

The Effect of Smoking in Young Adulthood on Smoking Later  
in Life: Evidence based on the Vietnam Era Draft Lottery  
(Forthcoming in *Forum for Health Economics and Policy*)\*

Daniel Eisenberg<sup>†</sup>      Brian Rowe<sup>‡</sup>

February 2009

## Abstract

An important, unresolved question for health policymakers and consumers is whether cigarette smoking in young adulthood has lasting effects into later adulthood. The Vietnam era draft lottery offers an opportunity to address this question, because it randomly assigned young men to be more likely to experience conditions favoring cigarette consumption, including highly subsidized prices. Using this natural experiment, we find that military service increased the probability of smoking by 35 percentage points as of 1978-80, when men in the relevant cohorts were aged 25-30, but later in adulthood this effect was substantially attenuated and did not lead to large negative health effects.

---

\*We thank Joshua Angrist, Martha Bailey, Charlie Brown, Tom Buchmueller, Christopher Carpenter, Lucas Davis, John Dinardo, Richard Frank, Jim Hines, Haiden Huskamp, Edward Norton, Dan Silverman, Jeff Smith, and Gary Solon, and anonymous reviewers for helpful comments. We also appreciate the many helpful suggestions from seminar participants at the University of Michigan, the Ann Arbor VA, the American Society of Health Economists 2006 meeting, and the Triangle Health Economics Seminar. We thank Joshua Angrist for sharing data on the probability of service by draft number interval. We thank Clint Carter, Bob Krasowski, Maggie Levenstein, Peter Meyer, and Chris Rogers for assistance with accessing restricted data from the National Health Interview Survey. Funding is gratefully acknowledged from the University of Michigan Tobacco Research Network (UMTRN), which is supported by the American Legacy Foundation. The views expressed in this paper do not reflect the views of the Foundation, Foundation staff, or its Board of Directors. The first version of this paper was circulated in May 2006.

<sup>†</sup>University of Michigan, Department of Health Management and Policy, M3517 SPH II  
Ann Arbor MI 48109-2029. 734-615-7764. daneis@umich.edu

<sup>‡</sup>University of Michigan, Department of Economics, rowebr@umich.edu

# 1 Introduction

One of the most well-established relationships in the smoking literature is that smoking during youth and young adulthood is a strong predictor of smoking later in life (e.g., Merline et al. 2004). A related well-known finding is that youth and young adults who manage to quit smoking are able to avoid or substantially mitigate lifetime health consequences (U.S. Department of Health and Human Services 1990). Together, these facts have motivated health policymakers to focus much of their prevention and cessation efforts on young populations (Glied 2003).

It is unclear, however, to what extent these well-known findings represent causal relationships, due to potentially omitted variables. Smoking during youth and young adulthood may be highly correlated with smoking later in life not only because of the addictive power of cigarettes but also because of imperfectly measured contextual factors (e.g., peers and family members, occupational and personal stresses, etc.) that influence smoking and are correlated over time. Analogously, young people who quit smoking may avoid long-term health consequences not only because of quitting but also because of imperfectly measured factors (e.g., motivation to become healthier) that influence both propensity to quit and lifetime health. In other words, smoking behavior during youth and young adulthood is far from exogenously determined, so outcomes in later adulthood should not necessarily be attributed to that earlier behavior.

In this context, a well-identified causal estimate requires exogenous variation in smoking during young adulthood. Randomized controlled studies of smoking cessation interventions offer one possible approach, but they generally do not follow subjects beyond a year or two. On the other hand, observational studies generally do not have truly exogenous sources of variation by which to estimate definitive causal relationships. Furthermore, observational studies that do have arguably exogenous variation still may not be able to separate short-term effects from long-term effects, if the source of variation remains present, or highly correlated, over time (e.g. a cigarette tax increase that remains in effect, as such increases typically do).

The Vietnam draft lottery, which randomly assigned eligibility for the draft based on birth dates, offers a unique opportunity to investigate this issue. In essence, it provides a population-level randomized experiment for which both a short-term and long-term follow-up are possible. By receiving a low draft number and thus becoming more likely to serve in the military, certain men became more likely to be exposed to a potentially large, positive shock to smoking behavior while they were 19-22 years of age. All men who served

in the military during the Vietnam era had access to tax-free cigarettes at military bases and commissaries, and men in combat received free cigarettes in their rations (U.S. Public Health Services, 1989, p. 425). In addition, the effect of these reduced prices may have been enhanced by the close proximity of peers who smoked and the stress of potential combat.

Using the draft lottery and pooled cross-sectional data from the National Health Interview Survey (NHIS), we construct instrumental variable estimates of the effects of military service on smoking behavior at different time points later in life. We estimate two-stage least squares models in which the first stage predicts the probability of military service and the second stage predicts the probability of smoking. 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 that determined draft eligibility in that year. Using this analysis, we address two main questions. First, what was the effect of military service during the Vietnam era on smoking behavior as of several years after the war (1978-80), when the men in the relevant cohorts were 25-30 years old? Second, what was this effect approximately 20-25 years later (as of 1997-2005), when these men were 45-55 years old?

As of 1978-1980 we find that military service increased the probability of current smoking by 35 percentage points. This estimate is substantially larger than the corresponding ordinary least squares (OLS) estimate of 11 percentage points. The apparent downward bias in OLS suggests that men who served in the military had unobservable characteristics making them less likely to smoke than the general population, which may be related to the physical and mental standards for induction into the military.

By contrast, we find that this large short-term effect dissipated to being small and statistically insignificant as of 1997-2005, when the men were aged 45-55. This result is due to the simple fact that by this period, draft eligible men were no longer significantly more likely to smoke than non-eligible men. The IV estimate of the military service effect as of 1997-2005 is a relatively imprecise “zero”, but point estimates of the effect of draft eligibility on smoking from two other later-period data sources (1987-1988 NHIS supplements and the 2002-2003 National Epidemiologic Survey on Alcohol and Related Conditions) are also close to zero and statistically insignificant.

Our findings suggest that the large exogenous increase in smoking early in adulthood was substantially attenuated by later adulthood. Before accepting this conclusion, however, we consider two alternative explanations for the disappearance of the smoking differential. First, there may have been differential attrition from the survey samples, due to mortality or other factors, between the two time periods. We find, however, that the proportion of men with draft

eligible lottery numbers remained identical between the two samples, which is consistent with Angrist and Chen’s (2007) findings with a much larger dataset. In addition, we show that even under extreme assumptions, differential mortality by draft-eligible smokers can account for at most a very small portion of the diminished smoking differential. Second, we consider the potential role of increased educational attainment among veterans. Based on the results of recent studies<sup>1</sup> we find that this factor can explain at most a small portion of the decline in the smoking differential.

The estimates produced by our empirical strategy represent the local average treatment effect (Angrist et al. 1996) of military service on smoking: the effect on smoking for those men induced to serve in the military as a result of being assigned to the draft-eligible group. Our results imply that for these “marginal smokers”, cigarettes were not so addictive that at least 5 to 7 years of smoking during young adulthood was sufficient to induce smoking into middle and later adulthood. Furthermore, we find that as of 1997-2005 men with low draft numbers were not significantly worse on health measures, including self-reported health and lifetime prevalence of cancer.

In effect, this analysis represents a natural experiment in which certain men were randomized to be exposed to factors that made them much more likely to smoke for a number of years. The longer-term outcomes from this natural experiment are inconsistent with the widespread presumption that smoking in young adulthood strongly increases the likelihood of smoking later in adulthood, but consistent with the idea that young people who quit smoking can avoid serious long-term health consequences. As we discuss in Section 5, the extent to which these findings would generalize to other contexts (e.g., nonveterans, females, other cohorts, other countries) is unclear, but, as the first evidence based on effectively random assignment, the findings offer a new point of reference.

## 2 Background and related literature

In this section we review how the Vietnam draft lottery operated as a natural experiment, and discuss historical information related to cigarettes in the military. We then briefly review recent theory and evidence related to cigarette addiction, focusing on predictions about the links between current and future cigarette consumption.

---

<sup>1</sup>Angrist and Chen (2007), De Walque (2007), and Grimard and Parent (2007).

## 2.1 The draft lottery, the war, and cigarettes

The Vietnam draft lotteries were held in each year from 1969 to 1972 to make induction into military service a fairer process. The lotteries randomly assigned a priority for induction based on date of birth by matching each birth date to a number from 1 to 365 (or 366 for leap years). Later in the year, after assessing its needs for manpower, the U.S. Department of Defense would choose a cut-off number for draft eligibility. Men with numbers below the cut-off were considered draft-eligible and were thereby more likely to end up serving, while men with numbers above the cut-off were not at risk for being drafted. Men from the cohorts we analyze were subjected to the draft lottery at age 19 or 20, and the typical term of service for these men was 2 years (Angrist 1990, Angrist and Chen 2007).

Previous studies have employed this natural experiment to look at the effects of military service on mortality, earnings, and other outcomes. In the first such study, Hearst, Newman, and Hulley (1986) used the draft lottery to estimate the effect of military service on mortality later in life. They found a small but statistically significant effect of service on total mortality, which was driven mainly by suicide and motor vehicle related mortality. As these results have bearing on potential attrition bias, we discuss them further when interpreting our results (Section 5). In another study using this natural experiment, Angrist (1990) estimated the effect of military service on lifetime earnings. He found a significant earnings penalty from military service for white veterans relative to similar white non-veterans.

Cigarette smoking may have been affected by military service during the Vietnam era for several reasons. First, men who served faced significantly reduced prices for cigarettes. Men in combat, or other places where cooking was not feasible, received C or K rations with free cigarettes. All servicemen could buy cigarettes at wholesale, tax-free prices at military bases and commissaries. We cannot determine the exact prices at these facilities, but we can infer that they would have been less than half of retail prices. In the U.S. in 1972, the average retail price of a pack of cigarettes was 40 cents and the average pre-tax wholesale price was 13 cents (USDA, 2007). Much of this difference is attributable to taxes: 8 cents per pack in federal taxes, and an average of 10 cents per pack in state taxes (Orzechowski and Walker 2006). Moreover, the price of cigarettes *relative to other goods* would have been especially low for servicemen who did not have access to many of the goods they would normally purchase as a civilian in the United States.

Second, peer effects may have augmented the effect of reduced prices, given the concentration of men at bases and other military facilities. Third, many

servicemen in the Vietnam era were subjected to stressful, if not traumatic, experiences. For some men these experiences could have caused depression or anxiety, both of which are associated with an increased risk of smoking (Glassman et al. 1990, Winefield et al. 1989).

It is unclear how important this third factor would have been for the cohorts in our analysis. Major combat in Vietnam ended shortly after the first draft lottery was held in December 1969. Available evidence indicates that the proportion of men who faced combat, among those drafted in the 1969 to 1971 lotteries, was very small compared to earlier cohorts who participated in the war.<sup>2</sup> Nevertheless, the anxiety associated with the possibility of engaging in combat may have still played a role in these men's smoking behavior.

Multiple correlational studies have shown that veterans of the Vietnam era 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 Veterans Administration population as among the general male population of the same age (Harris et al. 1989). Our use of the draft lottery overcomes the problem of systematic selection into the military, which could in principle result in a correlation between military service and smoking even in the absence of any causal effect.

## 2.2 Theory and evidence related to cigarette addiction

The natural experiment of the Vietnam draft lottery offers an opportunity to evaluate how an exogenous change to smoking behavior in one time period persists over a long time span. Here we review what theoretical models of addiction and empirical studies have to say about this issue. While the natural experiment in our analysis does not enable us to distinguish between different economic theories of addiction, it provides new evidence related to the strength and persistence of addiction, whatever the underlying mechanism may be.

---

<sup>2</sup>The last major incursion by U.S. troops occurred in May-June 1970, and the last major confrontation for U.S. ground troops was the Battle of Fire Support Base Ripcord in July 1970. As draftees had a training period, these events would have occurred before men drafted in the lottery were sent to war. Starting in 1969, the U.S. steadily reduced the number of troops in Southeast Asia, and the Vietnam War officially ended in January 1973. Casualties similarly declined in this period, from 16,592 in 1968, to 11,616 in 1969, to 6,081 in 1970, to 2,357 in 1971, to 641 in 1972. (National Archives, <http://www.archives.gov/research/vietnam-war/>). Furthermore, of the 7,575,000 active duty Vietnam era servicemen, only 2,850,000 (38%) were sent to Southeast Asia (Baskir and Strauss 1978). Servicemen based outside of Southeast Asia would not have faced combat, but had the same opportunity to purchase tax-free cigarettes at military bases.

Under most economic models of addiction, a temporary price decrease would lead to at least some increase in contemporaneous smoking, simply because consumers respond to prices.<sup>3</sup> Also, in most economic models of addiction, the degree to which a short-term price decrease produces a long-term increase in smoking depends on the strength of the addiction, i.e., the degree to which present consumption increases the marginal utility of cigarettes consumed in the future.

Under a myopic model of addiction, a temporary fall in cigarette prices would result in both a short-term and a long-term increase in cigarette consumption. People simply increase their consumption of cigarettes in response to lower prices today, failing to anticipate that they will consume more cigarettes in the future as a result of their increased consumption today. However, there is some evidence against strictly myopic cigarette addiction, as several studies have found that consumption of cigarettes responds to expected future prices of cigarettes (Becker et al. 1994, Chaloupka 1991, Gruber and Koszegi 2001). Even if smokers do account for the future consequences of their present smoking decisions, it remains theoretically ambiguous whether a temporary price decrease would result in a long-term increase in cigarette consumption. In the rational addiction model (Becker and Murphy 1988), smokers take into account all the future consequences of their actions, and they discount the future in a time-consistent fashion. A key feature of the rational addiction model is the existence of unstable equilibrium levels of cigarette consumption. Thus under certain circumstances, a temporary price decrease could induce individuals to move from an unstable equilibrium where cigarette consumption is low to a new equilibrium where consumption is permanently higher (see Becker and Murphy 1988, p.692-693). However, in this model it is also possible that a temporary price decrease will leave long term consumption relatively unchanged.

A prominent competing theory is that individuals may be forward looking in their smoking behavior, yet discount the future in a time-inconsistent fashion (Gruber and Koszegi 2001). With time-inconsistent preferences, individuals may decide to smoke more today in response to a temporary price decrease, while they plan to quit tomorrow when prices go back up. Higher prices tomorrow may serve as a commitment device in that they will help smokers to bring their consumption levels back down to what they were before the price cut. In fact, if higher future prices are a strong enough commitment device,

---

<sup>3</sup>In this discussion “price” can be thought of as encompassing monetary price as well as military service related factors which may affect the net benefit of current consumption (peer effects and stress).

a temporary price cut may lead to an especially large short term increase in smoking, as forward looking individuals anticipate being able to quit when prices rise (see Gruber and Koszegi 2001, p.1284). Of course, a long term increase in cigarette consumption may still result if individuals are unable to follow through on their plans to reduce smoking in the future, as is typical in models where preferences are time-inconsistent.

Two recent empirical studies pertain to the issue of how changes to smoking during one time period persist over the long-term. Gruber and Zinman (2001) and Glied (2002) estimate how cigarette taxes during one's youth are associated with smoking later in life, controlling for current taxes in adulthood. Gruber and Zinman (2001) find that, for pregnant women older than 24, the overall elasticity of smoking is -0.22 and the participation elasticity is -0.08, with respect to cigarette taxes in the women's state of residence when they were 14-17 years old (which is proxied by state of birth). They note that these elasticities are about 25-50% of same-period tax elasticities typically estimated for youths. Glied (2002) finds, using a general sample of adults (in the National Longitudinal Survey of Youth), that as of age 44, the participation elasticity with respect to taxes at age 14-17 is not statistically different from zero for women and -0.2 for men. These elasticities are significantly smaller than Glied's estimates of contemporaneous tax elasticities. Thus, both studies suggest that the effects of cigarette taxes during youth dissipate substantially over the course of adulthood. A limitation of this approach is that state taxes may be correlated with unmeasured factors which affect smoking, such as anti-smoking sentiment. Also, it is difficult to determine which empirical specification properly captures the timing of the effects of taxes, given that typically cigarette tax increases are not one-time exposures but rather remain in place until the next increase. Our approach is different in that we trace out the impact of a single randomly induced exposure over time.

Continuing to improve understanding of how smoking behavior during youth and young adulthood affects longer-run smoking behavior and health is essential for informed policymaking. As Glied (2003) points out, the main justification for focusing on youths with tobacco control policy is not to reduce their current smoking so much as it is to reduce their long-term through adulthood, for which the health consequences are clearer. To project the long-term, population level effects of youth tobacco control policies, simulation models are typically used to calculate lifetime effects on smoking and health (Levy et al. 2000, Ahmad and Billimek 2007), but these models can only be as accurate as the empirical evidence on which their assumptions are based.

## 2.3 Smoking and military service

Two recent studies estimate the effects of military service on smoking and measures of health. First, Bedard and Deschênes (2006) estimate the long-run effects of military service during World War II or the Korean War on smoking, mortality and other measures of health, using instrumental variables approaches that exploit variation in the proportion of men who served across birth cohorts. They find that military service is associated with approximately a 30 percentage point increase in the probability of lifetime smoking, and this appears to account for significantly increased post-service mortality. Their results on smoking are only for the probability of having ever smoked, not current smoking, and thus do not address the evolution of smoking over time that is the focus of our paper.<sup>4</sup>

Second, Dobkin and Shabani (2007) use the draft lottery to instrument for Vietnam era military service in order to estimate effects of service on a large number of health-related measures in the NHIS. Their outcomes include smoking as of 1997-2004, and they do not find a significant effect of draft eligibility, consistent with our finding for 1997-2005. They do not examine smoking in earlier years, however. Also, they find no significant effects of military service on a large number of health measures during various periods (1974-1981, 1982-1996, and 1997-2004). As we discuss in Section 4.6, although Dobkin and Shabani caution that their estimates are too imprecise to rule out small but meaningful health effects, at the least their null findings bolster our conclusion that the large increase in smoking during young adulthood due to military service did not seriously affect health later in life.

The novel aspects of our study are to: a) show that the effect of military service on smoking in early adulthood was large; b) show that the dissipation of this effect over time cannot be explained by factors such as mortality, education, or immigration. Therefore our use of the draft lottery natural experiment, in contrast to Dobkin and Shabani's, directly speaks to the persistence of cigarette smoking over time.

---

<sup>4</sup>Two other recent papers exploit variation across adjacent cohorts and male-female differentials in military service: de Walque (2007) and Grimard and Parent (2007). These papers estimate the effect of education on smoking by focusing on birth cohorts prior to 1950-52, who largely entered service prior to the draft lottery and its associated restrictions on educational deferments. As Card and Lemieux (2001) show, many men in these earlier cohorts were able to avoid the draft by attending or remaining in school.

### 3 Data

Our primary data source is the National Health Interview Survey (NHIS), conducted by the Centers for Disease Control (CDC). The NHIS is an annual survey that asks various health-related questions to a nationally representative random sample. Although the NHIS 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 2005.<sup>5</sup> By combining data from different NHIS survey years, we create two pooled cross-sectional data sets, one for 1978-1980 and one for 1997-2005.

Each year it was held, the Vietnam draft lottery applied to men from specific birth cohorts. In particular, the 1969, 1970, and 1971 lotteries applied in turn to the 1950, 1951, and 1952 birth cohorts, who were aged 19 to 20 when their respective lottery was conducted. We assigned respondents in the NHIS their lottery number, based on their year and day of birth, using the mapping published by the U.S. Selective Service (see <http://www.ssa.gov>). We obtained access to date of birth information via the CDC's restricted, in-house NHIS data files at a secure facility.

Table (1) shows the draft eligibility cutoff numbers and which cohorts they applied to. We created our instrument for military service 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. We

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

Lottery Year	Cohort(s) Affected	Eligibility Cutoff
1969	1944-1950	195
1970	1951	125
1971	1952	95
1972	1953	N.A.

Data Source: U.S. Selective Service.

leave out the 1944-1949 birth cohorts, who were subject to the first lottery in 1969, because most servicemen from these cohorts entered the military before the lotteries started (Angrist 1990). Therefore men from these cohorts who avoided service until 1970 are far from a representative sample. In addition,

---

<sup>5</sup>The NHIS also has supplements in 1987 (Cancer Epidemiology) and 1988 (Occupational Health) with smoking questions. We discuss results from these supplements in Section 4.5.

for these cohorts the draft lottery is a much weaker instrument than it is for the 1950-1952 cohorts.

There was also a final lottery in 1972, applying to men from the 1953 birth cohort, but no men from this cohort ended up being drafted. Empirically, men with low lottery numbers in this cohort were slightly more likely to become veterans (1.5 percentage points in our sample), as some of them presumably anticipated being drafted and pre-emptively enlisted to receive more favorable terms of service. Due to the weakness of the lottery as an instrument for these men, we exclude the 1953 cohort.

Our sample is therefore men from the 1950 to 1952 birth cohorts. In our main analyses, we use 3 instruments: The Draft variable interacted with a dummy for each of the 1950, 1951, and 1952 birth cohorts.<sup>6</sup> This improves the predictive power of draft eligibility on veteran status, because the proportions of draft eligible and ineligible men who ended up serving in the military varies across the 1969-1971 lotteries.

In the 1978-1980 NHIS, respondents were asked whether they were veterans, and if so, in which conflict they served. This allows us to identify veterans of the Vietnam era military. The question related to veteran status is somewhat different in the 1997-2005 NHIS: “Have you ever been honorably discharged from active duty in the U.S. Army, Navy, Air Force, Marine Corps, or Coast Guard?” This formulation understates the true number of veterans by omitting those who were not honorably discharged. Information about this proportion does not appear to be available by year or cohort, but we do know that approximately 7% of all Vietnam era servicemen were given discharges in categories other than honorable (Baskir and Strauss 1978).<sup>7</sup> Drawing from multiple Department of Defense documents and reports, Baskir and Strauss find that instances of violations of military rules such as Absent Without Leave and prolonged absence (“administrative desertion”) peaked during the last few years of the war. In addition, the military issued a peak number of “Chapter 10” discharges in 1972, 90% of which were classified as “Undesirable” discharges after administrative procedures. These facts suggest that the proportion of veterans from the 1950 to 1952 cohorts who were not honorably discharged is higher than 7%.

The measurement error in veteran status could bias our IV estimates for

---

<sup>6</sup>The simple estimates discussed in Section 4.1 only use draft eligibility, and are quite similar to our main results using eligibility interacted with year of birth.

<sup>7</sup>Out of 7,575,000 active duty Vietnam era servicemen, 563,000 were given less-than-honorable discharges of various categories, including General, Undesirable, Bad Conduct, and Dishonorable. Approximately 34,000 of the less-than-honorable discharges were in the most serious categories of Bad Conduct or Dishonorable.

1997-2005 in either direction, depending on the relationships between the likelihood of being other-than-honorably discharged, draft eligibility, and smoking propensity. Importantly, however, the measurement error in veteran status is not relevant to the reduced form relationship between draft eligibility (based on date of birth) and smoking. As we show later, this reduced form relationship is very small and not statistically different from zero in 1997-2005, and therefore our IV estimates would not be significant even if veteran status was measured perfectly.

In all years in which the NHIS asks about smoking, the questions about smoking begin with, “Have you smoked at least 100 cigarettes in your entire life?” The questions following this stem question differ slightly between the 1978-1980 and 1997-2005 survey periods. In 1978-1980, those who answer yes are then asked, “Do you smoke cigarettes now?” We classify those who answer yes as current smokers. In the more recent survey period, those who answer yes to the stem question are asked “Do you now smoke cigarettes every day, some days, or not at all?” We classify those who answer “every day” or “some days” as current smokers. We classify people as non-smokers if they answer no to either the stem questions about 100 lifetime cigarettes or the follow-up questions about current smoking.

## 4 Empirical methods and results

### 4.1 Reduced-form comparison of draft eligible to non-eligible men

The estimated effect of military service on smoking depends upon the reduced form relationship between draft eligibility and smoking behavior, and the first stage relationship between eligibility and veteran status. Table (2) shows that for men surveyed in 1978-1980, draft eligibility is positively related to both the probability of serving in the military and the probability of being a current smoker. Also, the differential diminishes by cohort, from 1950, to 1951, to 1952, suggesting that the differential might diminish as a function of time since service. Indeed, as of 1997-2005, draft eligibility is significantly related to veteran status but not to smoking. The difference in the proportion of current smokers between draft eligible and non-eligible men disappears completely for the 1950 and 1951 birth cohorts, and only a slight difference remains for the 1952 cohort.

The reduced-form patterns by draft eligibility for smoking in Table (2) drive our main results. The positive relationship between eligibility and smok-

ing in 1978-1980 produces positive and significant estimates for the effect of Vietnam-era military service. For 1997-2005, the lack of a significant relationship between eligibility and smoking implies that the effect of Vietnam-era service is no longer significant in that time period.<sup>8</sup> The columns labeled RF

Table 2: Sample proportions of Vietnam era veterans and smokers.

Cohort	Surveys	Draft Eligible	Veteran	Smoker	obs.
1950	78-80	No	0.18	0.46	229
		Yes	0.32	0.48	258
	97-05	No	0.23	0.31	1,042
		Yes	0.32	0.30	1,163
1951	78-80	No	0.16	0.42	342
		Yes	0.36	0.49	192
	97-05	No	0.18	0.30	1,432
		Yes	0.28	0.30	752
1952	78-80	No	0.10	0.42	396
		Yes	0.34	0.52	143
	97-05	No	0.16	0.30	1,617
		Yes	0.31	0.33	590
All Cohorts	78-80	No	0.14	0.43	967
		Yes	0.34	0.49	593
	97-05	No	0.18	0.31	4,091
		Yes	0.31	0.31	2,505

Data Source: 1978-1980 NHIS Smoking Supplements, 1997-2005 NHIS. Males only.

78-80 Veteran: Yes to “Veteran of U.S. Armed Forces?”

97-05 Veteran: Yes to “Ever honorably discharged from U.S. Armed Forces?”

in Table (3) highlight this difference in the reduced form results between the two time periods. In 1978-1980, draft-eligible men have a 0.064 higher probability of smoking, whereas in 1997-2005 this differential is almost exactly zero (0.001). The relationship between eligibility and veteran status also changes somewhat, from 0.20 to 0.12. This is probably related to the difference in survey questions discussed earlier, as well as the fact that the 1997-2005 sample includes a small proportion of immigrants who were not subject to the draft

<sup>8</sup>These results and all other results reported in tables and figures are based on analyses without sample weights. All results remain nearly identical when re-estimated using the NHIS sample weights.

during the 1970's.<sup>9</sup> As noted earlier, however, because the reduced form relationship between eligibility and smoking is close to zero and insignificant in 1997-2005, our estimates of the effect of military service would not be statistically significant regardless of the veteran status variable. To see this, consider

Table 3: Reduced form estimates of the effect of draft eligibility, and Wald estimates of the effect of military service.

Dependent Variable	1978-1980			1997-2005		
	RF	OLS	2SLS	RF	OLS	2SLS
Smoker	<b>0.064</b> (0.027)	<b>0.11</b> (0.03)	<b>0.32</b> (0.14)	0.001 (0.012)	<b>0.11</b> (0.01)	0.01 (0.09)
Veteran	<b>0.20</b> (0.023)			<b>0.12</b> (0.011)		
Observations	1,452			6,553		

RF: Regression of dependent variable on a constant and draft eligibility.

2SLS uses draft eligibility as the instrument for military service.

1950-1952 birth cohorts, males only, no control variables.

Heteroskedastic-robust standard errors.

an extreme case using the “true” difference in the probability of service by draft eligibility according to the Defense Manpower Data Center (DMDC, see Figure 2). According to the DMDC, the true effect of eligibility on the probability of service is about 0.123 (for the 1951 and 1952 cohorts). Our 1997-2005 reduced-form effect of eligibility on current smoking is 0.001 with a standard error of 0.012. Treating the DMDC effect as a known constant, a simple two-sample IV estimate of the effect of military service on smoking is  $\frac{0.001}{0.123} = 0.0081$ , with a standard error of  $\sqrt{\frac{0.012^2}{0.123^2}} = 0.0976$ .

<sup>9</sup>The NHIS does not ask about country of origin, but we found in a different nationally representative data set from 2002-2003 - the National Epidemiological Survey of Alcohol and Related Conditions - that 13% of men born in 1950-1952 were foreign born. If we make the conservative assumption that none of these men were veterans, then the inclusion of these men in the NHIS estimates lowers our estimate of p(veteran) by about 3 percentage points. Also, the inclusion of these men in the NHIS estimates lowers our estimated differential in p(veteran) by draft eligibility by about 1 percentage point.

## 4.2 Wald estimates

The simplest IV estimate of the causal effect of military service on smoking is the Wald (1940) estimator. The Wald estimator may be computed as:

$$\hat{\beta}_W = \frac{\hat{S}_e - \hat{S}_{ne}}{\hat{V}_e - \hat{V}_{ne}} \quad (1)$$

Here  $\hat{S}$  denotes the proportion of smokers,  $\hat{V}$  the proportion of veterans, the  $e$  subscript denotes draft eligible, and  $ne$  not draft-eligible. The Wald estimator can be calculated using the information given in Table (2). A corresponding naive estimate for the effect of military service on smoking is simply to compare the sample proportion of veterans who smoke with the proportion of non-veterans who smoke. Letting  $v$  denote veterans and  $nv$  non veterans, the naive estimator is:

$$\hat{\alpha} = \hat{S}_v - \hat{S}_{nv} \quad (2)$$

The Wald estimator given in (1) is equivalent to estimating two-stage least squares (2SLS) with only a constant and veteran status as regressors, and with only draft eligibility as the instrument. We calculate the Wald estimator in this way in order to generate the usual 2SLS standard errors. The model to be estimated is the following:

$$V_{it} = \delta_0 + \delta_1 D_{it} + e_{it} \quad (3)$$

$$S_{it} = \beta_0 + \beta \hat{V}_{it} + u_{it} \quad (4)$$

Here  $V_{it}$  is the veteran status of person  $i$  in survey year  $t$ ,  $D_{it}$  is a dummy for draft eligibility, and  $S_{it}$  is a smoking dummy variable, and  $\hat{V}_{it}$  is predicted veterans status from equation (3). The IV estimate of the effect of military service is the coefficient  $\beta$  in equation (4). This IV estimate is a consistent estimator for the causal effect of military service on smoking if draft eligibility is related to military service and is uncorrelated with any other factors in the error term  $u_{it}$  that affect smoking behavior. To use draft eligibility as an instrument for military service, we must assume that the reduced form relationship between eligibility and smoking is entirely due to the relationship between eligibility and military service. This assumption is reasonable for draft eligibility because it was randomly assigned based on date of birth. In regression form, the naive estimate for the effect of military service is the following:

$$S_{it} = \alpha_0 + \alpha V_{it} + \varepsilon_{it} \quad (5)$$

Table (3) shows the Wald estimates (using 2SLS) and the naive estimates (using OLS) of the effect of military service for both sample periods. The naive estimates are quite similar across the two sample periods. Veterans were about 11 percentage points more likely to be smokers than non-veterans. The Wald estimates are quite different. The Wald coefficient on veteran status for 1978-1980 indicates that serving in the military increased the probability of being a smoker by 32 percentage points. The corresponding estimate of 1 percentage point for the 1997-2005 surveys is not statistically different from zero.

### 4.3 2SLS estimates with controls

While the Wald estimates are simple and show a marked pattern of results, there are potential problems which we address further now. First, it has been documented that eligibility assigned by the draft lottery is slightly correlated with month of birth for the first lottery held in December 1969, due to imperfect mixing of the balls drawn from a container.<sup>10</sup> Because of this association, we include month of birth dummies as controls. We calculate first stage F statistics for the significance of the excluded instruments to check that our instruments are not weak, and we conduct the 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. Finally, we include race, age dummies, and cohort dummies as additional control variables.

For our main results we use a simple 2SLS specification, where the first stage is a linear probability model for veteran status, and the second stage is a linear probability model for current smoking.<sup>11</sup> Including controls, the model to be estimated becomes:

$$V_{it} = \delta_0 + \delta_1(D_{it} * B_{it}) + \delta_2 R_{it} + \delta_3 X_{it} + e_{it} \quad (6)$$

---

<sup>10</sup>See Feinberg, S.E. (1971), "Randomization and Social Affairs: The 1970 Draft Lottery", *Science*, 171, 255-261.

<sup>11</sup>As compared to 2SLS, Limited Information Maximum Likelihood (LIML) has been shown to have some desirable finite sample properties (Anderson 1982), and two-step linear Generalized Method of Moments (GMM) is more efficient in the case of multiple instruments and a single endogenous regressor. We find, however, that point estimates and standard errors from LIML and two-step GMM (available upon request) are very similar to our 2SLS estimates. Thus for ease of interpretation we focus on estimates from 2SLS, using heteroskedastic-robust estimates of standard errors.

$$S_{it} = \beta_0 + \beta_1 \hat{V}_{it} + \beta_2 R_{it} + \beta_3 X_{it} + u_{it} \quad (7)$$

Equation (6) is the first stage and equation (7) is the second stage.  $S_{it} = 1$  if individual  $i$  in survey year  $t$  is a current smoker, and  $V_{it} = 1$  if the individual is a veteran. The variable  $R_{it}$  is a race dummy equal to 1 if the individual is black.  $X_{it}$  is a vector of birth cohort dummies, age dummies, and month of birth dummies. The instrumental variables are  $D_{it} * B_{it}$ , which are birth cohort dummies interacted with draft eligibility. The parameter of interest is  $\beta_1$ , which measures the causal effect of serving in Vietnam on the probability of being an active smoker. Current smoking is a good candidate for the linear probability model because the sample proportion of smokers is not too close to 0 or 1.

A natural question to ask is how to include education in the model, if at all. An argument for omitting education is that it is endogenous in the smoking model, as it may be correlated with factors in the error term that affect smoking, such as the subjective rate of time preference. A possible argument for including education is that educational attainment could be a mechanism, beyond military service per se, by which draft eligibility influences smoking. Card and Lemieux (2001) find that for men born in 1950 or later, the Vietnam draft lottery had little or no effect on educational attainment. They do find positive effects of the Vietnam draft on educational attainment for men born in the mid to late 1940's, i.e. men not in our sample. Using more recent data (the 2000 Census) and draft eligibility as the instrument for Vietnam-era military service, Angrist and Chen (2007) find small but statistically significant positive effects of service on educational attainment for white men born in 1948-1952.<sup>12</sup> Based on the phase-out of educational draft deferments during the draft lottery period and the time pattern of educational attainment (in the CPS) for Vietnam veterans born 1948-1952, Angrist and Chen argue that the effects on schooling are due to increased educational attainment (subsidized by the GI Bill) in the years after military service, and not to schooling obtained to avoid the draft.

Our results are very similar when we include education as a control variable in specification checks. Also, in the NHIS data, we do not find a statistically significant relationship between draft eligibility and educational attainment, although our sample size is too small to rule out the small effect estimated by Angrist and Chen (2007). Later, in Section 5, we discuss an upper bound

---

<sup>12</sup>Angrist and Chen's draft-eligibility based estimates for whites indicate that military service increased years for schooling by about one-third of a year, and increased the chance of attaining at least a college degree by 0.05. The effects for non-whites are similar in magnitude but not statistically significant.

estimate for how much of the decline over time in our estimated effects of service on smoking may be due to increased educational attainment among veterans.

Our main results, estimated by 2SLS using draft eligibility interacted with year of birth as instruments for military service, are presented in Table (4). For the 1978-1980 time period, the lottery-based 2SLS estimate is larger (35.3 percentage points for current smoking) than the OLS estimate of 11.3, indicating a downward bias in the OLS estimate. Both coefficients are significant according to the reported heteroskedastic-robust standard errors. The first stage F statistics are well above the usual rule of thumb of 10, and the J-test does not reject the overidentifying restrictions imposed by our 3 instruments. While the 2SLS point estimates for 1978-1980 are substantially larger than

Table 4: Lottery-based estimates of the effect of Vietnam-era military service. Basic controls are cohort dummies, age dummies, birth-month dummies, and race. Instruments are birth cohort interacted with draft eligibility.

Dependent Variable: Smoker	Surveys	OLS	2SLS	F-stat	J-test
Basic controls	78-80	<b>0.113</b> (0.032)	<b>0.353</b> (0.145)	20.71	0.92
	97-05	<b>0.107</b> (0.014)	0.028 (0.105)	33.69	0.48
Basic controls plus college and marital status	78-80	<b>0.064</b> (0.033)	<b>0.322</b> (0.148)	20.63	0.85
	97-05	<b>0.080</b> (0.014)	0.055 (0.100)	35.03	0.39

F-stat: F-statistic for the test that instruments are jointly significant in the first stage.

J-test: p-value for the Hansen J-test of the over-identifying restrictions.

Observations: 1,452 for 78-80 results, and 6,553 for 97-05.

Heteroskedastic-robust standard errors.

the OLS point estimates, the standard errors for the 2SLS estimates are also fairly large. We use a regression based Hausman test for endogeneity to test whether the estimated 2SLS coefficients are significantly different from the OLS coefficients, and the test rejects the null hypothesis of no endogeneity at the 10% level ( $p=0.089$ ).

The results for the 1978-1980 surveys in Table (4) are nearly identical to the Wald estimates presented in Table (3). This is consistent with the fact that because draft eligibility was randomly assigned, additional control variables

are not essential to identify the parameter of interest. Table (4) also shows that we obtain very similar estimates when including indicators for college completion and marital status as additional control variables.

The fact that the lottery-based IV estimates for the effect of service on smoking are larger than OLS estimates in 1978-80 suggests that, on balance, unobservable factors made veterans *less* prone to smoking. This is because in order for the OLS estimate to be biased downwards, the correlation between the omitted variable (assuming there is only one) and smoking must have a different sign than the correlation between the omitted variable and military service. This may be related to the fact that health standards for enlistment into the military were relatively strict; as Angrist (1990) points out, over half of potential inductees in 1970 failed a physical or mental examination.<sup>13</sup> In economic terms, we can interpret our IV result for 1978-1980 as suggesting that, prior to military service, those who went on to become veterans had a lower reservation price for smoking initiation than those who did not.

Another potential explanation for the apparent downward bias of OLS may be that men induced to serve by the draft faced a different set of conditions than other men who served. Assuming that all of the reduced form effect of draft eligibility on smoking is attributable to increased military service, our lottery-based IV estimates represent the average effect of service on smoking behavior for those men induced to serve by the draft (i.e. a local average treatment effect, as shown by Angrist et al. 1996). It is possible that the smoking behavior of these “marginal” participants in the war 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 found 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.

For the 1997-2005 survey period, Table (4) shows that the OLS estimate for the effect of military service on smoking (10.7 percentage points) is similar to the OLS estimates for 1978-1980. However, the 2SLS estimate is only 0.028 and is not statistically different from zero, which is very different from the result for 1978-1980. The J-test comfortably fails to reject the null that 2SLS estimates using a subset of instruments differ only by sampling error from estimates using all instruments for the current smoker specification. Using

---

<sup>13</sup>Baskir and Strauss (1978) report that from 1967 to 1973, the failure rate for the pre-induction physical exam was 47%.

the regression based Hausman test, we cannot reject the null hypothesis of no endogeneity ( $p=0.448$ ). In addition, we find that our 2SLS estimates remain similar if we control for college completion and marital status. Thus, our results suggest that military service caused a large increase in smoking as of young adulthood, but this effect did not persist into later adulthood.

#### 4.4 Outcomes by draft lottery number intervals

The lottery-based 2SLS specification attributes all of the reduced form effect of draft eligibility on smoking to increased military service. Here we examine the validity of this assumption by looking at how smoking varies across more narrowly defined intervals of draft numbers. In particular, one concern is that having a very low draft number could have affected smoking via mechanisms other than military service, such as direct psychological distress or secondary effects resulting from a draft avoidance strategy. If this were the case, we might see distinct patterns in smoking by draft number within the draft-eligible group, such as highly elevated rates for men with very low numbers.

Figure 1: Sample proportion of current smokers by lottery number group and birth cohort.

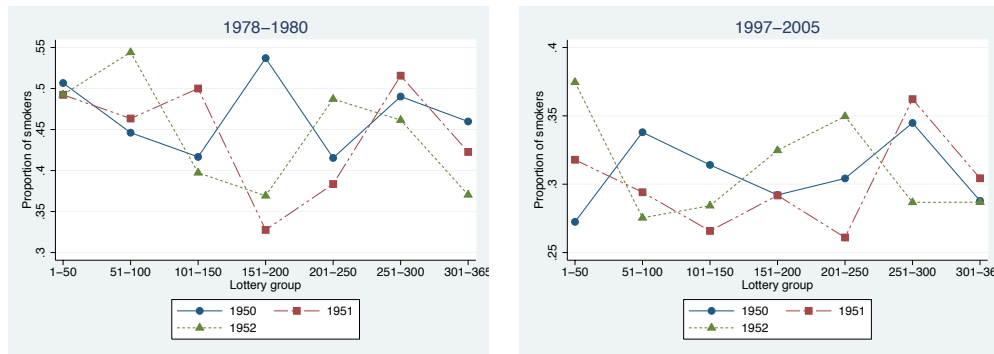
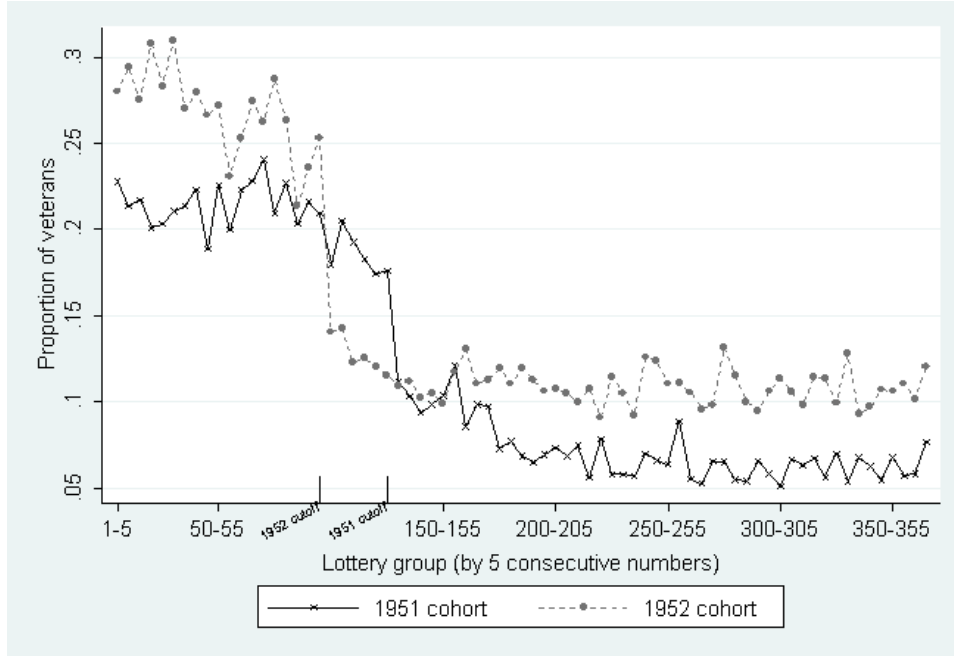


Figure (1) provides graphical evidence related to this possibility. Due to our modest sample sizes, we show means by groups of 50 consecutive lottery numbers (means by smaller groups are even noisier than those shown). Recall that the eligibility cut-offs were 195 for the 1950 cohort, 125 for the 1951 cohort, and 95 for the 1952 cohort. For the 1978-1980 sample, a sharp drop in smoking is observed to the right of the eligibility cutoff for all cohorts. These patterns are muted in 1997-2005 (consistent with our main results), and there is no clear pattern suggesting that very low lottery numbers had a distinctive

Figure 2: Proportion of men born in 1951 and 1952 who entered the military between July 1970 and December 1973, by draft lottery number. Whites only. Data source: Defense Manpower Data Center, provided to us by Joshua Angrist.



effect. In addition, there is no evidence for a monotonic relationship between draft lottery number and smoking, which might have suggested some direct effect of lottery numbers. We also show in Figure (2), using a much larger data set from the Defense Manpower Data Center,<sup>14</sup> that the relationship between draft number and veteran status approximates a relatively clean discontinuity at the cutoff numbers.

#### 4.5 Other smoking variables

In order to gain a fuller understanding of our main pattern of results, we examine several additional smoking variables available in NHIS. First, we look at current smoking in supplements to the NHIS in 1987 and 1988. Second, we look at variables other than current smoking: in particular, lifetime smoking, age of initiation, and number of cigarettes per day as of 1978-80, 1987-88, and 1997-2005. For simplicity, in this section we focus on reduced form comparisons

<sup>14</sup>Joshua Angrist used these data in his 1990 paper, and we thank him for sharing the data with us. The DMDC data are available for men born in 1951 and 1952, but not 1950.

between men with and without draft-eligible numbers.

Current smoking in 1987-88 was ascertained by NHIS in the exact same way as described earlier for the 1997-2005 surveys. Comparing by draft-eligibility, we find that smoking prevalence is 0.37 for both groups (Table 5). This suggests that the large effect observed in 1978-80 had already disappeared as of 1987-88.<sup>15</sup> Next we examine a dichotomous measure of lifetime smoking,

Table 5: Additional NHIS smoking variables: means by draft eligibility.

	1978-1980			1987-1988			1997-2005		
	Eligible			Eligible			Eligible		
	Yes	No	p-val	Yes	No	p-val	Yes	No	p-val
Current smoker	0.49	0.43	(0.02)	0.37	0.37	(0.93)	0.31	0.31	(0.91)
Ever smoked	0.65	0.59	(0.02)	0.59	0.61	(0.42)	0.59	0.59	(0.81)
Age first smoked	17.0	16.9	(0.88)	17.5	17.6	(0.69)	19.5	19.6	(0.93)
Cigs/day	20.8	21.0	(0.85)	24.6	25.1	(0.66)	19.8	19.5	(0.57)
Light smoker	0.24	0.20	(0.26)	0.22	0.23	(0.62)	0.31	0.32	(0.78)

p-values: chi-squared tests for 0-1 variables, t-tests for age first smoked and cigs/day.

which is based in all survey years on the answer to the stem question, “Have you smoked at least 100 cigarettes in your entire life?” As of 1978-80 we find that draft-eligible men have significantly higher lifetime smoking: 0.65 vs 0.59 (Table 5). By contrast, lifetime smoking is not significantly different by draft-eligibility as of 1987-88 (0.59 vs 0.61) or as of 1997-2005 (0.59 vs 0.59). It is interesting to note that the convergence in lifetime smoking comes from a decrease among draft-eligible men, rather than an increase among non-eligible men.

There are three possible, non-mutually exclusive explanations for this pattern over time: a change in the composition of the sample, misreporting of smoking history, or a true convergence in lifetime smoking probabilities. As we describe later in Section 5, a variety of evidence suggests that attrition is not a significant issue in this context, and we described earlier why immigration is not a significant issue either. By contrast, recall bias, or systematic errors made by individuals when they are asked to recall past events, may be important. Significant misreporting has been documented for former smok-

<sup>15</sup>We do not include these data in our main analysis because the null 2SLS results we find for 1987-1988 are not definitive on when the smoking stopped, because standard confidence intervals for the estimates include sizable effect sizes.

ers.<sup>16</sup> By contrast, self-reports of current smoking have been shown to be much more accurate. In the 1988-94 and 2001-2002 National Health and Nutrition Examination Surveys, only about 1 percent of people who said they were non-smokers had cotinine levels in their blood consistent with being an active smoker (Caraballo et al. 2001, West et al. 2007).

In our context, it seems plausible that misreporting by former smokers might operate differentially across draft eligibility status. Some veterans who were still smoking in 1978-1980 but who quit by the later sample period may have thought that smoking during their military days did not really “count”. This seems particularly plausible given that misreporting has been shown to be highest among former light smokers (Kenkel et al. 2004, Stanton et al. 2007), and draft-eligible men were more likely to smoke 10 or fewer cigarettes per day than non-eligible men (Table 5). Another piece of evidence suggesting that former smokers are misreporting is that the overall reported lifetime prevalence *decreases* over time, despite our evidence in Section 5 that changes in sample composition is not a significant issue.<sup>17</sup> In addition, for *women* born 1950-1952, our calculations from public use NHIS data files show that the proportion reporting having ever smoked falls from 0.485 in 1978-1980 to 0.424 in 1997-2005. Given that mortality rates for women are even lower than those for men in these cohorts, this marked decline over time in the proportion of women reporting having ever smoked casts further doubt on the accuracy of reports by former smokers. The remaining explanation is that non-eligible men “caught up” to eligible men between 1980 and 1997. This may seem unlikely, but it is important to keep in mind that the differences as of 1978-1980 were due to

---

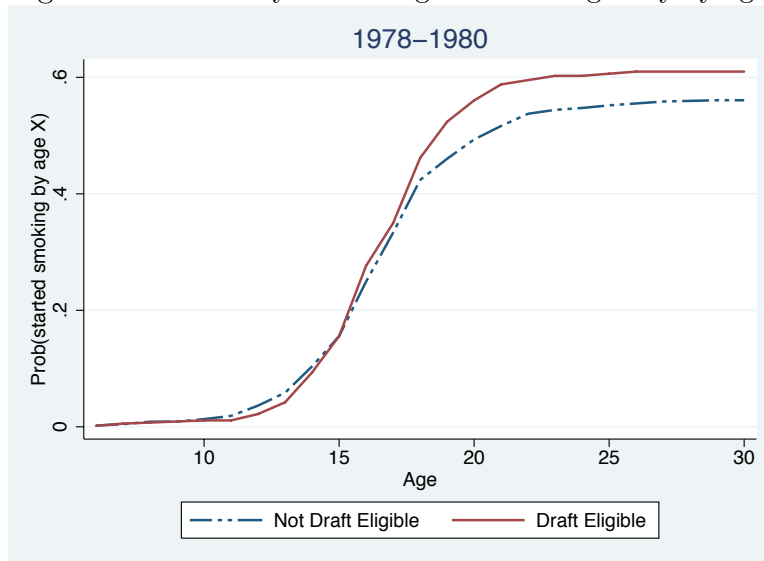
<sup>16</sup>For instance, Kenkel, Lillard, and Mathios (2004) show that among the NLSY respondents who contemporaneously reported being smokers in 1984, 26 percent reported in 1998 that they were not smokers in 1984. Glied (2002) points out that among the NLSY respondents who reported that they had never smoked daily in 1992, eight percent reported smoking at least one cigarette daily in 1984.

<sup>17</sup>A factor relevant to the decline in reported lifetime smoking is the presence of foreign-born men in the 1997-2005 NHIS sample. As explained earlier, we estimate based on the NESARC that in the 1997-2005 period 13% of men from the 1950-1952 cohorts are foreign-born. We also find in the NESARC that foreign-born men are less likely to report having ever smoked: 39% as compared to 56% of U.S. born men in these cohorts. Given that foreign-born men constitute about 1/8 of the sample, their inclusion in our NHIS estimates likely deflates our estimated p(ever smoked) by about 2 percentage points. (Along with misreporting, this helps explain why p(ever smoked) fell over time). The presence of foreign-born men in the data, however, cannot explain why the smoking *differential by draft eligibility* diminishes to zero among the 1950-1952 cohorts. Given that the estimated differential for the overall sample (U.S. plus foreign-born men) is zero, and the differential for foreign-born men is logically zero (as the vast majority of them were not subject to the draft), then the differential for U.S. born men must also be zero.

“marginal” smokers; i.e. men who would not have smoked by that point if not for having had a low draft number. From that vantage point, it is perhaps not surprising that many “marginal” non-smokers in the non-eligible group went on to try smoking after 1980.

We next examine the age of smoking initiation. In each survey this is asked as, “About how old were you when you first started smoking fairly regularly?” In each time period we find that the average age of initiation is not significantly different by draft-eligibility. The distribution of this variable, however, is probably more informative. In Figure (3) we show the probability that men initiated smoking by each age for the 1978-1980 sample. The first figure shows that the divergence in smoking behavior began around the age when men were subject to the draft (19-20).<sup>18</sup> In contrast, analogous figures for the later time periods (not shown) do not reveal any apparent differences in the distribution of initiation age by draft-eligibility; the graphs are nearly identical in each case. This supports the idea that misreporting by former smokers, rather than true convergence, explains the apparent convergence of lifetime smoking probability described earlier. If true convergence was the main explanation, then we would observe it in the distributions of initiation age.

Figure 3: Probability of starting to smoke regularly by age.



<sup>18</sup>The fact that the divergence started a year earlier than we would expect, at age 18, is presumably due to misremembering of the exact age.

Thus far we have focused on the extensive margin: the decision whether to smoke at all. We now look at the intensive margin, by examining responses to the question, “On the average, about how many cigarettes per day do you smoke?” As in the case of age of initiation, in each time period we do not observe any significant differences by draft-eligibility in the *mean* number of cigarettes (Table 5). As already noted, we do observe, however, a difference in the probability of being a “light smoker” (defined as 10 or fewer cigarettes per day) as of 1978-80. This foreshadows the apparent fact that draft-eligible men were more likely to quit after 1980, and, as noted above, is also consistent with the idea that in later periods, among former smokers, draft-eligible men were more likely to misreport (i.e., deny) having ever smoked.

## 4.6 Health effects

The evidence thus far suggests that draft-eligible men were much more likely to smoke through their mid to late twenties, but not much longer beyond that. We now examine whether this increase in smoking during young adulthood led to long-term health consequences. Although these men were still too young to experience significant mortality from smoking, as discussed in the next section, it is possible that they could have less severe health problems by the later sample period. In Table (6) we show 2SLS estimates, using our preferred specification (as in Table 4), for two health outcomes: self reported health and lifetime incidence of cancer. Respondents were asked to rate their health on a 5 point scale, where 1 is excellent and 5 is poor. The question about cancer asks the respondent if he has ever been diagnosed with cancer of any kind. Although the 2SLS estimates are relatively imprecise, they indicate

Table 6: Lottery-based estimates of the health effects of Vietnam-era military service. Controls are cohort dummies, age dummies, birth-month dummies, and race. Instruments are birth cohort interacted with draft eligibility.

Dependent Variable	Surveys	OLS	2SLS	F-stat	J-test
Reported Health	97-05	<b>0.084</b> (0.0321)	<b>-0.548</b> (0.252)	33.07	0.05
Cancer	97-05	0.009 (0.006)	-0.057 (0.042)	33.02	0.28

F-stat: test that instruments are jointly significant in the first stage.

J-test: p-value for the Hansen J-test of the over-identifying restrictions.

Observations: 6,592. Heteroskedastic-robust standard errors

that, if anything, men with draft-eligible numbers were slightly better off for these measures of health. The NHIS, of course, also offers the opportunity to examine a large number of other health measures. As described earlier, Dobkin and Shabani (2007) use the draft-lottery natural experiment and find very few significant health effects in any of several time periods from 1974-2004. They caution that the estimates are not precise enough to rule out meaningful differences for many measures, but at the least their results, along with our own estimates for self-reported health and cancer, support the idea that the increased smoking in young adulthood did not have large, negative health effects.

Another caveat pertinent to this result is that military service may have had effects on health via mechanisms other than smoking. The direction of this bias, if any, is unclear a priori. As noted earlier, the cohorts in our analysis were not likely to be involved in combat, but the stress of potential combat may have affected health. Also, participation in the military may have affected physical fitness later in life (although Dobkin and Shabani do not find any significant differences in hypertension, for example).

## **5 Why does the smoking effect dissipate over time?**

In this section we discuss two factors, post-service educational attainment and mortality, that could plausibly contribute to the disappearance over time of the smoking differential between men with and without draft-eligible lottery numbers. We find that these factors are unlikely to be significant in this context. We then discuss the implications of the fact that smoking behavior across the two groups appears to have converged after such a large differential in early adulthood.

Angrist and Chen (2007) use the draft-lottery IV approach to estimate that military service during the Vietnam-era led men to attain about one-third of a year of additional schooling in the years after they completed service. They find that this increase in education was likely related to educational subsidies offered to veterans. Given that there is a well-established inverse connection between education and smoking, this could account for a portion of the decline in smoking that we observe. Results from both De Walque (2007) and Grimard and Parent (2007) suggest that an upper bound on the size of the effect of educational attainment on smoking is about a 10 percentage point decline in the probability of being a current smoker for every year of education attained above high school. Combining these sets of results, it seems that increased

education due to military service may account for at most a  $\frac{0.10}{3} = 0.033$  decline in the differential probability of smoking. This is only a small portion (about one tenth) of the decline in our IV estimates between the early and later periods.

Attrition due to mortality is still a potential problem for our results for the 1997-2005 period. Given that we find no significant difference in smoking across draft eligibility status during that period, it is simplest to think about possible attrition bias for this reduced form relationship. The reduced form relationship between draft eligibility and smoking could be attenuated in the later survey period by either of two types of changes in sample composition. One possibility is that in the draft-eligible group, smokers were more likely to die than non-smokers. Another possibility is that in the draft-ineligible group, smokers were less likely to die than non-smokers. If smokers were more likely than non-smokers to die in both groups, which seems to be the most reasonable assumption, then there could still be attrition bias if mortality is higher in the draft eligible group.

In our data, however, we do not see evidence for differential mortality by draft eligibility status. If there were significant differences in mortality, we would see a reduction in the proportion of draft-eligible men between the earlier and later samples. As shown in Table (7), the proportion of draft-eligible men remained identical between the 1978-1980 and 1997-2005 NHIS samples: 38.0% in both cases. Furthermore, using a much larger sample (a subsample of the 2000 Census), Angrist and Chen (2007) also find no evidence of differential mortality; for men born in 1950-1952, the proportion who were draft eligible is almost identical to the proportion predicted based on a uniform distribution of dates-of-birth. The lack of evidence for differential mortality is perhaps not surprising, given that as of 1997-2005 these men were still in an age range (45-55) where smoking-related mortality is low (Sonnenschein and Brody, 2005).

We do not want to dismiss completely the possibility of differential mortality, however, given that Hearst et al. (1986) find for the 1950-1952 cohorts that the post-war mortality rate was slightly (1.04 times) higher among draft-eligible men than among non-eligible men, using records from California and Pennsylvania from 1974 to 1983. Using their results and other information, we now calculate an upper bound estimate of how attrition might attenuate the estimated differential in smoking by eligibility status in 1997-2005. We can approximate from national epidemiological data that 6.5% of men from the 1950-1952 birth cohorts died between the midpoints of the two sample

Table 7: Sample proportions of veterans and draft eligible men.

Cohort	1978-1980 NHIS			1997-2005 NHIS		
	Draft Eligible	Veteran	obs.	Draft Eligible	Veteran	obs.
1950	0.53	0.25	487	0.53	0.28	2,208
1951	0.36	0.23	534	0.34	0.21	2,189
1952	0.27	0.17	539	0.27	0.20	2,212
All Cohorts	0.38	0.22	1,560	0.38	0.23	6,609

Data Source: 1978-1980 and 1997-2005 NHIS. Males only.

periods (1979 and 2001, respectively).<sup>19</sup> National mortality rates by smoking status and age are not available, to our knowledge, so we make the conservative assumption that mortality was fully twice as likely among smokers during this period. If on average 40% of men were smokers during the time span of interest, then the 6.5% overall mortality rate would translate to about 8% for smokers and 4% for non-smokers. If we then assume that draft-eligible men were 1.04 times more likely to die, as estimated in Hearst et al. (1986), and make the additional conservative assumption that this “excess” mortality was entirely accounted for by *smokers* in the draft-eligible group (via a 1.08 relative risk), the mortality rates for the draft-eligible group would then be 8.64% for smokers and 4% among non-smokers, as compared to 8% and 4% for the ineligible group. Applying these mortality rates to the 1978-1980 smoking rates would cause the smoking rate for draft-eligible men to fall from 50.0% to 48.8% and that for draft-ineligible men to fall from 43.5% to 42.5%, meaning that the difference would fall from 6.5% only to 6.3%.<sup>20</sup> Therefore, even under conservative assumptions, it appears that mortality could explain very little of the dissipation in the smoking effect that we observe.

Another possible explanation for the dissipation of the smoking effect is that it is simply due to sampling error or some other anomalous feature of the NHIS data. As another check, we examine a different data source, the 2002-2003 National Epidemiologic Survey on Alcohol and Related Conditions (NESARC), which was conducted and sponsored by the National Institute on Alcohol Abuse and Alcoholism. This publicly available data set includes in-

<sup>19</sup>We used annual mortality rates by age group for men, as reported by the National Vital Statistics in 2001. Reference: Table 1 in Centers for Disease Control (2001).

<sup>20</sup>The smoking rate among draft-eligible men falls from  $\hat{S}_{1979}^e=0.50$  to  $\hat{S}_{2001}^e = \frac{50-50*0.0864}{(50-50*0.0864)+(50-50*0.04)}$ . The smoking rate among draft-ineligible men falls from  $\hat{S}_{1979}^{ne}=0.435$  to  $\hat{S}_{2001}^{ne} = \frac{43.5-43.5*0.08}{(43.5-43.5*0.08)+(56.5-56.5*0.04)}$ .

formation on smoking and exact date of birth. It does not include information on veteran status so we cannot repeat our full IV analysis. Instead, we simply compare means for current smoking across draft-eligibility status. We find that draft-eligible men are only slightly more likely to be smokers (0.39 versus 0.37), and the difference is not significant ( $p$ -value=0.5), again supporting the idea that the differential dissipated over time.<sup>21</sup>

Given that this dissipation is evident in two different national data sets and cannot be explained by education or attrition, our results suggest that for the men in our analysis smoking behavior is highly malleable over time. More specifically, a large randomly induced increase in the likelihood of smoking in young adulthood (these men were ages 25-30 in 1978-80) diminished to being small and statistically insignificant in middle age (they were ages 45-55 in 1997-2005). For these men, the addictiveness of cigarettes does not appear to have been sufficient to compel them to smoke throughout adulthood. This finding runs counter to the conventional wisdom, based on epidemiological evidence cited earlier, that smoking early in life “dooms” people to smoke later in life.

When considering whether this basic result would generalize to other contexts, such as non-veterans, females, or more recent cohorts, it is important to consider a few key factors. First, serving in the military during the Vietnam era is obviously a different experience from civilian life on a number of dimensions. It is unclear whether these factors would, on balance, make people more or less likely to persist in their smoking through adulthood, given that they are smoking in young adulthood. On the one hand, to the extent that certain factors associated with military life – such as stress or access to subsidized prices (at military stores) or proximity to peer smokers – persist beyond the period of military service, continued smoking would only be *more* likely among veterans. The fact that we still do not observe this continued smoking would suggest that this result would be especially likely to hold in non-veteran populations. On the other hand, there was a shift in culture and policy related to smoking in the military during the 1980s and 1990s. For example, the Veterans Health Administration (VHA) increased efforts to identify and counsel smokers, to the point where it was referred to as a “model health care system for smoking cessation” by a leading tobacco control expert (Schroeder 2004). Assuming that health care providers in general have increased smoking cessation efforts in the past 10-15 years, the VHA of the 1990s may be roughly representative of health systems overall today.

The dissipation of the smoking differential over time may also be related to the fact that men induced to smoke by having a low draft number can be

---

<sup>21</sup>The sample sizes are 391 draft eligible men and 614 non-eligible.

thought of as “marginal smokers”; they would not have been smoking as of 1978-1980 if not for having been draft eligible (and serving in the military as a result). People on the margin of smoking or not smoking are arguably the most relevant for policymakers considering whether to make incremental changes in one direction or another. It is important to note, however, that our main results do not necessarily apply to “always-smokers”, people who would have been smoking as young adults regardless of their draft number.

## 6 Conclusion

Using the Vietnam era draft lottery as a natural experiment, we find that military service caused men born in 1950-1952 to be 35 percentage points more likely to smoke as of their mid to late 20s, but this effect was markedly attenuated later in adulthood. We also find that there were no measurable health consequences as a result of the increased smoking during young adulthood, which is consistent with Dobkin and Shabani’s (2007) examination of a much larger set of health variables.

As described above, generalizing these findings to other contexts should be done with caution, particularly due to some of the unique features of military service during the Vietnam era. Nevertheless, with these caveats in mind, our results speak to the strength of cigarette addiction over the long-term for a large segment of the population at the time. In effect, this is the first large-scale study to examine the long-term consequences of a randomly assigned exposure that significantly affected smoking behavior. The results are consistent with evidence on the long-term effects of cigarette taxes during adolescence (Gruber and Zinman, 2001; Glied, 2002), in that the effects are substantially diminished over time. For health policymakers and consumers in general, our results can be viewed as an affirmation that smoking during young adulthood does not compel one into lifelong smoking, and by quitting in young adulthood one can substantially mitigate longer-term health consequences.

## 7 References

Ahmad, S, Billimek, J. “Limiting Youth Access to Tobacco: Comparing the long-term health impacts of increasing cigarette excise taxes and raising the legal smoking age to 21 in the United States.” *Health Policy* 2007; 80(3):378-91.

- Angrist, J. "Lifetime Earnings and the Vietnam Draft Lottery: Evidence from Social Security Administration Records." *American Economic Review* 1990; 80(3): 313-336.
- Angrist, J, Imbens, G, Rubin, D. "Identification of Causal Effects Using Instrumental Variables." *Journal of the American Statistical Association* 1996; 91(434): 444-455.
- Angrist, J, Chen, S. "Long-term Consequences of Vietnam-Era Conscription: Schooling, Experience, and Earnings." NBER Working paper 2007; #13411.
- Anderson, T, Kunitomo N, Sawa T "Evaluation of the Distribution Function of the Limited Information Maximum Likelihood Estimator." *Econometrica* 1982; 50(4): 1009-1027.
- Baskir, L, Strauss, W. "Chance and Circumstance: The Draft, the War, and the Vietnam Generation." New York: Knopf, 1978.
- Becker, G, Murphy, K. "A Theory of Rational Addiction." *Journal of Political Economy* 1988; 96: 675-700.
- Becker, G, Grossman, G, Murphy, K. "An Empirical Analysis of Cigarette Addiction." *American Economic Review* 1994; 84(3): 396-418.
- Bedard, K, Deschênes, O. "The Long-Term Impact of Military Service on Health: Evidence from World War II and Korean War Veterans." *American Economic Review* 2006; 96(1): 176-194.
- Caraballo, R, Giovino, G, Pechacek, T, Mowery P. Factors Associated with Discrepancies between Self-reports on Cigarette Smoking and Measured Serum Cotinine Levels among Persons Aged 17 Years or Older: Third National Health and Nutrition Examination Survey, 1988-1994. *American Journal of Epidemiology* 2001; 153: 807-14.
- Card, D, Lemieux, T. "Going to College to Avoid the Draft: The Unintended Legacy of the Vietnam War." *American Economic Review* 2001; 91(2): 97-102.
- Centers for Disease Control. *National Vital Statistics Report* 2001; 49(11).
- Chaloupka, F. "Rational Addictive Behavior and Cigarette Smoking." *Journal of Political Economy* 1991; 99: 722-742.
- De Walque, D. "Does education affect smoking behaviors? Evidence using the Vietnam draft as an instrument for college education." *Journal of Health Economics* 2007; 26(5): 877-895.

- Dobkin, C, Shabani, R. "The Health Effects of Military Service: Evidence From the Vietnam Draft", *Economic Inquiry* 2007; 47(1): 69-80.
- Glassman, A, Helzer, J, Covey, L, et al. "Smoking, Smoking Cessation and Major Depression." *JAMA* 1990; 264: 1546-1549.
- Glied, S. "Is Smoking Delayed Smoking Averted?" *American Journal of Public Health* 2003; 93(3): 412-6.
- Glied, S. "Youth Tobacco Control: Reconciling Theory and Empirical Evidence." *Journal of Health Economics* 2002; 21: 117-135.
- Grimard, F, Parent, D. "Education and Smoking: Were Vietnam war Draft Avoiders Also More Likely to Avoid Smoking?" *Journal of Health Economics*, 2007; 26(5): 896-926.
- Gruber, J, Koszegi, B. "Is Addiction 'Rational'? Theory and Evidence." *Quarterly Journal of Economics* 1991; 116: 1261-1303.
- Gruber J, Zinman J. "Youth Smoking in the U.S.: Evidence and Implications." In: Gruber J, ed. *Risky Behaviors among Youths*. Univ of Chicago Press 2001: 69-120.
- Harris, R, Hebert, J, Wynder, E. "Cancer Risk in Male Veterans Utilizing the Veterans Administration Medical System." *Cancer* 1989; 64: 1160-8.
- Hearst, N, Newman, T, Hulley, S. "Delayed Effects of the Military Draft on Mortality: A Randomized Natural Experiment." *New England Journal of Medicine*; 1986.
- Kenkel D, Lillard, D, Mathios A. "Accounting for Misclassification Error in Retrospective Smoking Data." *Health Economics* 2004; 12: 1031-1044.
- Klevens, R et al. "The Association between Veteran Status and Cigarette-smoking Behaviors." *American Journal of Preventive Medicine* 1995; 11(4): 245-250.
- Levy D, Cummings, K, Hyland, A. "A simulation of the effects of youth initiation policies on overall cigarette use." *American Journal of Public Health* 2000; 90: 1311-4.
- Merline, A, O'Malley, P, Schulenberg, J, Bachman, J, Johnston, L. Substance Use among Adults 35 Years of Age: Prevalence, Adulthood Predictors, and Impact of Adolescent Substance Use. *American Journal of Public Health* 2004; 94: 96-102.
- Orzechowski, W, Walker, R. *The Tax Burden on Tobacco* 2006. V41. Arlington, VA.
- Schroeder, S. "Tobacco Control in the Wake of the 1998 Master Settlement Agreement." *New England Journal of Medicine* 2004; 350: 293-301.

- Sonnenschein, E, Brody, J. "Effect of Population Aging on Proportionate Mortality From Heart Disease and Cancer, U.S. 2000-2005." *Journals of Gerontology Series B: Psychological Sciences and Social Sciences* 2005; 60: S110-S112.
- Stanton, C., et al. Consistency of Self-reported Smoking over a 6-year Interval from Adolescence to Young Adulthood. *Addiction* 2007; 102: 1831-1839.
- Stellman, S, Stellman, J, Koenen, K. "Enduring Social and Behavioral Effects of Exposure to Military Combat in Vietnam." *Annals of Epidemiology* 2000; 10(7): 480.
- U.S. Department of Agriculture Economic Research Service. Tobacco Briefing Room: [www.ers.usda.gov/Briefing/tobacco](http://www.ers.usda.gov/Briefing/tobacco), Tables 40, 131, and 132. Accessed June 28, 2007.
- U.S. Department of Defense, Office of the Inspector General. "Economic Impact of the Use of Tobacco in DOD." Report 97-060, Dec. 1996.
- U.S. Public Health Service, Surgeon General. "Reducing the Health Consequences of Smoking: 25 Years of Progress. A Report from the Surgeon General." No. 89-8411, 1989.
- U.S. Department of Health and Human Services. A Report of the Surgeon General: the Health Benefits of Smoking Cessation. Washington (DC): 1990.
- Wald, A. "The Fitting of Straight Lines if Both Variables are Subject to Error." *Annals of Mathematical Statistics* 1940; 11(3): 284-300.
- West, R., et al. Can We Trust National Smoking Prevalence Figures? Discrepancies Between Biochemically Assessed and Self-Reported Smoking Rates in Three Countries. *Cancer Epidemiology Biomarkers and Prevention* 2007; 16: 820-822.
- Winefield HR, et al. "Psychological Concomitants of Tobacco and Alcohol Use in Young Australian Adults." *British Journal of Addiction* 1989; 84: 1067-1073.