

# The Effect of Serving in Vietnam on Smoking Behavior and other Measures of Health

Daniel Eisenberg\*and Brian Rowe†

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## Abstract

Men who served in the military during the Vietnam era had access to free or inexpensive cigarettes. We use the Vietnam War draft lottery, which randomly assigned draft numbers based on birth dates, to construct instrumental variable estimates of the effects of service on smoking and other health-related measures later in life. We find that serving in the war increased the probability of smoking by 30 percentage points as of 1978-80 (when men in the relevant cohorts were aged 25-30), much more than suggested by naive OLS estimates. This large effect, however, dissipates to being statistically insignificant as of 1997-2003 (when they were 45-53). The results provide new evidence regarding the responsiveness of smoking behavior to reduced prices. The results also suggest a high malleability of smoking behavior over time. The sharp increase in the likelihood of smoking due to service in young adulthood did not persist into middle age.<sup>1</sup>

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\*University of Michigan, Department of Health Management and Policy

†University of Michigan, Department of Economics

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## Introduction

Veterans of the Vietnam War are significantly more likely to smoke than non-veteran males of the same ages (Stellman et al 2000, Klevens et al 1995). Furthermore, the lifetime risk of five types of smoking-related cancers is twice as high among the Veteran Administration (VA) population as among the general male population of the same age (Harris et al 1989). This evidence suggests that military service in Vietnam may have caused smoking to increase. Indeed, there are various plausible mechanisms for such an effect. First, men who served in Vietnam faced significantly reduced prices for cigarettes. Men in combat or other places where cooking was not feasible received free cigarettes in C rations and other servicemen could buy cigarettes at wholesale, tax-free prices at military bases and commissaries. Second, peer effects may have augmented the effect of reduced prices, given the concentration of men at bases and other military facilities. Third, servicemen in Vietnam were subjected to a host of stressful and traumatic experiences. For some men these experiences could have caused depressive or anxiety disorders, both of which are associated with an increased risk of smoking (Glassfield et al 1990, Winefield et al 1989).

The association between military service and smoking later in life may also be explained by selection on unobservable characteristics. One plausible difference is that veterans are less risk averse on average than non-veterans. Another possibility is that veterans have a higher subjective rate of time preference than non-veterans, so that veterans tend to put relatively less value on the future. If either of these differences in unobservable factors is true, we would expect veterans to be more likely to smoke than nonveterans, even in the absence of any causal effect.

We use the Vietnam War draft lottery, which randomly assigned draft numbers based on birth dates, to address this potential selection bias and estimate the effects of service on smoking and other health-related measures of health later in life. Specifically, we address two questions: 1) What was the effect of serving in Vietnam on smoking behavior and self-reported health shortly after the war (in the late 1970s)?; 2) If these effects occurred, to what extent did they persist over the next twenty five years (1980-2003)?

This analysis contributes policy-relevant information in two respects. Most directly, it provides evidence on the effects of military service on smoking and general health. This may be useful for evaluating the consequences of future engagements and planning for related health services. In addition, because our analysis focuses on changes in smoking in young adulthood that are randomly induced, it offers an opportunity to examine whether changes in smoking behavior in young adulthood cause changes in smoking behavior in later adulthood. It is well known from epidemiological research that smoking in later life is highly correlated with smoking earlier in life (e.g. Janson 1999). This relationship may be causal or it may be related to unmeasured

factors that persist over time.

In our main methodological approach, we estimate two-stage regressions in which the first stage predicts the probability of serving in Vietnam and the second stage predicts the health-related outcome (smoking or self-reported health). The key instrumental variable in the first stage is whether or not a man had a draft number (based on birth date) below or above the cut-off number determined by the Selective Service in that year. The draft lotteries were held in 1970-73 to make selection into service a fairer process. Men with numbers below the cut-off were considered draft-eligible and were thereby more likely to end up serving. Previous studies have employed this natural experiment to look at the effects of serving on mortality (Hearst et al 1986) and earnings (Angrist 1990).<sup>2</sup>

For the purpose of comparison, we also conduct our analysis with an alternative instrumental variables approach, in which the identification is driven by differences in the probability of serving in the war across adjacent male cohorts, using female cohorts as controls. This approach was used by Bedard and Deschenes (2006) to examine the effects of serving in World War II and the Korean War on smoking and other health outcomes later in life. Variants of this approach have also been used in many other contexts in labor and health economics (e.g. Bound and Turner 1999; Card and Lemieux 2001). One limitation of this type of approach is that it may falsely attribute to the exposure of interest any across sex-cohort differences in the outcome of interest that are actually due to unmeasured factors. By contrast, the draft lottery provides an essentially randomized design, affording an opportunity to assess the robustness of the across-sex-cohort approach.

To preview, using the draft lottery approach we find that the effect of serving in Vietnam on smoking as of 1978-80 (when the affected birth cohorts were 26-30 years old) was on the order of 30 percentage points but this effect dissipated enough to be statistically indistinguishable from zero by 1997-2003 (when the men were 45-55). The pattern for self-reported health was similar, with a large decrease in the probability of reporting excellent health in 1978-80 but no difference in the later samples. The most likely mechanism for the large effect in the earlier sample seems to be increased access to cigarettes through reduced prices, as opposed to stress and trauma, given that major combat in Vietnam ended shortly after the first draft lottery was held. In addition, despite strong associations over time in smoking behavior in epidemiological literature, our results suggest

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<sup>2</sup>Other researchers have successfully used the Vietnam draft lottery to estimate causal effects of military service on a variety of outcomes. Hearst, Newman, and Hulley (1986) use the the draft lottery to estimate the effect of military service on mortality rates later in life. They find that a small but significant effect of service in Vietnam on total mortality, and larger effects on suicide and motor vehicle related mortality. Angrist (1990) uses the lottery to estimate the effect of military service on lifetime earnings. Angrist finds a significant earnings penalty from military service for white veterans relative to comparable white non-veterans.

that exogenously increasing the probability of smoking in young adulthood does not necessarily increase the likelihood of smoking in later adulthood. Finally, replicating the analysis with the across-cohort IV approach yields different results for some outcomes and may thereby highlight limitations of this widely used method.

## Data and Construction of Instruments

Our data source is the National Health Interview Survey (NHIS)<sup>3</sup>, conducted by the Centers for Disease Control (CDC). The NHIS is an annual survey which asks various health-related questions to a nationally representative random sample. By combining data from different NHIS survey years together, we create a repeated cross-section data set. Although the NHIS data goes back to 1969, questions related to smoking were not asked in every survey. The NHIS contains data on smoking related questions in the 1978, 1979, and 1980 surveys, and also in every survey year from 1997 through 2003.

### Instrumental variables based on the draft lottery

The Vietnam draft lottery randomly assigned risk of induction into the military to individual men based on date of birth. However, information on respondent's date of birth is not available in the public use NHIS data files. To obtain this information, we applied for and received approval to access the CDC's in-house NHIS survey data at a secure facility.<sup>4</sup> The data files we use for analysis were created by combining the data on respondent's dates of birth with the publicly available data on respondent's answers to NHIS survey questions.

To construct valid instruments for selection into the military, we need to understand precisely how the draft lottery operated. The Vietnam draft lottery was administered by the U.S. Selective Service Administration. Lottery numbers ran from 1 to 365, with lower numbers indicating a higher priority for induction. For each year from 1970 to 1973, the Selective Service held a televised drawing to randomly match birthdays to lottery numbers. Each year, the lottery applied to men from specific birth cohorts. Therefore, the key step in constructing instruments is to assign respondents in the NHIS the appropriate lottery number, based on their year and day of birth. For each year of the lottery, we obtain the mapping from date of birth to lottery number using tables published by the Selective Service.<sup>5</sup>

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<sup>3</sup><http://www.cdc.gov/nchs/nhis.htm>

<sup>4</sup>We thank Bob Krasowski and Chris Rodgers for retrieving the birthday data from archived mainframe data files.

<sup>5</sup><http://www.ssa.gov>

After the lottery was held, the Defense Department would announce a draft eligibility cutoff number. Men with lottery numbers above the cutoff were at no risk of induction into the military. The Defense Department did not announce the cutoff number immediately after the lottery was held, and in fact there was usually a delay of several months from the time the lottery was held until the eligibility cutoff was announced. For each year from 1970-1973, different birth cohorts of men were subjected to the draft lottery. For instance, the 1971 draft lottery applied to men born in 1951. Table (1) summarizes this basic information about the draft lottery.

Year	Cohort(s) Affected	Eligibility Cutoff
1970	1944-1950	195
1971	1951	125
1972	1952	95
1973	1953	N.A.

Table 1: Draft eligibility cutoff number by birth cohort and year.

Using the information from Table (1) together with the Selective Service tables which map birth dates to lottery numbers, we can construct a valid instrumental variable. First we assign appropriate lottery numbers to men from the 1950-1953 birth cohorts. Next we create our instrument as a dummy variable Draft equal to 1 for men whose lottery numbers were below the eligibility cutoff in the year they were subjected to the lottery, and 0 otherwise. In other words, men who were eligible for induction have Draft equal to 1, while men who were at no risk of induction have Draft equal to 0.

In our main analyses, we actually use 3 instruments: The Draft variable interacted with a dummy for each of the 1950, 1951, and 1952 birth cohorts. This improves the fit of our first stage because the proportions of draft eligible and ineligible men who ended up serving in the military varies across the 1970-1972 lotteries.

We leave out the 1944-1949 birth cohorts because Angrist (1990) finds that most men from these cohorts who served in Vietnam entered the military before 1970. Therefore, men from these cohorts who avoided service until 1970 do not constitute a representative sample. To ensure that our analysis is free of selection bias, we must drop the 1944-1949 birth cohorts from our sample.

Even though no men from the 1953 birth cohort were actually drafted in the 1973 lottery, we include the 1953 cohort in our sample. This is because the Defense Department took several months after the 1973 lottery to decide that no additional inductees were needed. Thus the 1953 cohort is an interesting case. Men from this cohort with low lottery numbers would

have initially perceived a high risk of induction, until the announcement that no additional men would be drafted in 1973. In anticipation of being drafted, these men had an incentive to enlist in order to receive more favorable terms of service in the armed forces. In our analysis we include the 1953 cohort, even though all men in this cohort are draft ineligible.<sup>6</sup>

Another available instrument is the true proportion of men by birth cohort, race, and group of 5 consecutive lottery numbers who served in Vietnam. This data was collected by the Defense Manpower Data Center (DMDC) and was obtained and used by Angrist (1990).

A problem with using the DMDC data is that it is only available for the 1951-1953 cohorts. As nobody was actually drafted from the 1953 cohort, using the DMDC data as an instrument restricts us to using only 2 birth cohorts from which men were actually drafted into military service. Having to drop the 1950 cohort from the analysis may outweigh the benefits from using the DMDC data.

### **Veteran status**

In the 1978, 1979 and 1980 NHIS, respondents were asked about their status as veterans. If the respondent was a veteran, the conflict in which he was involved was also recorded. This allows us to precisely identify veterans of the Vietnam War in the 1978-1980 surveys.

The identification of veterans is somewhat different for the 1997-2003 NHIS data. This is because the survey question we must use to identify Vietnam veterans is less precise than in the 1978-1980 surveys. The information we would like to know is "Are you a Vietnam veteran?" However, respondents in the 1997-2003 surveys were asked "Have you ever been honorably discharged from active duty in the [Armed Forces]?"<sup>7</sup> This formulation overstates the numbers of Vietnam veterans by including respondents who enlisted in a branch of the armed forces after the Vietnam war, and understates the number of Vietnam veterans by omitting those who were dishonorably or generally discharged.

When we construct an indicator for Vietnam veterans in the 1997-2003 data using the "honorably discharged" question, we are introducing measurement error. Results which we present later show that the relationship between "honorably discharged" and "current smoker" (in the 1997-2003 sample) is quite similar to the relationship between "Vietnam veteran" and "current smoker" (in the 1978-1980 sample). This suggests the measurement error problem is not severe.

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<sup>6</sup>Using the draft eligibility cutoff for the 1952 cohort (95) for the 1953 cohort results in only very small changes in coefficients, and a slight increase in the standard errors of coefficients.

<sup>7</sup>U.S. Army, Navy, Air Force, Marine Corp, or Coast Guard. This question is in the Person File in the 1997-2003 NHIS.

## Main outcome measures

The first outcome variable of interest is whether or not men reported being an active cigarette smoker. Data on smoking related questions is contained in the 1978-1980 Supplemental files of the NHIS. Using the information in the "Smoking Status" question, we create a variable Smoker equal to 1 if the respondent is a "current" or "occasional" smoker.

The "Smoking Status" question can also be used to construct an indicator for whether an individual has ever been a smoker, even if he is not currently an active smoker. We create a variable Smoked equal to 1 if the respondent self-reports as an "occasional smoker", "current smoker", or a "former smoker".

For the 1997-2003 NHIS data, questions on smoking are included in the Sample Adult file. To identify current smokers a variety of survey questions were asked to determine if the person is a current "every day" smoker or a current "some day" smoker. We define a respondent to be a current smoker (Smoker equal to 1) if they are either an "every day" or a "some day" smoker. This formulation matches the definition of current smoker we used for the 1978-1980 data, where we set Smoker equal to 1 if the respondent was a "current smoker" or an "occasional smoker".

The 1997-2003 data defines a "former smoker" as somebody who answered yes to the question "Have you smoked at least 100 cigarettes in your entire life?" , but is not currently an "every day" or "some day" smoker. Therefore to define our indicator for having ever been a smoker, we set the variable Smoked equal to 1 if the respondent has smoked at least 100 cigarettes or is a current smoker.<sup>8</sup> It is not clear how well this definition of Smoked matches the definition we used in the 1978-1980 sample. In the 1978-1980 data, there was no precise definition of a "former smoker" because that was left largely to the respondent.

Despite the slight differences in survey question definitions, we can construct the "Smoked" and "Smoker" variables for both the 1978-1980 and 1997-2003 NHIS data. A third outcome variable that we can construct for both survey periods is related to self reported health status.

In the 1978-1980 survey period, respondents were asked to rate their health on a scale of 1 to 4, with 1 being "Excellent" and 4 being "Poor". In the 1997-2003 NHIS, respondents were asked to rate their health on a scale of 1 to 5, where 1 is "Excellent", 2 is "Very Good", and 5 is "Poor". Once again there is a slight difference in survey questions. In spite of this, we create an outcome variable "Healthy" that is 1 if health was reported as Excellent and 0 otherwise.

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<sup>8</sup>One useful feature of the 1997-2003 Smoked variable is that it exactly matches the definition used by Bedard and Deschênes (2006).

## Other outcomes

Finally, we create 3 additional variables for 3 other outcomes using data from the 1997-2003 NHIS. These additional outcomes are either not available in the 1978-1980 data or are too rare to analyze.

"Alcohol" is 1 if the respondent reported consuming alcohol at least once per week and 0 otherwise. "High Blood Pressure" is an indicator for if the respondent was ever told by a medical professional that they had high blood pressure. "Cancer" indicates if the respondent was ever told they had cancer.

## A Simple Comparison of Draft Eligible vs. Non-Eligible Men

Table (2) demonstrates that for men surveyed from 1978-1980, draft eligibility is related to the probability of serving, the probability of being a current smoker, and the probability of having ever smoked. For the 1950-1952 birth cohorts, the proportion of draft eligible men who served in Vietnam is significantly higher than the proportion of ineligible men who served. In addition, draft eligible men from the 1950-1952 cohorts were more likely to be active smokers and to have ever smoked than ineligible men.

In the 1978-1980 NHIS data, draft eligibility is most strongly related to veterans status for the 1952 birth cohort. Only 10% of ineligible men from the 1952 cohort served in Vietnam, while 34% of eligible men served. The 1952 cohort also exhibits the strongest relationship between draft eligibility and smoking status. 52% of draft eligible men born in 1952 self-reported as active smokers in the 1978-1980 NHIS while 42% of draft ineligible men were active smokers.

Table (3) indicates that for men surveyed from 1997-2003, draft eligibility is related to our veterans status indicator. The largest difference is again seen in the 1952 cohort, where 31% of draft eligible men are veterans and only 17% of non-eligible men are veterans. However, the differences in the proportions of current smokers between draft eligible and non-eligible men have disappeared. All of the differences are small and none of them are statistically significant. While there are differences in the proportions of men who have ever been smokers, there is no clear pattern. For the 1950 cohort, draft eligible men were actually less likely (by about 3 percentage points) to report having ever smoked than draft-ineligible men.

## Wald Estimates for the effect of Military Service on Smoking

Our goal is to estimate the causal effect of military service on smoking and health outcomes by using the draft lottery to create instrumental variables for veterans status. The simplest way of doing this is with the Wald (1940) estimator. Computing the Wald estimator of the effect of military service on smoking is done to illustrate the results obtained from the simplest possible specification.

The Wald estimator for the effect of military service on smoking is the same as estimating two-stage least squares (2SLS) with only a constant and veterans status as regressors, and with only draft eligibility as the instrument. This estimation strategy is valid because draft eligibility status is randomly assigned and is therefore uncorrelated with any other factors that affect smoking behavior. The model to be estimated is the following:

$$V_{it} = \delta_0 + \delta_1 D_{it} + e_{it} \quad (1)$$

$$S_{it} = \beta_0 + \beta_1 V_{it} + u_{it} \quad (2)$$

Here  $V_{it}$  is veterans status of person  $i$  in survey year  $t$ ,  $D_{it}$  is a dummy for draft eligibility, and  $S_{it}$  is a smoking dummy variable, either Smoker or Smoked. The Wald estimate of the effect of military service is the coefficient  $\beta_1$  in the second stage.

In regression form, the naive estimate for the effect of military service is the following:

$$S_{it} = \beta_0 + \beta_1 V_{it} + \varepsilon_{ijt} \quad (3)$$

Before we compute and discuss the estimates, note that one can also compute Wald and naive estimates using only sample proportions. The formula for the Wald estimator of the effect of military service on smoking is the following:

$$\hat{\alpha} = \frac{\hat{P}_{se} - \hat{P}_{sn}}{\hat{P}_{ve} - \hat{P}_{vn}} \quad (4)$$

Here  $\hat{P}$  is always a sample proportion. The  $s$  subscript denotes smoking, the  $v$  subscript denotes veterans, the  $e$  denotes draft eligible and the  $n$  denotes draft-ineligible. For example,  $\hat{P}_{se}$  is the sample proportion of draft-eligible men who self-reported as current smokers. This formula will be useful for intuitively understanding why the estimates are different for the two different samples.

To finish this brief aside, in terms of sample proportions the naive estimate of the effect of military service is just the difference in sample proportions of veterans and non-veterans who smoke:

$$\hat{m} = \hat{P}_{sv} - \hat{P}_{sc} \quad (5)$$

Table (4) calculates the Wald estimate (using the 2SLS form) and the naive estimate of the effect of military service on being a current smoker for each birth cohort in each sample. The first thing to notice is that all 8 naive estimates are fairly similar, as they all fall somewhere in the interval [0.081, 0.156]. For the 1978-1980 sample, the Wald estimates for the 1950-1952 cohorts are systematically higher than the naive estimates. For the 1997-2003 sample, the Wald estimate is higher than the naive estimate only for the 1952 cohort, and the difference is very small (about 0.02). As was done in Tables (2) and (3), for purposes of exposition we assigned draft eligibility for the 1953 cohort based on the cutoff number for the 1952 cohort.

Table (5) calculates Wald and naive estimates of the effect of military service on having ever smoked. The pattern of Wald and naive estimates is very similar to the pattern for being a current smoker that we discussed above. The naive estimates are again all positive, with quite similar magnitudes between the 1978-1980 and 1997-2003 samples for every cohort except the 1952 cohort. The 1978-1980 Wald estimates are uniformly higher than the corresponding naive estimates for the 1950-1952 cohorts, while for the 1997-2003 sample the Wald estimates are uniformly smaller for those same cohorts.

Examining the sample proportions formula for the Wald estimator highlights the source of the different results for the two samples. The denominator is the difference in the proportion of draft eligible men who are veterans and the proportion of draft ineligible men who are veterans. Looking at Tables (4) and (5), we see that the denominator is generally smaller for the 1997-2003 sample than for the 1978-1980 sample. Therefore in order for the Wald estimates to be so much lower for the 1997-2003 sample, the numerator must be significantly smaller. The numerator is the difference in the proportions of draft eligible men who smoke and draft ineligible men who smoke (or who have ever smoked). These differences are small and even negative for the 1997-2003 sample.

Comparing Wald to naive estimates for the two NHIS samples allows us to pinpoint the source of the differences in IV results between the two samples. The IV results are different because the relationship between draft eligibility and current smoking status is quite small or even negative for the 1997-2003 sample, while the same relationship is larger and always positive for the 1978-1980 sample. Our interpretation (to be discussed in more detail later) is that the effect of military service on current smoking status has gone away 25 to 30 years after the Vietnam war.

## Estimation using Instrumental Variables

While the Wald estimates of the effect of military service discussed above are consistent estimates, none of them are statistically significant. The methods of this section use additional control variables in the estimating equations and the GMM estimation technique with instruments based on draft eligibility to reduce the standard errors of our estimates.

There are potential problems with instrumental variable techniques. Specifically, it is well known that if the instrumental variables are weak then the conventional standard error estimates, which are based on an asymptotic approximation, are incorrect. Second, if the instruments are weak even very slight endogeneity of the instruments will lead to badly biased estimates. We report standard first stage F statistics as evidence that our instruments are not weak. We report the standard Hansen J-test of the validity of over-identifying restrictions to provide evidence that our instruments are exogenous. The null hypothesis of the J-test is that the IV estimate using all available instruments differs only by sampling error from the IV estimate using a subset of instruments (one, in our case) that just identify the equation.

In addition, there are concerns about the size of the finite sample bias of IV estimators. IV estimators are never unbiased in finite samples. An alternative estimation technique, Limited Information Maximum Likelihood, has been shown to have desirable finite sample properties. For instance, Anderson et. al. (1982) find that the sampling distribution of the LIML estimator converges to the asymptotic distribution faster than that of the 2SLS estimator. It has also been shown, as in (Reference) that the finite sample bias of the LIML estimator does not depend on the number of instruments, while the finite sample bias of the 2SLS and GMM estimators increases with the number of instruments. However, as the results from estimation by LIML were consistent with the GMM results, for ease of interpretation we present the GMM specification and report results from it. For completeness we report some LIML results in additional specification checks.

Using only draft eligibility interacted with birth year as instruments, our IV estimator is two-step linear GMM. The first stage is a linear probability model for Vietnam veterans status, and the second stage is a linear probability model for the dependent variable (such as smoking status or self reported health). The standard optimal weighting matrix (equal to the inverse of the estimated covariance matrix) is used.

Because we are estimating linear probability models, 2SLS is not the efficient IV estimator when using multiple instruments for our single endogenous variable. The linear probability model is heteroskedastic by construction, and 2SLS is the efficient IV estimator with multiple instruments only when the error term is conditionally homoskedastic. When conditional homoskedasticity is violated, as it is for us, two-step GMM is the efficient

linear IV estimator.<sup>9</sup>For smoking, the model that we estimate is as follows:

$$V_{it} = \delta_0 + \delta_1(D_{it} * B_{it}) + \delta_2R_{it} + \delta_3CD + \delta_4X_{it} + e_{it} \quad (6)$$

$$S_{it} = \beta_0 + \beta_1V_{it} + \beta_2R_{it} + \beta_3CD + \beta_4X_{it} + u_{it} \quad (7)$$

Equation (6) is the first stage and equation (7) is the second stage.  $S_{it} = 1$  if individual  $i$  in survey year  $t$  is an active smoker, and  $V_{it} = 1$  if the individual is a Vietnam veteran. The variable  $R_{it}$  is a race dummy equal to 1 if the individual is black. CD is a vector of birth cohort and age dummies. X is a vector of other demographics, including college education, marital status, and dummies for residence in each of the 4 regions (North, South, Northeast, and West) of the United States. The instrumental variable is  $D_{it} * B_{it}$ , which equals 1 if the individual's lottery number made him draft eligible in the year his birth cohort was subjected to the draft. The parameter of interest is  $\beta_1$ , which measures the causal effect of serving in Vietnam on the probability of being an active smoker.

The effect of military service on all of our outcome variables is estimated according to the specification in equations (6) and (7). All of the dependent variables except Cancer are good candidates for the linear probability model because their sample proportions are not too close to 0 or 1. Cancer is a relatively rare event and therefore it is difficult to analyze with this methodology.

A concern about this two stage specification is that it greatly restricts the set of 2nd stage regressors that will be plausibly exogenous in the 1st stage. For instance, adding education dummies as regressors to the 2nd stage equation would certainly improve the model's predictive power. However, the mechanics of two step GMM dictate that all exogenous regressors must be included in the 1st stage estimation.

For consistent 1st stage estimation, we require variables that are exogenous in 2 distinct time periods. The first period is 1970-1973, when men were being inducted into service. The second time period is 1978-1980 (and 1997-2003), when men are surveyed in the NHIS. Thus we are restricted to using draft eligibility, race, birth cohort and age dummies to predict veterans status in the 1st stage.

Including education dummies would result in using educational attainment as measured in 1978, 1979, or 1980 to predict whether the individual served in Vietnam. This could make the 1st stage inconsistent, because causality might run from service in Vietnam to eventual educational attainment levels.

However, Card and Lemieux (2001) find that for men born after 1950, the Vietnam draft lottery had little or no effect on college completion rates. They do find positive effects of the Vietnam draft on educational attainment

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<sup>9</sup>See Wooldridge (p.96, p.454) and Hayashi (p.226) for details.

for men born in the mid to late 1940's. In another study, Angrist and Krueger (1995) also find that the relationship between Vietnam draft lottery number and completed education level is weak.

This evidence suggests that if we omit educational attainment, we can interpret our results as the effect of military service itself, and not an indirect effect operating through changes in educational attainment. However, to strengthen this case we do include a dummy for college completion as if it were an exogenous variable, as this does not change our results. We report specification checks where models are estimated with and without college completion to demonstrate this in Tables (10) and (11).

A drawback of using draft eligibility based instruments is that we can only use the 1950 to 1953 birth cohorts, which reduces our sample size. An alternative approach is to estimate equations (6) and (7) for both males and females for many birth cohorts, and use year of birth dummies interacted with a dummy for male as the instrumental variables. The only additional control variable to be included is a dummy for males. This type of approach is common in both health and labor economics. Intuitively, the control group in this methodology is women in the same birth cohort, while the control group in our methodology is draft ineligible men from the same cohort. We estimate models which employ the cohort-gender approach for all of our outcome variables of interest, so that we can compare the results generated by the two methodologies.

## Description of results

### Results for the 1978-1980 sample

From Table (6), we see that the OLS estimates of the effect of service in Vietnam on the probability of being a current smoker (military service increases the probability of smoking by 7.4 percentage points) and having ever smoked (8.3 percentage points) are smaller than the IV estimates of 31.4 and 25.7 percentage points. All four coefficients are significant according to the reported heteroskedastic-robust standard errors. The first stage F statistics for the GMM estimates are 22.12 and 21.98 respectively, which is above the usual rule of thumb of 10.

The IV estimate for the effect on service on reporting excellent health is -0.329, well below the OLS estimate which is not different from zero.

For comparison, the IV estimates in Table (7) which use cohort-male dummies as instruments on a sample including the 1944-1957 birth cohorts give similar results for the effect of military service on current smoking and having ever smoked. The first stage F statistics are very good, but in the Smoked and Excellent Health models the Hansen J-test rejects the overidentifying restrictions imposed by using cohort-male dummies as instruments. This is evidence that our estimates using draft eligibility estimates are more

reliable than those using cohort-male instruments.

### Results for the 1997-2003 sample

Referring to Table (8), our IV estimates do not find any significant long term effects of military service on the 6 outcome variables we examine. For Smoker, Excellent Health, Cancer, Alcohol, and High Blood Pressure the IV estimates are all well within 1 standard error of the OLS point estimates. The first stage F statistics are all above 20.

The alternative IV method reported in Table (9), which uses cohort-male instruments, generates quite different results for the effect of military service on having ever smoked. The alternative IV estimate of a 26.7 percentage point increase in the probability of having ever smoked is about 2 standard deviations higher (using the standard error from Table (8) )than the statistically insignificant point estimate obtained from using cohort-draft eligibility instruments. However, the Hansen J test rejects the overidentifying restrictions imposed by using the cohort-male dummy instruments for the Smoked specification. This indicates that our cohort-draft eligibility instruments are preferable to cohort-male instruments in the case of the "Ever Smoked" dependent variable.

## Interpretation of Results

The results thus far pose three puzzles that merit further investigation. First, why are naive OLS estimates of the effect of service on smoking as of 1978-80 biased substantially downwards? Second, why does the large effect on smoking as of this earlier period disappear in the later time periods? Third, why do the two IV approaches (based on draft lottery number and sex-cohorts respectively) yield very different results in some cases?

- Puzzle #1: Why is OLS biased downwards in the 1978-80 sample?

As noted earlier, naive OLS estimates are likely to be biased by unobservable differences between veterans and non-veterans.

Our IV results indicate that the 1978-1980 OLS estimates of the effects of military service on being a current smoker or having ever smoked are biased downwards. If OLS is biased downwards, what does this imply about the relationship between smoking, military service, and the important unobservable factor which is biasing the results?

Let  $q$  denote an unobservable variable which affects both smoking and military service. Equation (7) when including  $q$  is then:

$$S_{it} = \beta_0 + \beta_1 V_{it} + \beta_2 R_{it} + \beta_3 CD + \beta_4 X_{it} + \gamma q_{it} + u_{it} \quad (8)$$

Here  $\gamma$  represents the effect of  $q$  on smoking. For simplicity, assume that  $q$  is uncorrelated with the other explanatory variables such as race and birth cohort. The OLS inconsistency in the estimate of the effect of veterans status on smoking is then:

$$\text{Plim}(\hat{\beta}_1) = \beta_1 + \gamma \frac{\text{Cov}(V_{it}, q_{it})}{\text{Var}(V_{it})} \quad (9)$$

Equation (9) indicates that in order for the OLS estimate of  $\beta_1$  to be biased downwards,  $\gamma$  and  $\text{Cov}(V_{it}, q_{it})$  must be opposite in sign. For instance, if  $q$  has a positive effect on smoking ( $\gamma > 0$ ) and is negatively related to military service ( $\text{Cov}(V_{it}, q_{it}) < 0$ ), OLS is biased downwards. If we think  $q$  is tolerance for health risks, we would not observe a downward OLS bias because  $q$  would be positively related to both smoking and military service.

The fact that the estimates for the effect of service on smoking are much larger with IV approaches than with OLS in 1978-80 suggests that, on balance, unobservable factors made veterans less prone to smoking. This is consistent with the fact that health standards for enlistment into the military were relatively strict, with nearly half of potential inductees failing their physical examinations (need more reliable citation for this).

Another potential explanation for the apparent downward bias of OLS lies in the fact that the IV approach measures local average treatment effects. It is possible that the smoking behavior of "marginal" participants in the war, i.e. men who were induced to serve by having a low draft number, was more affected by service than people who served in general. This could happen, for example, if men with low draft numbers who served were more likely to be assigned to places where access to cigarettes was higher than elsewhere in the military. We have not uncovered any evidence, however, that, conditional on serving, men with low draft numbers were more likely to be sent to particular divisions of the military, nor any evidence that access to cigarettes varied significantly across divisions.

- Puzzle #2: Why do effects dissipate over time?

The results suggest that the effects of service on smoking and self-reported health are large as of 1978-80 but much smaller, if not zero, by 1997-2003. In interpreting this decline, it is again important to keep in mind that men induced to smoke by having low draft numbers can be thought of as marginal smokers; they would not have been smoking as of 1978-80 if not for having had a low draft number. Thus, as they became more removed from the war over time and they had less contact with military bases and military stores, it is perhaps not surprising that many of them quit smoking. Similarly, many marginal non-smokers, i.e. men with high draft numbers who would have smoked if they had had a low draft numbers, undoubtedly went on to smoke later in life.

In other words, our results suggest that for these men smoking behavior is highly malleable over time. More specifically, a randomly induced increase in the likelihood of smoking in young adulthood (these men were ages 25-30 in 1978-80) did not cause a corresponding increase in middle age (they were ages 45-53 in 1997-2003). This finding runs counter to the conventional wisdom, based on correlational evidence, that smoking early in life causes people to smoke later in life. One likely reason for this result is the shift in culture and policy related to smoking in the military during the 1980s and 1990s. Although cigarettes remained available tax-free at military bases and commissaries, the military began to actively discourage smoking and the Veterans Health Administration increased smoking cessation efforts during the 1980s.

To check further our results regarding recent time periods, we analyze data from the 2002-2003 National Epidemiologic Survey on Alcohol and Related Conditions (NESARC), conducted and sponsored by the National Institute on Alcohol Abuse and Alcoholism (NIAAA). This publicly available data set includes information on smoking, self-reported health, and exact date of birth. It does not include information on veteran status, meaning that we cannot repeat our full analysis. We can, however, simply compare means for the key outcome variables across draft-eligibility status. These results, in Table (14), are consistent with the results from the NHIS data; smoking behavior and self-reported health are not significantly different across draft eligibility status.

A potential problem with the results for the 1997-2003 sample is attrition due to mortality. Such attrition could attenuate estimated effects of service on smoking if smokers had high levels of mortality. However, while it is true that smokers have higher mortality than non-smokers, absolute rates of deaths due to cancer and heart disease remain very low until ages above 45-53 (Sonnenschein and Brody 2005). In addition, it is important to recall that with few exceptions men from the 1950-52 birth cohorts were drafted into the war after the last major combat in 1970. For the NHIS data, Table (15) shows that there is no clear pattern in the sample proportion of veterans between the 1978-1980 and 1997-2003 survey periods.

The Hearst et al. (1986) study finds a reduced form relationship between mortality and draft eligibility. Specifically, being draft eligible was found to be associated with a slightly higher risk of death in the decade or so after the Vietnam war. Table (16) shows that in our data, we find no significant differences in the proportion of draft eligible men between the 1978-1980 and 1997-2003 survey periods. This serves as evidence that the amount of sample attrition due to mortality is small.

- Puzzle #3: Why do results differ across the two IV approaches?

As shown earlier, the results in our preferred analysis (based on the draft lottery IV approach) differ in some cases from those based on the sex-cohort

IV. The main potential weakness of the sex-cohort approach is that it cannot account for unmeasured factors that differentially affect smoking across cohort-sex groups. For example, the 1970s marked the rise of antismoking campaigns in the United States, and the effects of these efforts may have had varying male-female differential effects across cohorts. This seems particularly possible given other social and economic trends, such as declining fertility and increased female labor supply.

We can illustrate the potential problems of using adjacent cohorts plus same-cohort females as controls for a male cohort in two ways. First, Table (17) shows smoking rates by sex-cohort group for the 1944-1960 cohorts. These data show that there are different fluctuations and trends for males and females across cohorts in general and even for cohorts who were not significantly exposed to military service (cohorts 1954-1957).

(analysis of placebo experiment for 1958-65 cohorts to be added here)

## Conclusion

By using the Vietnam draft lottery to construct instruments for veteran status, we obtain consistent estimates for the effect of service in Vietnam on smoking and self-reported health. For men surveyed in 1978-80, which was 3-5 years after the war ended, our IV estimates indicate that military service increased the probability of smoking by about 30 percentage points. For men surveyed between 1997 and 2003, which is up to 28 years after the end of U.S. involvement in Vietnam, our IV estimates find no significant effect of military service on the probability of being an active smoker. In addition, from a methodological perspective, our comparison of these results to a widely used alternative IV approach, which is based on differences across sex-cohorts, highlights some of the vulnerabilities of the latter approach.

We conclude that the effect of military service on smoking status was strong in the years immediately following the Vietnam War but dissipated by the 1997-2003 time period. These findings are significant not only for historical understanding of the consequences of a much debated war, but also for contemporary policy considerations. The results provide new evidence regarding the responsiveness of smoking behavior to reduced prices (through the provision of cigarettes in rations and availability of inexpensive purchases). In addition, the results suggest a high malleability of smoking behavior over time. The sharp increase in the likelihood of smoking due to service in young adulthood did not persist into middle age.

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Cohort	Draft Eligible	Freq.	mean(Veteran)	mean(Smoker)	mean(Smoked)
1950	No	229	0.18	0.46	0.61
	Yes	258	0.32	0.48	0.67
1951	No	342	0.16	0.42	0.57
	Yes	192	0.36	0.49	0.63
1952	No	396	0.10	0.42	0.60
	Yes	143	0.34	0.52	0.64
1953	No	413	0.13	0.44	0.59
	Yes	137	0.15	0.44	0.59

Table 2: **1978-1980 NHIS Smoking Supplements.** Sample proportions of Vietnam veterans, current smokers, and "ever smoked" by birth cohort and draft eligibility. For exposition the eligibility cutoff for the 1952 cohort (95) is also used for the 1953 cohort, from which nobody was actually drafted.

Cohort	Draft Eligible	Freq.	mean(Veteran)	mean(Smoker)	mean(Smoked)
1950	No	806	0.23	0.31	0.62
	Yes	912	0.32	0.29	0.58
1951	No	1108	0.18	0.30	0.58
	Yes	597	0.27	0.31	0.57
1952	No	1298	0.17	0.32	0.58
	Yes	468	0.31	0.33	0.60
1953	No	1394	0.18	0.33	0.58
	Yes	423	0.19	0.33	0.59

Table 3: **1997-2003 NHIS.** Sample proportions of Vietnam veterans, current smokers, and "ever smoked" by birth cohort and draft eligibility. For exposition the eligibility cutoff for the 1952 cohort (95) is also used for the 1953 cohort, from which nobody was actually drafted.

Simple estimates, Smoker

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Cohort	1978-1980		1997-2003	
	Wald	naive	Wald	naive
1950	0.181 (0.351)	<b>0.111</b> (0.054)	-0.248 (0.278)	<b>0.092</b> (0.025)
1951	0.315 (0.219)	<b>0.129</b> (0.052)	0.128 (0.271)	<b>0.133</b> (0.028)
1952	0.413 (0.218)	0.081 (0.060)	0.121 (0.175)	<b>0.110</b> (0.028)
1953	0.016 (2.535)	<b>0.156</b> (0.064)	-0.285 (2.520)	<b>0.122</b> (0.030)

Table 4: Wald and naive estimates for the effect of military service on the probability of being a current smoker by cohort and sample period.

Simple estimates, Ever Smoked

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Cohort	1978-1980		1997-2003	
	Wald	naive	Wald	naive
1950	0.471 (0.345)	<b>0.181</b> (0.047)	-0.480 (0.323)	<b>0.145</b> (0.025)
1951	0.308 (0.215)	<b>0.112</b> (0.049)	-0.064 (0.296)	<b>0.165</b> (0.027)
1952	0.180 (0.206)	0.074 (0.057)	0.115 (0.181)	<b>0.176</b> (0.027)
1953	0.052 (2.467)	<b>0.165</b> (0.058)	1.253 (3.407)	<b>0.207</b> (0.027)

Table 5: Wald and naive estimates for the effect of military service on the probability of having ever smoked by cohort and sample period.

Estimates of the effect of serving in Vietnam, cohort-draft IV

Dependent Variable	OLS	IV (GMM)	First Stage F	J-test
Smoked	<b>0.083</b> (0.027)	<b>0.257</b> (0.136)	21.98	0.77
Smoker	<b>0.074</b> (0.029)	<b>0.314</b> (0.142)	22.12	0.37
Excellent health	-0.012 (0.028)	<b>-0.329</b> (0.143)	22.93	0.29

Table 6: 1978-1980 NHIS. Instruments are birth cohort interacted with draft eligibility.  $n = 1965$ . The F test column reports the value of the F-statistic for the test that the instruments are jointly significant in the first stage regression. The J-test column reports the p-value for Hansen J-test of the over-identifying restrictions. The null hypothesis is that IV estimates using a subset of the instruments differ only by sampling error from IV estimates using all of the instruments.

Alternative estimates, 1978-1980 NHIS, 1944-1957 cohorts

Dependent Variable	OLS	IV (GMM)	First Stage F	J-test
Smoked	<b>0.094</b> (0.011)	<b>0.266</b> (0.043)	169.73	0.07
Smoker	<b>0.055</b> (0.012)	<b>0.152</b> (0.042)	170.22	0.13
Excellent health	<b>0.032</b> (0.011)	-0.006 (0.043)	180.54	0.06

Table 7: 1978-1980 NHIS. Instruments are birth cohort interacted with male.  $n = 23565$ . The F test column reports the value of the F-statistic for the test that the instruments are jointly significant in the first stage regression. The J-test column reports the p-value for the hypothesis test that estimates using a subset of the instruments differ only by sampling error from estimates using all of the instruments.

Estimates of the effect of serving in Vietnam, cohort-draft IV

Dependent Variable	OLS	IV (GMM)	First Stage F	J-test
Smoked	<b>0.152</b> (0.013)	-0.033 (0.134)	23.02	0.16
Smoker	<b>0.085</b> (0.013)	0.074 (0.124)	23.05	0.28
Excellent health	-0.006 (0.013)	0.086 (0.125)	22.77	0.20
Cancer	<b>0.0124</b> (0.006)	-0.068 (0.043)	22.70	0.08
Alcohol (once/week)	<b>0.034</b> (0.015)	0.026 (0.140)	21.81	0.17
High BP	<b>0.023</b> (0.013)	0.054 (0.121)	22.51	0.54

Table 8: 1997-2003 NHIS. Instruments are birth cohort interacted with draft eligibility. The F test column reports the value of the F-statistic for the test that the instruments are jointly significant in the first stage regression. n= 6939

Alternative estimates, 1997-2003 NHIS, 1944-1957 cohorts

Dependent Variable	OLS	IV (GMM)	First Stage F	J-test
Smoked	<b>0.124</b> (0.007)	<b>0.267</b> (0.037)	119.91	0.00
Smoker	<b>0.054</b> (0.007)	0.037 (0.033)	120.17	0.28
Excellent health	-0.000 (0.006)	-0.047 (0.032)	121.56	0.53
Cancer	<b>0.011</b> (0.003)	-0.014 (0.017)	121.28	0.06
Alcohol (once/week)	<b>0.045</b> (0.007)	-0.014 (0.035)	120.65	0.84
High BP	<b>0.018</b> (0.007)	0.020 (0.033)	120.84	0.17

Table 9: 1997-2003 NHIS. Instruments are birth cohort interacted with male. n= 52972

Specification checks for college, cohort-draft IV			
Dependent Variable	College	IV (LIML)	
		1978-1980	1997-2003
Smoked	Yes	<b>0.256</b> (0.138)	0.011 (0.114)
	No	<b>0.282</b> (0.137)	-0.072 (0.145)
Smoker	Yes	<b>0.316</b> (0.143)	0.075 (0.128)
	No	<b>0.338</b> (0.141)	0.041 (0.131)

Table 10: 1978-1980 and 1997-2003 NHIS. Instruments are birth cohort interacted with draft eligibility. LIML estimates are reported here for comparison with the GMM estimates.

Specification checks for college, cohort-male IV			
Dependent Variable	College	IV (LIML)	
		1978-1980	1997-2003
Smoked	Yes	<b>0.269</b> (0.044)	<b>0.270</b> (0.037)
	No	<b>0.212</b> (0.045)	<b>0.244</b> (0.038)
Smoker	Yes	<b>0.153</b> (0.043)	0.038 (0.033)
	No	<b>0.080</b> (0.044)	0.008 (0.034)

Table 11: 1978-1980 and 1997-2003 NHIS. Instruments are birth cohort interacted with male. LIML estimates are reported here for comparison with the GMM estimates.

Cohort	Veteran	1978-1980		1997-2003	
		mean(Smoker)	obs.	mean(Smoker)	obs.
1950	No	0.441	331	0.274	1,239
	Yes	0.552	116	0.366	475
1951	No	0.412	383	0.275	1,333
	Yes	0.542	120	0.401	360
1952	No	0.431	415	0.297	1,382
	Yes	0.512	82	0.407	366
1953	No	0.418	450	0.307	1,480
	Yes	0.573	68	0.429	322

Table 12: Sample proportions of current smokers by birth cohort, veteran status, and survey period. (Males only). NHIS data.

Cohort	Veteran	1978-1980		1997-2003	
		mean(College)	obs.	mean(College)	obs.
1950	No	0.301	363	0.360	1,242
	Yes	0.105	124	0.229	476
1951	No	0.326	411	0.330	1,345
	Yes	0.106	124	0.214	360
1952	No	0.228	449	0.308	1,397
	Yes	0.067	90	0.211	369
1953	No	0.255	478	0.277	1,493
	Yes	0.042	72	0.222	324

Table 13: Sample proportions of college graduates by birth cohort, veteran status, and survey period. (Males only). NHIS data.

2002-2003 NESARC Data			
Draft Eligible	Freq	mean(Smoked)	mean(Smoker)
No	973	0.56	0.37
Yes	391	0.52	0.39

Table 14: Sample proportions of current smokers and ever smoked by draft eligibility. (Males only) Data source: 2002-2003 NESARC.

Cohort	1978-1980		1997-2003	
	$\hat{P}_v$	obs.	$\hat{P}_v$	obs.
1950	0.255	487	0.277	1,718
1951	0.230	534	0.211	1,705
1952	0.167	539	0.209	1,766
1953	0.131	550	0.178	1,817

Table 15: Sample proportions of male veterans, 1978-1980 and 1997-2003 NHIS.

Cohort	1978-1980		1997-2003	
	$\hat{P}_d$	obs.	$\hat{P}_d$	obs.
1950	0.530	487	0.531	1,719
1951	0.360	534	0.351	1,709
1952	0.265	539	0.265	1,770

Table 16: Sample proportions of draft-eligible men, 1978-1980 and 1997-2003 NHIS.

Cohort	Gender	1978-1980 mean(Smoked)	1997-2003 mean(Smoked)
1944	Female	0.55	0.50
	Male	0.72	0.65
1945	Female	0.55	0.49
	Male	0.71	0.66
1946	Female	0.52	0.48
	Male	0.67	0.65
1947	Female	0.46	0.49
	Male	0.68	0.64
1948	Female	0.48	0.46
	Male	0.66	0.62
1949	Female	0.50	0.44
	Male	0.65	0.60
1950	Female	0.46	0.43
	Male	0.64	0.60
1951	Female	0.51	0.41
	Male	0.59	0.57
1952	Female	0.49	0.44
	Male	0.61	0.59
1953	Female	0.45	0.44
	Male	0.59	0.58
1954	Female	0.43	0.45
	Male	0.58	0.56
1955	Female	0.45	0.45
	Male	0.54	0.57
1956	Female	0.46	0.44
	Male	0.55	0.53
1957	Female	0.44	0.45
	Male	0.50	0.52

Table 17: Sample proportions of ever smoked by birth cohort, gender, and survey period