Does Military Service Make You a More Violent Person?:
Evidence from the Vietnam Draft Lottery

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Abstract

In this paper, I use the Vietnam draft lottery as a natural experiment to measure the effects of military service on crime. First, I present time-series evidence on self-reported sexual assault, domestic abuse, and incarcerations. I find that men from Vietnam-era cohorts are nearly twice as likely as those from surrounding cohorts to report having committed sexual assault. No such effects appear in the time-series for domestic abuse or incarcerations. Adjusting for the draft’s effects on education does not change this result. Next, I use the Vietnam draft lottery to measure military service’s effects on incarcerations. I find that the incarceration rate is slightly higher than average among men whose lottery numbers were called during Vietnam. However, this difference is small and insignificant, even after adjusting for the lottery’s positive effects on education. I propose a handful of explanations to reconcile these findings.

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This study uses the Vietnam draft lottery as a natural experiment to estimate the effects of military service on criminal activity. Correctly estimating these effects can have important implications for public policy and for the understanding of violent behavior more generally. The optimal military policy should take all costs into account, including any effects of military service on crime. The Vietnam draft also constitutes one extreme form of exposure to violent and potentially desensitizing stimuli. Studying this episode can help provide insights into the degree to which environmental factors influence violent or criminal activity.

Military service could affect young men’s propensities toward violence and criminal behavior through a variety of mechanisms. Combat stress and captivity have been shown to produce long-term adverse psychological effects, including suicide, depression, irritability, alcoholism, and nightmares (Newman, Hearst, and Hulley, 1986; US Centers for Disease Control, 1988; Grossman and Siddle, 2000; Page and Engdahl, 1991; Page, Engdahl, and Eberly, 1997; Gold and others, 2000). In addition, psychologists have hypothesized that combat and combat training permanently break down the mind’s natural barriers to committing violent acts (Grossman, 2000; Grossman and Siddle, 2000). Military service has been found to substantially reduce men’s wages (Angrist, 1990), which could induce them to commit property crimes like theft. Military training and experience also teach young men obedience and discipline, which could lower their rates of criminality.

Previous studies of the effects of military service on crime have yielded mixed results. Mumola (2000) finds that veterans are no more likely to be in prison than are non-veterans of the same age. Among those in jail, however, he finds that veterans are more likely than non-veterans to have committed violent crimes. Bouffard (2003) finds that men who served in the Vietnam Era were slightly less likely than average to commit crimes as adults. Among Vietnam
veterans, Yager, Laufer, and Gallops (1984) find positive correlations between combat exposure and arrests and convictions. They find this association primarily for nonviolent crimes.

One possible explanation for these mixed results is omitted variables bias. Veterans are more likely than non-veterans to be white or to have graduated from high school. Men who voluntarily enlist may also have higher underlying levels of aggression than men who do not enlist. Among Vietnam veterans, those who experienced combat were less educated and came from poorer families than those who did not experience combat (Harris and Associates, 1980). Consequently, cross-sectional comparisons may not accurately estimate the true effects of military service and combat. Moreover, the direction of this bias is unclear.

To estimate the causal relationships of interest, we must find a variable that affects military service but does not otherwise affect crime. To solve this problem, I use the Vietnam draft lottery. The draft lottery randomly assigned draft eligibility to 19- to 26-year-old males based on their birthdays. Hence, for reasons totally unrelated to their underlying levels of aggression or criminality, some men faced higher conscription risk than others. Other authors have used this approach to estimate the effects of draft eligibility on a variety of outcomes (Hearst, Newman, and Hulley, 1986; Angrist, 1990, 1991; Angrist and Krueger, 1992).

The first pieces of evidence presented in this study relate to the timing of the Vietnam War. Section IV of this paper shows rates of incarceration, self-reported sexual assault, and self-reported domestic abuse by birth cohort. I find that men who were draft age during the Vietnam era are disproportionately likely to report having committed sexual assault. No such pattern appears for domestic abuse or incarceration rates.

Next, using individual-level data on inmates, I estimate reduced form relationships between Vietnam draft lottery results and criminal behavior. For each lottery, I divide the data
into cohorts who were affected by that lottery and cohorts who were not. To measure the fraction of the population born on each day, I use the unaffected cohorts as a control group. Among affected cohorts, men whose lottery numbers were called were draft eligible and faced elevated risk of conscription. I estimate the draft’s effect on crime by comparing the fraction on inmates with this set of birthdays between affected and unaffected cohorts.

Using this approach, I find small, insignificant, positive effects of military service on crime. I measure short-term effects of the draft lottery on crime using data from a national survey of inmates in 1979. I measure long-term effects using administrative data on prison admissions from 1983 to 2001. I find large short-term effects for men born in 1952 (the youngest cohort). Of the cohorts affected by the draft lottery, men born in 1950 are likely to have seen the most combat. For this cohort, draft eligibility is found to have small long-term effects on crime. For most cohorts, however, I find that the long-term and short-term effects are insignificant and close to zero.

I propose a handful of explanations to reconcile these results for incarceration rates with the results for self-reported sexual assault. Socialization, training, or experience in the military could have made veterans report assault more honestly than average. The sexual assault survey question used here asks whether the respondent has ever forced a woman to commit sexual acts. This measure could capture assault that occurred in Vietnam, where men were relatively anonymous and may have felt prejudice against local women. This measure could also be capturing extremely large short-term effect of military service on violence that totally disappeared 5 years later. The evidence presented here is not consistent, however, with a story in which military service has large long-term effects on violence or crime.
II. Key Institutional Factors about the Vietnam Draft

Through the Vietnam draft lottery, the United States Selective Service System randomly assigned conscription risk to young men based on their birthdays. Each birthday in the year was assigned a random number ranging from 1 to 366. The first draft lottery occurred in December 1969, more than 5 years after the United States entered the Vietnam War. This first lottery applied to men born between 1944 and 1950. The majority of men who were drafted based on this lottery were born in 1950. Men born in 1951 were subject to a different lottery, which was held in July, 1970. Men born in 1952 were subject to a lottery that was held in August, 1971. Additional lotteries were held after 1971, but no more men were drafted based on these lotteries.

In the year after each lottery drawing, Selective Service announced a cutoff level for lottery numbers. Men with lottery numbers at or below that cutoff were treated as draft eligible, and men with lottery numbers above the cutoff were not. Some men failed the military’s physical or mental aptitude requirements or obtained exemptions or deferments for other reasons. Among the remaining men, only those with lottery numbers at or below the cutoff were drafted between 1970 and 1972. For the first lottery, the cutoff was 195. For the second lottery, the cutoff was 125, and for the third lottery, the cutoff was 95. Angrist (1990, 1991) and Angrist and Krueger (1992) describe the Vietnam draft lotteries in more detail.

A considerable fraction of Vietnam era draftees were exposed to training and combat situations that could have plausibly made them more violent people. Of the 2 million Vietnam era draftees, all received combat training, and 53% also served in the Vietnamese Theater of Operations. Of those, 78% saw at least some moderate form of combat, and 36% saw heavy combat (Harris and Associates, 1980). Among United States infantrymen facing intense combat, the fraction firing their weapons increased from 15% to 95% between World War II and
Vietnam. The US Army achieved this increase in part by training soldiers with human-shaped, moving targets (Grossman and Siddle, 2000, pg. 146; Marshall, 1947, pg. 56). Among all veterans who served in the Vietnamese Theater, 43% thought they killed someone there, and an additional 10% were unsure (Harris and Associates, 1980). Roughly one third of all Vietnam veterans report symptoms of depression, anxiety, and post-traumatic stress (Shane, 2004; US Centers for Disease Control, 1988).

Most of the United States combat operations in Vietnam occurred before the draft lottery began. Roughly 0.9% of the draftees who served in the Vietnam Era suffered combat-related deaths. However, of those draftees who began service in 1970 or later, only 0.2% died of combat. Hence, exposure to combat was probably larger for earlier cohorts than for those exposed to the draft lottery. However, men drafted through the lottery were younger than other draftees. Consequently, they may have been especially psychologically vulnerable to the combat they did experience.

III. Data on Inmates and Prison Admissions

To measure crime, this study makes use of two individual-level datasets on convicted criminals. The first is the 1979 Survey of Inmates at State Correctional Facilities (ISCF). The second dataset is the National Corrections Reporting Program (NCRP), which covers state and federal prison admissions from 1983-2001. The number of states contributing to the NCRP varies from year to year. To adjust for this variation, observations are weighted by the fraction

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3 Data sources: Card and Lemieux (2001) for semi-annual inductions and Statistical Abstract of the United States for combat deaths. Draftees typically spent the first year in training and the second year in active duty. To calculate the rate of combat deaths, it is assumed that any draftee who died in combat died in his second year.
of nationwide prison admissions included in the dataset for that year.\textsuperscript{4} For this analysis, the NCRP data are restricted to prison admissions resulting from new court commitments. While the ISCF is a representative of inmates (after applying the sample weights), the NCRP is not. In 1983, 29 states contributed data to the NCRP. In 2001, the latest available year, the NCRP includes data from 36 states. Kuziemko and Levitt (2001) describe the NCRP data in more detail. Both datasets include individuals’ birthdays. Given individuals’ birthdays, we can uniquely identify each man’s draft lottery number using data from the United States Selective Service System.

IV. Time-Series Evidence on Military Service and Violent Behavior

We begin our empirical analysis by considering time-series evidence on the relationship between military service and violent behavior. The dotted line in Figures 1 and 2 shows the fraction of males who served in the military by birth year. This same time-series appears in Card and Lemieux (2001). This variable exhibits a downward trend that is interrupted by a bulge for the 1940 to 1951 cohorts. The departure from trend occurred due to an increase in procurement of troops for the Vietnam War. It is unlikely that underlying determinants of men’s aggression changed in this same nonlinear way. If military service makes men more violent, then we should observe disproportionately high rates of violence among cohorts affected by the Vietnam War.

The dashed line in Figure 1 is 10 times the fraction of males born that year who were in state correctional facilities in 1979. The solid line is the per capita rate of prison admissions from 1983-2001 for men born that year. Both lines exhibit upward trends, indicating that younger men are more likely to be incarcerated in a given time period. However, neither the

\textsuperscript{4} This weight ranges from 1.02 (in 1990) to 1.74 (in 1983). Data on new court commitments to prison taken from Bureau of Justice Statistics (1998). Data are not available for 1999-2001. For those years, the 1998 weight of 1.25 is used.
dashed line nor the solid line appears higher for the cohorts affected by the Vietnam War. Hence, from this simple comparison, it appears that military service does not affect criminal behavior.

The rates of self-reported violent behavior shown in Figure 2 provide another perspective on the relationship between military service and crime. The dashed line in Figure 2 shows domestic abuse by cohort, as measured by the 1995-1997 Chicago Health and Social Life Survey. Domestic abuse is measured as ever threatening, pushing, hitting, beating up, or using a knife or gun on one’s partner. These self-reports were obtained using a computer-based questionnaire. The sample used here includes the 291 male respondents in the sample who fell within the relevant ages. Of these, 24 (including 4 veterans) reported having committed some form of domestic abuse. The solid line in Figure 2 shows sexual assault by birth cohort, as measured by the 1992 National Health and Social Life Survey. The exact question asked is “Have you ever forced a woman to do something sexual that she did not want to do?” Respondents answered this question on paper and returned the completed questionnaires to the interviewers in sealed envelopes. The sample used here includes the 657 male respondents who fell within the relevant age range. Of these, 26 responded affirmatively, 14 of whom were veterans. To make the graphs easier to read, 3-year moving averages are presented for each variable. As a first approximation, the cohorts with elevated rates of military service exhibit elevated rates of sexual abuse. After controlling for a time trend, these two time-series are significantly positively correlated. The time-series for domestic abuse, on the other hand, is uncorrelated with the time-series for military service. Given the low numbers of affirmative responses, however, these results should be taken as suggestive evidence.\(^5\)

\(^5\) An additional 2 men refused to answer all the domestic abuse questions and are dropped from the Chicago sample. Treating these non-responses as affirmative answers produces the same result. An additional 51 men (including 20
One possible source of bias in the analysis above is that many men attended school to avoid the draft. Lochner and Moretti (2004) find that high school education significantly reduces criminal behavior. Among high school graduates, however, they find that additional years of schooling have negligible effects on incarceration. The Vietnam draft primarily increased education at the college level. Males’ high school completion rates did rise by about 1 percentage point relative to females during the Vietnam Era. However, the effect of this change on crime is likely to be so small that it would not be perceptible on these graphs. The remainder of this paper uses the Vietnam draft lottery as an instrument for military service. Men did not learn their lottery numbers until age 19 or later. For this reason, lottery-induced risk of military service had considerably smaller effects on education than did the cohort-level risk measures used above. Hence, the bias due to education is likely to be negligible for the remaining results.

V. Econometric Methods

This next section describes in detail the econometric strategy used in this analysis. Although the data in this study are restricted to prison inmates, the model developed here describes both criminals and non-criminals. For the $i$th man in birth cohort $c$, we can write:

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veterans) refused to answer the sexual abuse question and are dropped from the national sample. Treating these non-responses as affirmative answers leads to slightly larger but only marginally significant effects.

6 Comparing the 1946 and 1947 cohorts with earlier and later cohorts from the 1980 Public Use Microdata Samples of the US Census.

7 Lochner and Moretti (2004) find that high school completion decreases incarceration rates by roughly one half to two thirds. Hence, a one percentage point increase in education would lower crime by a factor of 0.005 to 0.007. Given benchmark levels of about 0.05 and 0.025 in Figure 1, this effect would lower crime by about 0.0003 and 0.0002, respectively.

8 For the cohorts most affected by the Vietnam War, cohort-level risk of military service increased education by about 0.6 years. During the draft lottery, Angrist and Krueger (1992) find that draft eligible men completed roughly 0.0-0.2 years more schooling than other men. Because men did not know their lottery numbers before age 19, these effects on education were probably concentrated at the college level. Thus, the lottery’s effects on high school completion were probably less than one third as large as the effects of cohort-specific service risk. Cohort-level education calculations performed by comparing the 1946 and 1947 cohorts to surrounding cohorts, using females as a control group. Draft lottery calculations performed using coefficients from Table 2 of Angrist and Krueger’s paper, excluding RSN categories for which draft eligibility is ambiguous.
(1) \[ \text{Crime}_i = \alpha \text{Served}_i + \gamma_c + u_i \]

where \text{Crime}_i is the number of crimes committed by individual \( i \) and \text{Served}_i is a dummy for whether individual \( i \) served in the military. The parameter of interest – the effect of military service on crime – is \( \alpha \). The model also allows for fixed cohort effects \( \gamma_c \) and random error \( u_i \).

If \text{Served}_i is correlated with the unobserved components of the crime equation, then Ordinary Least Squares will not consistently estimate \( \alpha \). For instance, particularly low-skilled or aggressive men may self-select into the military, and they may also exhibit high underlying rates of criminality. The military’s physical and mental aptitude requirements may also be correlated with crime in ways that are difficult to observe.

The approach taken here makes use of the fact that the Vietnam draft lottery randomly assigned conscription risk across otherwise identical men. Let \( \text{Called}_{1969,b,i}, \text{Called}_{1970,b,i} \) and \( \text{Called}_{1971,b,i} \) be indicators for whether individual \( i \)'s birthday \( b \) was called in each of the 3 draft lotteries.\(^9\) We can then write an equation to predict military service:

(2) \[ \text{Served}_i = \sum_{j=1969}^{1971} \delta_{j,c} \cdot \text{Called}_{j,b,i} + \eta_c + \nu_i, \]

where

(3) \[ \delta_{j,c} = \delta_j \text{ if cohort } c \text{ was affected by lottery } j, \text{ and} \]

\[ = 0 \text{ otherwise.} \]

This specification also includes fixed cohort effects \( \eta_c \) and random error \( \nu_i \). The estimates for \( \delta_j \) used in this study come from Angrist (1990). By combining Equations (1) and (2) we can express criminal behavior as a reduced-form function of draft lottery results:

\(^9\) In addition to the lottery’s effects on conscription, Angrist (1991) finds significant effects of lottery numbers on enlistment behavior. Before the cutoff was announced, some individuals with moderately low lottery numbers may have enlisted to obtain better assignments than draftees. However, the majority of the effects of the draft lottery are effectively captured by the “draft eligible” and “not draft eligible” categories.
\begin{equation}
 Crime_i = \sum_{j=1969}^{1971} \pi_{j,c} \times Called_{j,b,i} + \theta_c + \epsilon_i, \end{equation}

where

\begin{equation}
 \pi_{j,c} = \pi_j \text{ if cohort } c \text{ was affected by lottery } j, \text{ and }
 = 0 \text{ otherwise,}
\end{equation}

where \(\pi_j = \alpha \delta_j, \theta_c = \gamma_c + \alpha \eta_c\), and \(\epsilon_i = u_i + \alpha \nu_i\). Because the lottery was random, \(Called_{j,b,i}\) should be totally uncorrelated with \(\epsilon_i\).

Without data on the birthdays of non-inmates, it is not possible to estimate \(\pi_j\) directly using two-stage least squares. However, it is possible to use inmate data to indirectly estimate the effects of draft eligibility on crime under fairly general conditions. Suppose that the fraction of individuals whose birthdays satisfy \(Called_{j,b,i} = 1\) equals some number \(\lambda\). Now, let us assume that the fraction of individuals with these birthdays is the same for every cohort.\(^{10}\) Let \(c = 1\) be a cohort that is not affected by lottery \(j\). Let \(c = 2\) be a cohort that is affected by lottery \(j\). For cohorts unaffected by the lottery, the fraction with \(Called_{j,b,i} = 1\) is the same for criminals as for the general population.\(^{11}\)

\begin{equation}
 E[Called_{j,b,i} \mid Crime_i > 0, c = 1] = \lambda.
\end{equation}

Hence, we can use data on the unaffected cohort to estimate \(\lambda\). Among cohorts affected by lottery \(j\), military service increases the total number of crimes by \(\lambda \pi_j\) times the population size.

We have:

\begin{equation}
 E[Called_{j,b,i} \mid Crime_i > 0, c = 2] = (\lambda \theta_2 + \lambda \pi_j)/(\theta_2 + \lambda \pi_j).
\end{equation}

Together, these equations suggest a test for whether military service affects crime. If military service has no effect on crime, then the mean for \(Called_{j,b,i}\) should be equal across

\(^{10}\) One might suppose that leap years could produce small changes in this fraction. Dropping February 29th birthdays from the data does not change the results from this study.

\(^{11}\) The NCRP data include one observation for every new court commitment. Hence, the actual means calculated for these data are weighted by the number of crimes committed by each individual.
cohorts 1 and 2. All else equal, a person in cohort 2 with \( \text{Called}_{j,b,i} = 0 \) is expected to commit \( \theta_2 \) crimes. However, making this person draft eligible would raise his expected number of crimes to \( \pi_j + \theta_2 \). Let us now consider the relative effect of draft eligibility, which we can express as the ratio of these two, \( \pi_j/\theta_2 \). For example, if \( \pi_j/\theta_2 \) were equal to 1, then we would say military service doubled a man’s propensity to commit crimes. Rearranging Equations (6) and (7), we can express this relative effect of draft eligibility in terms of observable sample means:\(^12\)

\[
\pi_j/\theta_2 = (L_2 - L_1)/[(1 - L_2)*L_1],
\]

where \( L_1 = E[\text{Called}_{j,b,i} | \text{Crime}_i > 0, c = 1] \), and \( L_2 = E[\text{Called}_{j,b,i} | \text{Crime}_i > 0, c = 2] \).\(^13\)

One may wish to interpret the estimates in this paper in a more general setting in which the effects are heterogeneous. Certain groups – for instance disabled persons and people with political connections – faced no risk of being drafted. The draft lottery provides no information about the effects that military service would have on these individuals. In econometric terms, if \( \alpha \) and \( \delta_j \) vary across individuals, the estimates in this paper can be interpreted as treatment on the treated. These estimates constitute weighted averages of \( \alpha_j \), where the weights are \( \delta_j \), the effects of draft eligibility on military service. For a more detailed discussion of estimation of heterogeneous effects, see Heckman, Urzua, and Vytlacil (2004).

VI. Evidence on the Draft Lottery and Incarcerations

\(^12\) We might also suppose that people born in certain parts of the year exhibit higher rates of criminality. Due to ineffective randomization, late (in the year) birthdays were especially likely to be called in the 1969 lottery. Angrist and Kruger (1991) show that individuals born late in the year are more likely than average to drop out of high school. Suppose that, rather than a cohort fixed effect \( \theta_c \), Equation (4) included a birthday fixed effect \( \beta_b \). Suppose that the fraction \( \phi_b \) of individuals born with a given birthday is constant over time. Suppose too that the relative effect of each birthday (i.e., \( \beta_b/\sum_{c} \phi_b \beta_b \)) is constant across cohorts. In this more general setting, the hypothesis tests in Table 1 are still unbiased tests of the null hypothesis that \( \pi_j = 0 \). However, Equation (8) no longer applies, and instead we have \( \lambda \pi_j/\theta_2 = (L_2 - L_1)/(1 - L_2) \).

\(^13\) It is straightforward to solve for \( \pi_j \) directly as well. To do this, multiply the formula in Equation (8) by \( E[\text{Crime}_i | \text{Called}_{j,b,i} = 0, c = 2] \). Let \( N_2 \) denote the population size of cohort 2. \( E[\text{Crime}_i | \text{Called}_{j,b,i} = 0, c = 2] \) is total crime for those with \( \text{Called}_{j,b,i} = 0 \) in cohort 2, divided by \( N_2*(1 - L_1) \).
Next, we will estimate the effects of draft eligibility on incarcerations for each of the 1969, 1970, and 1971 draft lotteries. Table 1 shows the fraction of inmates whose lottery numbers were called for different cohorts and lotteries. Panel A shows results from the ISCF, and Panel B shows results from the NCRP. Rows 1 and 2 show means for “Lottery Number Was Called” for the 1969 draft lottery. This lottery affected individuals born between 1944 and 1950. Most of the men who were drafted through this lottery came from the youngest (1950) cohort. Hence, this younger cohort is treated separately from the older cohorts. Row 3 shows means of “Lottery Number Was Called” for the 1970 draft lottery, which affected men born in 1951. Row 4 shows means for the 1971 draft lottery, which affected men born in 1952. Columns (1) and (4) show the fraction of inmates whose lottery numbers were called for cohorts not affected by that lottery. Among these inmates, “Lottery Number Called” simply represents a random collection of birthdays with no relevance to military service. In Row 1, the affected cohorts include men born from 1944 to 1949. In Row 2, the affected cohort includes men born in 1950. For Rows 3 and 4, the affected cohorts include men born in 1951 and 1952, respectively. For the 1969 lottery, “Cohorts Not Affected” includes men born from 1940-1943 and 1951-1960. For the 1970 and 1971 lotteries, “Cohorts Not Affected” includes men born from 1940-1960 not including 1951 or 1952, respectively. Columns (2) and (5) show the fraction of inmates whose lottery numbers were called for the cohorts that were affected by that lottery. Columns (3) and (6) of Table 1 show tests for whether the means are equal between the affected and unaffected groups.

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14 Combat-related fatalities represent one possible source of bias in estimating this mean. That is, draft eligible men may not appear in the sample because they died in Vietnam. However, this effect is likely to have very small (and probably negligible) effects on the estimates presented here. Of the 300,000 men who were drafted from 1970-1972, roughly 0.2% suffered combat-related fatalities. If draft eligibility increased the probability of military service by 0.10, then 0.02% fewer men should appear in the draft eligible sample. Sources described in footnote 3.
For most cohorts, we do not reject the null hypothesis of equality. Hence, in general, it
does not appear that draft eligibility affected criminal behavior. For the 1952 cohort, however,
we find a marginally significant positive effect of draft eligibility on being incarcerated in 1979.
We also find a significant effect of draft eligibility on later prison admissions for the 1950
cohort. While Table 1 illustrates the basic approach used in this paper, the actual numbers are
not easy to interpret. Next, we will use the information from Table 1 to calculate $\pi/\theta_2$, the
relative effect of draft eligibility on crime.

Table 2 shows estimates of $\pi/\theta_2$ for each of the affected cohorts. These estimates are
calculated by applying Equation (8) to the means presented in Table 1. Row 1 shows estimates
for the 1944-1949 cohorts. Row 2, 3, and 4 show estimates for the 1950, 1951, and 1952
cohorts, respectively. Row 5 shows weighted averages of $\pi/\theta_2$ across all cohorts, where the
weights are the numbers of draft eligible inmates in each cohort.\(^{15}\) Column (1) shows estimates
from the ISCF data, and Column (2) shows results from the NCRP data.

From Row 5, it appears that draft eligible men were 1.9% more likely than other men to
be in state prisons in 1979. Draft eligible men were also only 0.5% more likely than other men
to be admitted to prison from 1983 to 2001. Angrist (1990) estimates that being draft eligible
increased an individual’s probability of serving by roughly 10 percentage points.\(^{16}\) Dividing the
$\pi/\theta_2$ estimates in Row 5 by 0.1, we obtain effects of 20% for the ISCF data. For the NCRP data,

\(^{15}\) To estimate $\pi/\theta_2$ averaged across all cohorts with a standard error, the data are rearranged. Each observation
represents an inmate X lottery year combination. Hence, the number of observations equals 3 times the number of
inmates – one for each of the 1969, 1970, and 1971 lotteries. The dependent variable is a dummy for whether the
inmate’s birthday was among those called in that lottery year. The 3 regressors of interest are dummies for whether
that inmate’s cohort was affected by that lottery. Then $\pi/\theta_2$ is estimated for each lottery year using the formula in
Equation (8). The figures presented here are weighted by the number of inmates in the affected cohort in each year.
Robust standard errors calculated using the delta method, with observations clustered by inmate.

\(^{16}\) For the 1950-1952 cohorts, Angrist (1990) estimates effects ranging from 10 to 12 percentage points. However,
fewer men were drafted from the 1944-1949 cohorts. In the ISCF data, a regression of veteran status on draft
eligibility produces a coefficient of 0.11 for the 1950 cohort. For the 1944-1949 cohorts, this coefficient is 0.04.
Regressions not shown. Taking this difference into account, the first-stage effect rounds to roughly 0.1.
we obtain smaller effects of roughly 5%. Hence, in the short run, military service increased criminality by a factor of about 1.2. In the long run, military service only increased criminality by a factor of 1.05. However, some significant short-term and long-term effects are found for specific cohorts.

For the 1952 cohort, the short-term effect is fairly large. From Row 4 of Column (1), we have $\pi_j/\theta_2 = 0.212$. Applying Angrist’s first-stage estimates of 0.1, we find that draft eligibility increased individuals’ incarceration rates by a factor of roughly $1 + 2.1$. Hence, among men from this youngest cohort who served, military service appears to have tripled the probability of being in prison in 1979.

For the 1950 cohort, we find significant long-term effects of military service on incarcerations. However, these effects are considerably smaller than the short-term effects for the 1952 cohort. From Row 2 of Column (2), we estimate that $\pi_j/\theta_2 = 0.024$. Dividing by 0.1, we find that military service increased incarceration by a factor of 1.24.

A. Sensitivity Analysis

Table 3 examines the sensitivity of the results to changes in the sample population. The estimates shown here are $\pi_j/\theta_2$ averaged across cohorts, as in Row 5 of Table 2. As in Table 1, Panel A shows results for the 1979 ISCF, and Panel B shows results for the 1983-2001 NCRP. Column (1) shows regression results for the full sample. Column (2) excludes observations in which the inmate has previously spent time in jail or prison. Column (3) only considers inmates whose crimes were violent. Column (4) restricts the sample to inmates who are white.

One phenomenon that may cause bias in our estimates is prior offenses. Military service occupies young men’s time for two years and consequently may prevent them from committing crimes that would get them arrested. Consequently, when they are arrested and tried, veterans
are likely to be treated leniently because they do not have prior records. For this reason, we may underestimate veterans’ annual rates of criminality.\(^{17}\) In Column (2) of Table 3, the sample excludes inmates who have previously spent time in jail or prison. Dropping these extreme prior offenders from the sample should reduce the downward bias. As Panel A of Table 3 shows, dropping these observations from the 1979 ISCF data increases our estimate slightly. Given the long time span covered in the 1983-2001 data, the effects of two years of prior offenses are likely to be small. As Panel B of Table 3 shows, dropping repeat offenders from the NCRP data actually reduces our estimates. In both panels, our estimated effect is small and insignificant whether the repeat offenders appear in the data or not.

If military service makes men more aggressive, then the effects of draft eligibility on crime should be concentrated in violent crimes. In Column (3) of Table 3, the sample is restricted to violent crimes. For both datasets, restricting the sample to violent crimes does not affect our estimates of \(\pi_j/\theta_2\).

In Column (4), the sample is restricted to inmates who are white. Angrist (1990) finds that the effects of draft eligibility on military service were 2 to 3 times larger for whites than for nonwhites. In both cases, the effect of draft eligibility on incarcerations is 2 to 3 times larger for white men than for nonwhite men.\(^{18}\) Hence, our estimated effects of military service on incarcerations are roughly similar across whites and nonwhites.

### VII. Interpretation of Results

\(^{17}\) Thanks to Tom Newman for raising this point. Military training might also teach men skills that help them to commit crimes without getting caught. One additional source of bias is that veterans might receive sympathy from judges or juries simply because they are veterans. If these phenomena were widespread, then effects estimated in this paper may be downward biased. In addition, some veterans may have been acquitted from extremely violent crimes by reason of insanity (Baker and Alfonso, 2005). In general, however, verdicts of not guilty by reason of insanity constitute a negligible fraction of criminal cases (American Psychiatric Association, 2003).

\(^{18}\) The large effect for the 1952 cohort appears in the ISCF data for both races. Similarly, the smaller effect for the 1950 cohort appears in the NCRP for both races. Cohort-specific regression coefficients not shown.
The time-series results on sexual abuse appear inconsistent with the other estimates presented in this study. The timing of the rise and fall in sexual abuse is consistent with the story that military service causes men to commit violent acts. If this story were true, however, we would expect to see a positive relationship between the veteran rate and domestic abuse or incarcerations. We would also expect disproportionately many men with low lottery numbers to be incarcerated.

One possible explanation for the sexual abuse finding is that veterans are more honest than non-veterans in their responses. Military training and socialization and exposure to violence may have reduced the degree to which men regarded sexual assault as a severe crime. Consequently, they may have felt less ashamed than average to report the acts to interviewers. Some preliminary evidence suggests that veterans were no more honest than average in answering sensitive questions. Both the national and the Chicago surveys also include interviewers’ subjective assessment of the degree to which each respondent was honest. This variable is uncorrelated with veteran status and the cohort-specific veteran rate in both samples. Self-assessments of respondent honesty are also uncorrelated with veteran status in the Chicago data.

Another possible explanation for these conflicting findings is that soldiers forced sex on women while serving in Vietnam. While in Vietnam, soldiers may have found it easy to commit and get away with forced sexual acts. Soldiers spent much of their time isolated from women, and some may have developed abnormal levels of sexual frustration. Soldiers may have also felt less sympathy toward Vietnamese women than toward American women due to racial or national prejudice. Moreover, because they were in a foreign country, soldiers were relatively anonymous, which may have reduced the social stigma of committing sexual assault.
Finally, it is conceivable that the time-series results for sexual assault reflect a short-term effect of military service on crime. Because the sexuality survey measures whether respondents ever committed sexual assault, it should capture any short-term effects of military service. Using the draft lottery instrument, I find large short-term effects of military service on crime for the youngest cohort. However, the evidence for these short-term effects is not strong. If these effects exist, they are sufficiently short-lived that they cannot be detected in 1979 among men who completed service before 1974.\textsuperscript{19}

One major way in which military service affects crime is to take potential criminals off the streets for two years. Compared to this large negative effect, the long-term positive effects found in this study are fairly small. If the temporary effects of military service are large, they could outweigh the effect of taking young men off the streets. However, it is not possible with the data presented here to make this comparison in a conclusive way.

\textbf{VIII. Conclusion}

This study uses a well-known randomized natural experiment – the Vietnam draft lottery – to estimate whether military service makes men more violent. First, using time-series evidence, I examine the effects of military service on self-reports of sexual assault, domestic abuse, and incarcerations. I find that Vietnam-era cohorts are substantially more likely to report having commit sexual assault than men from surrounding cohorts. However, I find no effects for domestic abuse or incarcerations. Next, using the Vietnam draft lottery, I find that military service had small and insignificant effects on incarcerations. I find some evidence suggesting that large short-term effects may exist. However, I find that any short-term effects that do exist almost entirely disappear within 5 years of service. I find some small long-term effects of

\textsuperscript{19} Men who were affected by the 1971 draft lottery were drafted in 1972 and completed service 2 years later in 1974.
military service on incarcerations. However, these effects only appear for one of the four groups of cohorts studied. Adjusting for the draft’s effects on education does not affect these results.

Bibliography

http://www.psych.org/public_info/insanity.cfm


http://www.ncptsd.va.gov/facts/fs_legal.html


Notes: See notes to Table 2. Veteran rate and population size (denominator for Figure 1) by cohort taken from 1980 Census (5% Public Use Microdata Samples). Self-reported sexual abuse taken from National Health and Social Life Survey. Self-reported domestic abuse taken from Chicago Health and Social Life Survey. Domestic abuse and forced sexual acts are smoothed using 3-year moving averages. Additional details in the text.
Table 1: Fractions of Incarcerated Persons With Lottery Numbers Called

<table>
<thead>
<tr>
<th>Lottery Year</th>
<th>Fraction With Lottery Number Called</th>
<th>Cohorts Not Affected by That Lottery</th>
<th>Cohorts Affected by That Lottery</th>
<th>t-test for Equality</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. 1969</td>
<td></td>
<td>0.540</td>
<td>0.531</td>
<td>-0.61</td>
</tr>
<tr>
<td>(Older Cohorts)</td>
<td></td>
<td>(0.007)</td>
<td>(0.013)</td>
<td></td>
</tr>
<tr>
<td>N = 5,757</td>
<td></td>
<td>N = 1,608</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2. 1969</td>
<td></td>
<td>0.540</td>
<td>0.536</td>
<td>-0.14</td>
</tr>
<tr>
<td>(Younger Cohort)</td>
<td></td>
<td>(0.007)</td>
<td>(0.025)</td>
<td></td>
</tr>
<tr>
<td>N = 5,757</td>
<td></td>
<td>N = 399</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. 1970</td>
<td></td>
<td>0.342</td>
<td>0.346</td>
<td>0.16</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.006)</td>
<td>(0.023)</td>
<td></td>
</tr>
<tr>
<td>N = 7,332</td>
<td></td>
<td>N = 432</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. 1971</td>
<td></td>
<td>0.253</td>
<td>0.291</td>
<td>1.82*</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.005)</td>
<td>(0.020)</td>
<td></td>
</tr>
<tr>
<td>N = 7,250</td>
<td></td>
<td>N = 514</td>
<td></td>
<td></td>
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</tbody>
</table>


<table>
<thead>
<tr>
<th>Fraction With Lottery Number Called</th>
<th>Cohorts Not Affected by That Lottery</th>
<th>Cohorts Affected by That Lottery</th>
<th>t-test for Equality</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. 1969</td>
<td>0.539</td>
<td>0.540</td>
<td>1.17</td>
</tr>
<tr>
<td>(Older Cohorts)</td>
<td>(0.000)</td>
<td>(0.001)</td>
<td></td>
</tr>
<tr>
<td>N = 1,225,258</td>
<td>N = 236,155</td>
<td></td>
<td></td>
</tr>
<tr>
<td>2. 1969</td>
<td>0.539</td>
<td>0.545</td>
<td>2.85**</td>
</tr>
<tr>
<td>(Younger Cohort)</td>
<td>(0.000)</td>
<td>(0.002)</td>
<td></td>
</tr>
<tr>
<td>N = 1,225,258</td>
<td>N = 62,821</td>
<td></td>
<td></td>
</tr>
<tr>
<td>3. 1970</td>
<td>0.342</td>
<td>0.341</td>
<td>-0.24</td>
</tr>
<tr>
<td>(0.000)</td>
<td>(0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N = 1,456,213</td>
<td>N = 68,021</td>
<td></td>
<td></td>
</tr>
<tr>
<td>4. 1971</td>
<td>0.259</td>
<td>0.258</td>
<td>-0.75</td>
</tr>
<tr>
<td>(0.000)</td>
<td>(0.002)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>N = 1,446,453</td>
<td>N = 77,781</td>
<td></td>
<td></td>
</tr>
</tbody>
</table>

Notes: Statistics for Panel A weighted using sample weights. Sample for Panel B is restricted to prison admissions resulting from new court commitments. Statistics for Panel B weighted by total annual prison admissions divided by prison admissions in sample. Robust standard errors in parentheses. For "Cohorts Not Affected," the lottery numbers apply to the same birthdays but different birth years. For the 1969 lottery, the affected cohorts include men born from 1944-1949 (the older cohorts) and 1950 (the younger cohort). For the 1970 and 1971 lotteries, the affected cohorts include men born in 1951 and 1952, respectively. For the 1969 lottery, "Cohorts Not Affected" includes men 1940-1943 and 1951-1960. For the 1970 and 1971 lotteries, "Cohorts Not Affected" include men born from 1940-1960 not including 1951 or 1952, respectively. Additional details in the text.
Table 2: Relative Effect of Draft Eligibility on Incarceration Rates by Cohort, ISCF and NCRP Data

<table>
<thead>
<tr>
<th>Cohort</th>
<th>( \pi_j/\theta_2 ) ISCF</th>
<th>( \pi_j/\theta_2 ) NCRP</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. 1944-1949</td>
<td>-0.034</td>
<td>0.005</td>
</tr>
<tr>
<td>2. 1950</td>
<td>-0.014</td>
<td>0.023</td>
</tr>
<tr>
<td>3. 1951</td>
<td>0.017</td>
<td>-0.002</td>
</tr>
<tr>
<td>4. 1952</td>
<td>0.210</td>
<td>-0.006</td>
</tr>
<tr>
<td>5. All Affected Cohorts</td>
<td>0.019</td>
<td>0.005</td>
</tr>
</tbody>
</table>

Notes: See notes to Table 1. The formula for \( \pi_j/\theta_2 \) appears in Equation (8) and is calculated as:

\[
\pi_j/\theta_2 = \frac{(L_2 - L_1)/(1 - L_2)L_1}{},
\]

where \( L_1 = E[Called_{j,b,i} | Crime_i > 0, c = 1] \), and \( L_2 = E[Called_{j,b,i} | Crime_i > 0, c = 2] \). In each case, cohort 2 includes the "affected" cohorts and cohort 1 includes the "unaffected" cohorts. Standard errors calculated using the delta method. "All Affected Cohorts" is a weighted average of the estimated effects across the 3 lottery years. Calculations for this weighted average described in the footnotes to Table 3. Additional details in the text.
Table 3: Sensitivity Analysis: Estimated Values for $\pi_j/\theta_2$ for All Affected Cohorts Across Different Specifications

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Including All Observations</td>
<td>Excluding Those With Prior Jail Time</td>
<td>Including Only Violent Crimes</td>
<td>Including Only Whites</td>
</tr>
<tr>
<td>$\pi_j/\theta_2$</td>
<td>0.0185</td>
<td>0.0232</td>
<td>0.0196</td>
<td>0.0266</td>
</tr>
<tr>
<td></td>
<td>(0.0439)</td>
<td>(0.0706)</td>
<td>(0.0570)</td>
<td>(0.0633)</td>
</tr>
<tr>
<td>Obs (Inmates X Lottery Years)</td>
<td>23,292</td>
<td>10,203</td>
<td>13,353</td>
<td>11,685</td>
</tr>
<tr>
<td>Clusters (Inmates)</td>
<td>7,764</td>
<td>3,401</td>
<td>4,451</td>
<td>3,895</td>
</tr>
</tbody>
</table>

Panel A: Survey of Inmates at State Correctional Facilities

<table>
<thead>
<tr>
<th></th>
<th>(1)</th>
<th>(2)</th>
<th>(3)</th>
<th>(4)</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\pi_j/\theta_2$</td>
<td>0.0047</td>
<td>-0.0037</td>
<td>0.0035</td>
<td>0.0085</td>
</tr>
<tr>
<td></td>
<td>(0.0034)</td>
<td>(0.0067)</td>
<td>(0.0064)</td>
<td>(0.0048)*</td>
</tr>
<tr>
<td>Obs (Inmates X Lottery Years)</td>
<td>4,573,131</td>
<td>1,122,522</td>
<td>1,261,821</td>
<td>2,221,134</td>
</tr>
<tr>
<td>Clusters (Inmates)</td>
<td>1,524,377</td>
<td>374,174</td>
<td>420,607</td>
<td>740,378</td>
</tr>
</tbody>
</table>


Notes: See notes to Tables 1 and 2. Estimates of $\pi_j/\theta_2$ in Column (1) correspond to the "All Affected Cohorts" estimates shown in Row 5 of Table 2. Additional columns show the same calculation for subsets of the data. To estimate $\pi_j/\theta_2$ averaged across all cohorts with a standard error, the data are rearranged. Each observation represents an inmate X lottery year combination. Hence, the number of observations equals 3 times the number of inmates – one for each of the 1969, 1970, and 1971 lotteries. The dependent variable is a dummy for whether the inmate’s birthday was among those called in that lottery year. The 3 regressors of interest are dummies for whether that inmate’s cohort was affected by that lottery. Each regression includes dummies for lottery year (1969, 1970, and 1971). Both samples include cohorts born between 1940 and 1960. Estimates of $\pi_j/\theta_2$ are calculated for each lottery year using the formula in Equation (8). The figures presented here are weighted by the number of inmates in the affected cohort in each year. Robust standard errors calculated using the delta method, with observations clustered by inmate. Additional details in the text.