariate Linear Models: A Simple Measure. *The Review of Economics and Statistics* 79: 348 – 352.

Staiger, D. and J. H. Stock. 1997. Instrumental Variables Regression with Weak Instruments. *Econometrica* 65: 557 – 586.

Stiglitz, J. and L. Bilmes. 2008. *The Three Trillion Dollars War,* W.W. Norton & Co.

Stock, J. H. and J. H. Wright. 2000. GMM with Weak Identification. *Econometrica* 68: 1055 – 1096.

Stock, J. H. and M. Yogo. 2005. Testing for Weak Instruments in Linear IV Regression. *Identification and Inference for Econometric Models: Essays in Honor of Thomas Rothenberg*, D. W. K. Andrews and J. H. Stock (eds), Cambridge: Cambridge University Press: 80 – 108.

Teachman, J and V. Call 1996. The Effect of Military Service on Educational, Occupational and Income Attainment. *Social Science Research* 25: 1 – 31.

Teachman, J. and L.M. Tedrow. 2004. Wages, Earnings, and Occupational Status: Did World War II Veterans Receive a Premium? *Social Science Research* 33: 581 – 605.

Teachman, J. 2004. Military Service during the Vietnam Era: Were There Consequences for Subsequent Civilian Earnings? *Social Forces* 83: 709 – 730.

Teachman, J. 2005. Military Service in the Vietnam Era and Educational Attainment. *Sociology of Education* 78: 50 – 68.

Table 1: Descriptive Statistics

Variables	Mean (s.d.)		
	Overall	Veterans	Not Veterans
log(real wage: 1981$)	6.734 (.502)	6.761 (.476)	6.717 (.517)
Veteran (proportion)	.392 (.488)	-	-
Schooling: Highest year completed	13.49 (2.67)	13.562 (2.150)	13.439 (2.959)
Black	.252 (.434)	.224 (.417)	.270 (.444)
Proportion of men whose wage is from the year:			
1975	.020 (.139)	.021 (.144)	.019 (.135)
1976	.027 (.162)	.023 (.150)	.029 (.169)
1978	.048 (.214)	.053 (.224)	.045 (.208)
1980	.088 (.283)	.090 (.286)	.087 (.281)
1981	.811 (.392)	.809 (.393)	.812 (.391)
Age at which wage is earned	32.354 (2.289)	32.506 (2.186)	32.257 (2.349)
Residence at the age of 14 (South-Atlantic is omitted category)			
Northeast	.040 (.196)	.040 (.196)	.040 (.196)
Mid-Atlantic	.161 (.367)	.150 (.357)	.168 (.374)
East North Central	.186 (.389)	.191 (.393)	.182 (.386)
West North Central	.095 (.294)	.124 (.330)	.077 (.268)
East South Central	.098 (.297)	.089 (.285)	.104 (.305)
West South Central	.115 (.319)	.101 (.301)	.124 (.330)
Pacific	.089 (.285)	.088 (.283)	.090 (.286)

Table 1: Descriptive Statistics (continued)

Variables	Mean (s.d.)		
	Overall	Veterans	Not Veterans
Type of area in 1966 (Rural is the omitted category)			
Urbanized	.434 (.496)	.452 (.498)	.422 (.494)
Urban place	.165 (.371)	.169 (.375)	.162 (.368)
Instrumental Variables			
Lottery Number	181.566 (103.689)	173.426 (104.446)	186.817 (102.888)
Lottery Ceiling	180.697 (30.134)	184.389 (26.463)	178.315 (32.065)
Proportion with at least one 4 year accredited college in the neighborhood			
Private College	.580 (.494)	.596 (.491)	.569 (.495)
Public College	.481 (.500)	.494 (.500)	.473 (.499)
Total Number of Observations	2754	1080	1674

Table 2: Regression Results from Equation (1) [18]

		Specifications				
		(A)	(B)	(C)	(D)	(E)
Method of estimation		IV	IV	OLS	OLS	IV
Veteran is treated as		endogenous	endogenous	exogenous	exogenous	endogenous
Schooling is treated as		endogenous	excluded	exogenous	excluded	exogenous
Veteran	Coefficient: γ	-.374* (.222)	-.172 (.168)	.019 (.018)	.019 (.018)	-.234 (.165)
	Effect: $\delta = e^{\gamma} - 1$	-.312** (.153)	-.158 (.141)	.019 (.018)	.019 (.018)	-.209 (.131)
Schooling		.161** (.078)	-	.049*** (.003)	-	.049*** (.003)
Sargan-statistic Test of over-identification		.044 (.978)	6.085 (.108)	-	-	3.084 (.379)
Test of Endogeneity	For Veteran	4.639 (.0312)	1.360 (.244)	-	-	2.573 (.109)
	For Schooling	3.019 (.082)	-	-	-	-
	For Veteran and Schooling	5.820 (.055)	-	-	-	-
Hausman Test (use only veteran and schooling)	Compare with (C)	3.675 (.159)	-	-	-	2.385 (.304)
	Compare with (E)	2.055 (.358)	-	-	-	-
Anderson LM statistic Test of under-identification		7.601 (.055)	33.65 (.000)	-	-	33.687 (.000)
Partial R^2 (Shea)	Veteran	.012	.012	-	-	.012
	Schooling	.004	-	-	-	-
F-stat for instruments	Veteran	8.46	8.46	-	-	8.46
	Schooling	2.53	-	-	-	-
Test of Weak Identification: Stock and Yogo (2005)	Cragg-Donald Statistics	1.894	8.464	-	-	8.47
	Bias of IV relative to OLS	more than 30%	between 10% - 20%	-	-	between 10% - 20%
	Size of 5% Wald-test	more than 25%	between 20% - 25%	-	-	between 20% - 25%

[18] Results are based on 2754 observations. Rows corresponding to the coefficients contain the standard errors within parentheses. *, ** and *** represent significance at the 10 percent, 5 percent and 1 percent level respectively. Rows corresponding to the specification tests (endogeneity, over and under identification) report the test statistic and the p-values (within parentheses).

Table 2(a): Regression Results from Equation (1) [19]

	Specifications				
	(A)	(B)	(C)	(D)	(E)
Method of estimation	IV	IV	OLS	OLS	IV
Veteran is treated as	endogenous	Endogenous	exogenous	exogenous	endogenous
Schooling is treated as	endogenous	Excluded	exogenous	excluded	exogenous
Year in which wage is earned[20]	-.027*** (.010)	-.015** (.007)	-.018*** (.006)	-.014** (.007)	-.018*** (.007)
Age at which wage is earned	.029*** (.005)	.030*** (.005)	.027*** (.004)	.028*** (.004)	.030*** (.004)
Black	-.111 (.098)	-.305*** (.025)	-.231*** (.023)	-.294*** (.023)	-.245*** (.025)
Region: Northeast	.059 (.061)	.069 (.052)	.079 (.048)	.079 (.050)	.066 (.050)
Region: Mid-Atlantic	.026 (.061)	.121*** (.034)	.109*** (.030)	.134*** (.031)	.092*** (.033)
Region: East North Central	.074 (.057)	.164*** (.031)	.143*** (.029)	.169*** (.030)	.137*** (.030)
Region: West North Central	-.011 (.070)	.098** (.039)	.044 (.034)	.083** (.036)	.064* (.038)
Region: East South Central	.053 (.042)	.056 (.035)	.064* (.033)	.063* (.034)	.055 (.035)
Region: West South Central	.027 (.043)	.048 (.035)	.058* (.031)	.061* (.033)	.042 (.034)
Region: Pacific	.042 (.071)	.151*** (.039)	.130*** (.036)	.161*** (.038)	.118*** (.038)
Area: Urbanized	.006 (.069)	.138*** (.022)	.085*** (.020)	.128*** (.020)	.097*** (.022)
Area: Urban place	-.030 (.054)	.060** (.027)	.026 (.025)	.055** (.026)	.032 (.026)
Intercept	5.96*** (.769)	6.929*** (.514)	6.56*** (.485)	6.88*** (.504)	6.630*** (.504)

[19] Results are based on 2754 observations. Standard errors are reported within parentheses. *, ** and *** represent significance at the 10 percent, 5 percent and 1 percent level respectively.
[20] Had this been the variable of interest, once should use dummies to control for the years in which wage is earned to obtain practically meaningful coefficients.

Table 3: Testing Exogeneity/Orthogonality restrictions in Equation (2)

Orthogonality of instruments (tested)	4 year public college	4 year private College	4 year public college	4 year private college	4 year public 4 year private
C-statistic	.041	.043	.001	.003	.044
(p-value)	(.839)	(.836)	(.979)	(.957)	(.978)
Sargan-statistic	.041	.043	.044	.044	.044
(p-value)	(.839)	(.836)	(.978)	(.978)	(.978)
Instruments used in the model	1) 4 year public 2) Lottery number 3) Ceiling in Lottery	1) 4 year private 2) Lottery number 3) Ceiling in Lottery	1) 4 year public 2) 4 year private 3) Lottery number 4) Ceiling in Lottery	1) 4 year public 2) 4 year private 2) Lottery number 3) Ceiling in Lottery	1) 4 year public 2) 4 year private 3) Lottery number 4) Ceiling in Lottery