Drawn into Violence:
Evidence on 'What Makes a Criminal' from the Vietnam Draft Lotteries[*]

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Abstract

Draft lottery number assignment during the Vietnam Era provides a natural experiment to examine the effects of military service on crime. Using exact dates of birth for inmates in state and federal prisons in 1979, 1986, and 1991, we find robust evidence of effects on violent crimes among whites. In particular, we find that draft eligibility increases incarceration rates for violent crimes by 14 to 19 percent. Based on Angrist and Chen's (2008) estimate of the effect of draft eligibility on veteran status, these estimates imply that military service increases the probability of incarceration for a violent crime by 0.27 percentage points. Results for nonwhites are not robust. We conduct two falsification tests, one that applies each of the three binding lotteries to unaffected cohorts and another that considers the effects of lotteries that were not used to draft servicemen.

JEL Classification: H56, K42, N42 *Key Words:* veterans, crime, violence, Vietnam War, two-sample IV

1 Introduction

"CRIMINALS ARE MADE, NOT BORN." —Stenciled sign left behind by Michigan school board member and suicidal mass murderer Andrew Kehoe after killing 45 people, mostly school children.

Understanding the extent to which criminals are "made" and, further, identifying the determinants of criminal behavior is of utmost importance to any society that wants to reduce crime. To date, most research in this area has focused on the effects of individuals' immediate environments. For example, researchers have considered the causal effects of punishments for infractions (Levitt 1998), policing (Levitt 1997; McCrary 2002; Levitt 2002; Lee and McCrary 2009), the ability to drink legally (Carpenter 2007; Carpenter and Dobkin 2008), neighborhoods (Ludwig, Duncan, and Hirschfield 2001; Kling, Ludwig, and Katz 2004), gun shows (Duggan, Hamjalmarrson, and Jacob 2007), sports losses (Rees and Schnepel 2009; Card and Dahl 2009), and incapacitation (Jacob and Lefgren 2003; Dahl and DellaVigna 2009). Studies that explore how individuals' backgrounds affect criminal behavior are rarer with Lochner and Moretti's (2004) research on the effect of education providing a notable exception.[1] In this paper, we add to this literature by exploiting the randomness of the national Vietnam draft lotteries to examine the effects of military service on subsequent incarceration.

Our study also speaks to the costs of military engagements. Any discussion about the benefits of military engagements must include a discussion of the true social costs of military service. It is widely acknowledged that, in addition to direct costs incurred during conflict, costs continue to accrue after a conflict as military service can lead to post-traumatic stress disorder and other long-term health problems. This paper, which analyzes impacts on crime, can be thought of as exploring another such long-term cost.

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[1] Though subsequent re-analysis by Black, Devereux, and Salvanes (2008) has questioned the strength of Lochner and Moretti's first stage.

While the discipline involved with military training could make veterans less likely to commit crimes, there are several reasons that military service might increase criminality. Perhaps most importantly, the military trains soldiers to be "ready to react within a split-second of any provocative activity and [to shut down] emotions" (Slone and Friedman 2008) in addition to desensitizing them to violence and familiarizing them with weapons. These types of behaviors, which are crucial to survival in a combat zone, can lead to problems after a soldier returns to civilian life.[2] In addition to the training, events in the combat zone can have lasting psychological effects in the form of post traumatic stress disorder. Service in the Vietnam War also might increase crime because it precluded labor market experience thus reducing wages (Angrist 1990; Abadie (2002); Angrist and Chen 2008) or because it was associated with high rates of opiate use (Robins, Davis, and Goodwin 1974).

A sizable literature links military service to criminal behavior, particularly to violent behavior, but much of the prior work lacks plausibly exogenous variation and focuses on small non-random samples.[3] Exogenous variation in military service is crucial since men who are more likely to engage in criminal activities may be the most likely to enlist. A few recent papers have made serious attempts to address this type of non-random selection. Galiani, Rossi and Schargrodsky (2009) use variation driven by Argentina's draft lottery, finding that conscription during the Falklands War increased crime rates. Looking in the U.S. context, Rohlfs (2010) examines one of the several possible mechanisms through which

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[2]These types of behaviors also led to problems amongst fellow servicemen. In *Another Brother*, Greg Payton describes one such conflict:

> We had been brought to Vietnam for violence, for violent purposes, so it wasn't unusual for us to be violent amongst ourselves you know. I remember the first time I got shot at it was Christmas Eve and an African American GI had a fight with a white GI. The white GI went back to his hooch and he got his weapon. We heard a weapon being loaded. Instinctively we hit the ground and he opened up automatic fire. It was just by split seconds that we weren't all killed.

[3]For example, Yager (1976) shows that veterans are more violent, Yesavage (1984) and Resnick et al. (1989) show that combat veterans are more violent, and Yager, Laufer, and Gallops (1984) show that combat exposure is correlated with criminal convictions. Bouffard (2003) is an exception in this literature, finding no evidence of an impact on violent crime after controlling for criminal behavior prior to Vietnam.

military service might affect crime. He uses cohort-level variation in combat deaths during the Vietnam War as an instrument for combat experience, finding that combat exposure increases self-reported violent acts among blacks (and has an imprecise effect among whites). Rohlfs (2006) is the only prior study using plausibly exogenous variation to consider the effects of military service on crime in the U.S. He compares the fraction of Vietnam-era draft eligible inmates in prison to the fraction expected based on cohorts not subjected to the drafts, and finds weak evidence of effects on crime.[4]

In this paper, we also focus on variation provided by the Vietnam draft lotteries. In particular, our identifying variation is driven by: (1) the Vietnam Era draft lotteries which randomly assigned lottery numbers to exact dates of birth and (2) the fact that the military drafted men with the lowest lottery numbers for potential induction until manpower requirements were met each year. As such, we are able to determine the extent to which military service affects criminal behavior by comparing the incarceration rates (based on the number births) for those whose lottery numbers were called to report for induction into the military to the incarceration rates those whose numbers were not called. We do this by combining data from the 1979, 1986, and 1991 Surveys of Inmates in State and Federal Correctional Facilities (SISFC) with data from the Vital Statistics of the United States to create measures of incarceration rates for each day of birth for the cohorts affected by the draft lotteries.

While random assignment allows us to cleanly identify the effects of draft eligibility, our data do not allow us to directly estimate the effect of *military service* on crime since we

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[4]Our study offers several advantages over this pioneering study. First, instead of using a cross-cohort differences and differences framework we focus solely on within cohort variation provided by the draft lotteries. Thus, we are able to use non-affected cohorts as a robustness check to verify that our results are not driven by the particular sets of birthdays selected in the drafts. Second, our outcome variable lends itself to a natural interpretation. Specifically, it provides a direct estimate of the effect of draft eligibility on the probability of incarceration in the survey years. Third, we explore longer-run effects and enhance the precision of our estimates by expanding the sample to include inmates incarcerated in 1986 and 1991 in addition to those incarcerated in 1979. Finally, we present a comprehensive exploration of the effects of draft eligibility on crime by separately considering its effects on violent crime, drug-related crime, and property-related crime, and public order crime.

only have data on the population of inmates. In particular, these data do not allow us to estimate the first-stage effect of draft eligibility on military service. As such, we use Angrist and Chen's (2008) first-stage estimates to obtain two sample instrumental variables estimates of the effect of military service on incarceration. A caveat to this exercise is that draft eligibility may affect crime through mechanisms besides its impact on military service. For example, Angrist and Krueger (1992) and Card and Lemieux (2001) find positive effects of eligibility on education. This phenomenon, however, will reduce the likelihood that we find positive effects on crime. At least two other possibilities make it less likely that we are able to identify an effect on crime. First, veterans may be less likely to be observed in prison due increased mortality rates. Second, veterans may be granted leniency by judges, juries, and/or law enforcement officers.[5] While "negative" draft avoidance behaviors may bias us towards finding effects on crime, we present evidence based on the non-binding 1953–1956 lotteries that suggests this is unlikely.

We find weak evidence of impacts on overall incarceration rates among whites but find robust evidence of impacts on incarceration rates for violent crimes. In particular, we find that draft eligibility increases incarceration rates for violent crimes by 14 to 19 percent across the three survey years. Based on Angrist and Chen's (2008) estimate of the effect of eligibility on veteran status, these estimates imply that being a veteran increases the probability of being incarcerated for a violent crime by 0.27 percentage points for whites. We find similar results when separately considering the effects by birth cohort and by survey year. The results for nonwhites are less robust and relatively imprecise, with standard error estimates usually seven times greater than similar estimates for whites.

The rest of the paper is organized as follows. Section 2 provides background on the Vietnam Era draft lotteries. Sections 3 and 4 describe our data and the empirical strategy.

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[5]We return to this issue in the conclusion where we discuss special courts that have been set up to try offenders who are veterans.

Section 5 discusses the tradeoffs involved in choosing the analysis sample. Section 6 presents our results and our robustness checks. Finally, Section 7 discusses our results and concludes.

2   Background on the Draft Lotteries

To fairly allocate military service in Vietnam, a total of seven lottery drawings were held to determine who would serve, although conscription was halted after the third lottery. The three lotteries used to draft servicemen were held in 1969, 1970, and 1971. While the 1969 lottery applied to those born 1944–1950, each subsequent drawing applied only to men who turned 18 in the year of the lottery. In particular, the 1970 lottery applied to those born in 1951 and the 1971 lottery applied to those born in 1952.

In each drawing, the birthdays of the year were randomly assigned a Random Sequence Number (RSN). In the 1969 drawing September 1st was assigned RSN 1 so men born on September 1st were asked to report to their local draft boards for potential induction before men born on other days. April 24th was assigned RSN 2 so men born on that day were asked to report second, and so forth. The military continued to call men for potential induction in order of RSN until the manpower requirements were met for that year. The last RSN called for service, also known as the highest Administrative Processing Number (APN), was 195 for the 1969 drawing, 125 for the 1970 drawing, and 95 for the 1971 drawing. Throughout the paper, we refer to indivduals with RSNs less than the APN as "draft eligible."

While the issue was addressed for later drawings, there was an important mechanical problem with the randomization mechanism used in the 1969 drawing. In particular, each birthday was coded onto a capsule and these capsules were added month by month into a drawer, with the drawer being "shuffled after each month. As a result of incomplete mixing, dates later in the year remained on top of the pile and were more likely to be drawn first and called first for induction (Fienberg 1971). This phenomenon is shown in Figure A1 in the

appendix, which plots the number of draft eligible days by month for each lottery. To the extent that people born in later months might be more or less likely to commit crimes this could lead to omitted variable bias. We follow the previous literature (Angrist and Chen 2008, Conley and Heerwing 2009, Eisenberg and Rowe 2009, Angrist, Chen, and Frandsen 2010) to address this potential issue by controlling for year by month of birth fixed effects in our analysis.[6]

As mentioned, military service is not perfectly predicted by being born on a draft-eligible day. Men born on non-eligible birthdays could volunteer and men born on drafted days could fail the medical exams, refuse to report, or apply for various exemptions. Despite these issues, the draft had a significant effects on military service, the magnitude of which is discussed in Section 6.1.

3   Data Description and Construction

Our data on incarceration come from the 1979, 1986, and 1991 Surveys of Inmates in State and Federal Correctional Facilities (SISFC), which are representative of the prison population in state and federal correctional facilities. These data contain information on the prisoner's exact date of birth, race, sex, and the type of offense for which he was incarcerated. The type of offense is classified according to approximately 80 offense codes and each inmate is associated with up to four different offense codes (since inmates can concurrently serve time for multiple offenses).

We conduct the analysis separately for whites and nonwhites at the date of birth by survey year level. Each observation represents a collapsed cell measuring the incarceration rate (per 10,000 births) in survey year s for individuals born on day d. To construct this variable, we divide the number of male convicts we observe in prison in survey year s with

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[6]Information on the details of the Vietnam Draft lottery can be found at the Selective Service Website http://www.sss.gov/lotter1.htm and in Flynn (1993) and Baskir and Strauss (1978).

date of birth d by the number of males that were born in the United States on day d:

$$IncarcerationRate_{sd} = 10,000 * \frac{\#ofCriminals_{sd}}{\#ofBirths_d}. \qquad (1)$$

The denominator for equation above comes from the Vital Statistics of the United States (VSUS). Since the VSUS only reports births by month prior to 1969, we must construct the number of births for each given day. We report results in which the number of births in each month are apportioned evenly across the days in the month. The results are nearly identical using strategies for constructing the denominator that adjust for differing birth patterns observed on weekdays versus weekends. These robustness checks are described further in the appendix.

To properly link each birthday with a particular draft lottery number we use the draft lottery information available from the Selective Service System. This allows us to associate each birth date with a lottery number for each of the three binding lotteries.

4   Empirical Strategy

If military service were purely random, we could use our data to analyze the effect of military service on incarceration rates by estimating

$$IncarcerationRate_{sd} = \alpha * FractionVeterans_d + u_{sd}. \qquad (2)$$

where $IncarcerationRate_{sd}$ is the incarceration rate for individuals born on day d in survey year s and $FractionVeterans_d$ is the fraction of veterans among those who were born on day d. Because selection into the military is not random, this approach would likely lead to biased estimates of α. Perhaps of greatest concern is the possibility that aggressive individuals are more likely to serve in the military and also to commit crimes, in which case the effect of

being a veteran, α, would be would be biased upwards. Alternatively, if individuals with more respect for authority are more likely to become veterans and less likely to commit crimes then α will be biased downwards. We overcome these obstacles by utilizing variation driven by the Vietnam draft lotteries. Given the random assignment of *draft eligibility* by date of birth, we instead estimate

$$IncarcerationRate_{sd} = \gamma * DraftEligible_d + \epsilon_{sd} \tag{3}$$

where γ estimates the average reduced form effect of draft eligibility on incarceration rates. Due to the mechanical issues associated with the draft lotteries described above and because the data span multiple survey years, we also include year by month of birth fixed effect and survey year fixed effects.[7]

To recover the estimated effect of *military service* on incarceration rates, we need to know the effect of draft eligibility on military service which can be estimated by

$$FractionVeterans_d = \beta * DraftEligible_d + \omega_d, \tag{4}$$

where $DraftEligible_d$ is an indicator variable that equals one if men born on date d are assigned a lottery number that makes them eligible to be drafted into the military and zero otherwise. However, our data is based on a population of inmates and not from the entire population subjected to the draft lotteries so we are unable to estimate equation 4. Instead, we use Angrist and Chen's (2008) estimates of β to calculate two-sample instrumental variable estimates of the effect of military service on incarceration rates. We obtain the TSIV

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[7]While it might appear desirable to control for other covariates, Angrist (1989) suggests that this is not necessary since there is no correlation between draft lottery status and characteristics besides subsequent veteran status.

estimate by taking the ratio of the reduced-form estimate and the first-stage estimate,

$$\hat{\alpha}_{TSIV} = \frac{\hat{\gamma}}{\hat{\beta}}, \tag{5}$$

and estimate the standard error using the delta method.[8]

While random assignment ensures that $\hat{\gamma}$ will be unbiased, the instrumental variables estimation strategy relies on the assumption that veteran status is the only mechanism of transmission between draft eligibility and incarceration rates. We acknowledge that $\hat{\alpha}$ will be biased if draft eligibility also affects incarceration rates through any other mechanisms. It has been documented that men with low lottery numbers enrolled in college or prolonged their time in college to avoid military service (Angrist and Krueger 1992). To the extent that increased education levels lead to decreased crime (Lochner and Moretti 2004) the extra education conferred by holding a low draft lottery number should bias our estimates of α downward. Another important consideration is that military service might make men more likely to die either in combat or afterward.[9] This potential bias also works against us finding a positive effect. We should also note that this instrumental variable approach identifies the local average treatment effect (LATE), or the effect of military service on those individuals who can be compelled to enter the military by the draft lotteries.

5   Tradeoffs In the Choice of Analysis Sample

The 1979, 1986, and 1991 waves of the SISFCF used in this analysis contain information on 6642, 6612, and 6631 male inmates subjected to the drafts, respectively. In selecting

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[8]In particular, we assume $cov(\hat{\gamma}, \hat{\beta}) = 0$ which yields $var(\hat{\alpha}_{TSIV}) = \frac{var(\hat{\gamma})}{\hat{\beta}^2} + \frac{\hat{\gamma}^2 * var(\hat{\beta})}{\hat{\beta}^4}$. Bootstrapping produces nearly identical standard error estimates.

[9]As mentioned previously, the literature has not firmly established whether higher mortality effects for draft eligible men exist. Efforts to identify health effects among survivors using variation from the draft lotteries have been also been inconclusive (Dobkin and Shabani 2009).

an appropriate sample to analyze, there is a tradeoff between ease of interpretation of the results and sample size. The easiest results to interpret are those where data are limited to a single survey wave. For example, if we limit the sample to cells collapsed from the 1979 data, our estimates will provide us with the estimated effect of military service on the probability of being incarcerated seven to nine years after conscription. The interpretation is more complicated when we expand the sample to include all three survey waves, where we are estimating a combination of the probabilities of being observed in jail 7–9, 14–16, and 19–21 years later. On the other hand, limiting results to a single survey usually leads to large standard errors since the estimates are based on a smaller sample of inmates.

Combining observations from all three surveys maximizes the coverage over lottery numbers. The benefit of this approach is perhaps most evident when one considers that 65% of the 1097 lottery numbers have nonzero observations when using data from all three waves but only about 38% are nonzero when the lottery number cells are constructed from a single survey year.[10] Our preferred specification uses all survey waves together to maximize precision but we also estimate the effects separately using data from each wave.

A similar tradeoff involves which birth cohorts to use in the analysis. For men born 1944–1949, the 1969 drawing was not the first time they could have been drafted. Prior drafts were conducted by local draft boards in which each board had some discretion about who was drafted first or qualified for deferments (Baskir and Strauss 1978).[11] Further, many men had volunteered before the draft was held. As a result, the 1969 draft had a smaller impact on military service for men born 1944–1947 relative to those born 1948–1949 for whom the effect was, in turn, smaller than the effects on those born after 1950 (Angrist and Chen 2008). For this reason, our data may have insufficient power to identify effect on crimes for

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[10]For violent nonwhite offenders, the percentages are 74% and 46%, respectively. We should also note that the first and third drawings included a 366th number to allow for leap day. In our estimates we discard the leap day from the 1969 drawing since it is not as precisely estimated as the other 365 days in that drawing.

[11]There were approximately 4,000 local draft boards.

the earliest cohorts.[12] On the other hand, restricting the sample to cohorts that faced the draft for the first time in 1979 and later severely reduces the sample size. Because of these considerations we separately present results pooling all of the cohorts, in addition to showing estimates for the 1948–1952 and 1950–1952 cohorts who are more-commonly focused on in the literature.

6   Results

This section is organized into multiple parts. We begin by presenting the first stage effect of draft eligibility on military service and then show summary statistics for incarceration rates. We then estimate effects of draft eligibility and military service on incarceration. Finally, we perform a robustness check that verifies that our main results are not driven by the particular birthdays that were drawn in any given lottery or by avoidance behaviors among eligible men.

6.1   First Stage Effect of Eligibility on Military Service

As we described above, the relevant estimate of the effect of the Vietnam draft lotteries on military service must be based on the entire set of individuals exposed to the draft. Since our data is limited to the prison population, we use first stage estimates from Angrist and Chen (2008). Their first stage estimates, from restricted-use U.S. Census data, are presented in Table 1.[13]

On average, draft eligibility increased the probability of military service by approximately 11 percentage points for whites born 1948–1952. The effect is 2–4 percentage points higher for

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[12]Another potential drawback of including pre-1950 cohorts in the analysis is that the estimated effects will be partially identified by men who had successfully avoided previous drafts. To the extent that these individuals may differ from the general population, this may limit the external validity of these estimates.

[13]Angrist and Chen (2008) also explore a specification in which the effects are interacted with groups of lottery numbers. They find that these additional instruments do not increase precision. For this reason, we focus on the single instrument case which simplifies statistical inference for the TSIV estimates.

the latter three cohorts who were only eighteen when they were subjected to their respective drafts. The first stage is approximately 50 percent greater for whites than nonwhites. For this reason, we follow Angrist, Chen and Frandsen (2010) and henceforth focus on results for whites until Section 6.5.

6.2 Summary Statistics

Table 2 presents mean incarceration rates by eligibility status, pooling observations from all drafted cohorts and all survey years. The table separately considers incarceration rates for all crimes, violent crimes, drug-related crimes, property crimes, and public order crimes.[14] These categories are mutually exclusive, but since an inmate can be concurrently serving time for multiple offenses, he may appear on multiple lines in the table.

In most cases, the means in Table 2 suggest that induction had only a small effect, if any, on criminality for whites. On the other hand, the means suggest that induction increased incarceration for violent crimes by approximately 13 percent. Figure 1 shows the mean incarceration rates for three different groups of cohorts. First, it shows incarceration rates for all of the cohorts subjected to the binding drafts, those born 1944–1952. It then shows the incarceration rates for the 1948–1952 cohorts, for whom we know the impact of the draft on veteran status was greater. Finally, the figure shows incarceration rates for the 1950–1952 cohorts—all of the men in these cohorts were 18 at the time of the draft lottery and the lottery had the greatest impact on their probability of service. In Panel A, there is only evidence of an impact on overall incarceration rates only for the 1950–1952 cohorts. Panel B, focusing on incarceration rates solely for violent crimes, tells a different story. Here we see that draft eligibility is associated with higher incarceration rates for violent crimes for each

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[14]We follow the National Prisoner Statistics offense code categorization. Violent crimes include any attempt at murder, manslaughter, kidnapping, rape, robbery, assault, or extortion. Drug-related crimes include traffic in or possession of drugs. Property crimes include robbery, extortion, burglary, auto theft, fraud, larceny, embezzlement, any stolen property crime, and drug trafficking. Finally, public order crimes are more varied but primarily consist of weapons violations and serious traffic offenses.

of the three groups of cohorts. Again, the difference is most prominent for the 1950-1952 cohorts for whom the lottery had the greatest impact on military service. The next section will estimate these effects in a regression framework that allows us to conduct statistical inference.

6.3 Regression Based Estimates For Whites Using All Available Data

Table 3 reports the estimated reduced-form effects of draft eligibility on overall incarceration rates in 1979, 1986, and 1991 among whites. The data are aggregated at the exact date of birth by survey year level. The estimates control for month by year of birth fixed effects to deal with the fact later birth months had a higher probability of being drawn in the 1969 draft due to mechanical problems. The estimates also control for survey year fixed effects.

Echoing the results presented in the previous section, we do not find evidence that eligibility increased overall incarceration rates for whites who were older than 18 when their draft lottery was held. The point estimate for the 1950–1952 cohorts, however, indicates a 7.7 percent increase in incarceration rates although this effect is not statistically significant at conventional levels.

Below the estimated percent impact, Table 3 shows estimated effects of military service on the probability of incarceration. These two-sample instrumental variables estimates use the first stage estimates shown in Table 1.[15] As we discussed above, while these calculations rely on the assumption that draft eligibility only affects criminality through its impact on veteran status, the most likely violations work against us finding an effect. Although the point estimate suggests that military service increased the probability of incarceration by 0.32 percentage points for whites born 1950–1952, we cannot reject zero at conventional confidence levels.

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[15]Note that we can only calculate this estimate for the cohorts analyzed by Angrist and Chen (2008).

Table 4 presents similar results but focuses on incarceration rates only for violent crimes. These results indicate that eligibility led to significant increases in incarceration rates for violent crimes for whites. The point estimates indicate that draft eligibility increased incarceration rates by 2.6 per 10,000, corresponding to an increase of 13.5 percent. We again find more dramatic effects for the later cohorts. For these cohorts, our point estimates indicate that draft eligibility increased rates of incarceration for violent crimes by 19.1 percent. Correspondingly, these results suggest that military service increases the probability of incarceration for a violent crime by 0.28 percentage points.

Table 5 explores effects on incarceration for drug-related crimes, property crimes, and public order crimes. The results are never statistically significant and have no consistent sign, which sheds light on why we tend to not find effects of draft eligibility on overall incarceration rates.[16] It is also important to note that, although these results are never statistically significant, we cannot rule out relatively large effects. Further, we can reject that the estimated effects on overall crime, violent crime, drug crime, property crime, and public order crime are jointly zero (p-value = 0.0141).

6.4 Results For Whites Stratified by Cohort and Survey Wave

The preceding section presented our preferred estimates since they maximized efficiency by pooling data from all cohorts subjected to the drafts and data from all survey waves. However, we acknowledge that doing so comes at the cost of interpretability and the risk of masking heterogeneity. This section presents results that are estimated separately for each of the cohorts subjected to the draft when they were eighteen years old and for each survey year for which we have data on inmates. While we focus on incarceration rates for violent crimes in the discussion below, estimated effects for overall incarceration rates for whites are

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[16]Hearst et al. (1991) also uses variation from the Vietnam draft lotteries and finds no evidence that military service leads to increased drug use.

shown in the appendix.

Columns 1 through 5 of Table 6 show the estimated effects on incarceration rates for violent crimes by draft cohort for whites. Although the standard errors are never small enough to reject zero when any single cohort is considered, the point estimates are fairly stable across different cohorts, indicating that military service increases the probability of incarceration for a violent crime by 0.15 to 0.39 percentage points.

Columns 6 through 8 of Table 6 separately consider the effects of draft eligibility using inmates in prison in 1979, 1986, and 1991. The men conscripted by the lotteries would have finished their mandatory service five to seven years before the 1979 survey was conducted. Again, the estimates are fairly stable. The estimated effect in 1979 is statistically significant at the five percent level, indicating that military service increases the probability of incarceration for a violent crime by 0.34 percentage points.[17] The estimates are similar for 1986 and 1991 although the standard error estimates are larger.

6.5 Estimated Effects for Nonwhites

Table 7 summarizes the estimated effects of draft eligibility and military service on incarceration among nonwhites. As we alluded to earlier, the first stage is much smaller for nonwhites.[18] In addition, the reduced-form and two-sample IV estimates have standard error estimates approximately seven times larger than those for whites and the point estimates are less robust.[19]

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[17]The estimated effects on overall incarceration in 1979, shown in the appendix, are not statistically distinguishable from correlational evidence based on the 1980 Census which suggests a small but significant negative effect of service in Vietnam on being observed in a correctional facility.

[18]Angrist (1991) sheds light on this issue. Whites were relatively likely to avoid military service until drafted which produces a relatively-strong relationship between draft status and eventual military service. However, because the military was a more attractive option for blacks, they were more likely to have already signed up for the military by the time the draft lottery was implemented, yielding a relatively-weak relationship between draft status and veteran service.

[19]While we could pool whites and nonwhites together, there are several possible reasons to expect heterogeneity across race. Whites were at lower risk of dying in combat throughout until late in the war (Rohlfs 2010), whites enrolled in college at higher rates to avoid conscription (Rohlfs 2006, Kuziemko 2008) and

While the estimate based on all of the data provides suggestive evidence of an effect on violent crime (significant at the ten-percent level), estimates based on individual cohorts and individual survey years highlight a lack of robustness. The estimated effects vary substantially across both cohorts and survey years and are often negative or close to zero. The biggest outlier is the estimated effect on incarceration in 1991 which suggests that effects may have manifested for nonwhites in the late 1980s. While this estimate may very well be a statistical anomaly, it might be related to increases in poverty among veterans during the 1980s or the crack epidemic and its accompanying rise in the incarceration rates of black males (Chaiken 2000).[20] Further research utilizing alternative data sets and/or empirical strategies will be necessary to further pin down this issue. Towards this end, we have also investigated data from the National Corrections Reporting Program (NCRP) which collects data on the universe of individuals placed into prisons to try to corroborate these findings. While the point estimates suggest that draft eligibility is associated with increased incarceration for violent crimes among nonwhites from 1986-1991, they are never statistically significant.[21]

6.6   Robustness Checks Using Lotteries for Unaffected Cohorts

One possible concern with our main estimation strategy is that, despite being random, the first numbers drawn (which led to eligibility) may have included a disproportionate number

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whites had higher take-up rates of GI Bill benefits after completion of service (Turner and Bound 2003).

[20]Data from the Current Population Survey reveals that extreme poverty rates rose steadily for Vietnam veterans during the 1980s while it fell for non-veterans from the same cohorts. These results are available upon request. While we have examined effects of military service on drug-related crimes for nonwhites in 1991 and found small negative effects, the standard errors are such that the 95 percent confidence interval includes effects as large 21 percent. In addition, statistically insignificant point estimates suggest that draft eligibility is associated with 153 percent (2.3 per 10,000) more nonwhite incarcerations for crimes involving both violence and drugs in 1991 versus -17.8 percent and 14.8 percent for 1979 and 1986, respectively. These results are also available upon request. Also consistent with our results, Fryer et al. (2006) study this period and conclude that "the greatest social costs of crack have been associated with prohibition-related violence, rather than drug use *per se*."

[21]Note that this data is less useful for corroborating our results for whites because the NCRP does not begin tracking admissions until 1983, whereas the effects for whites manifest as early as 1979.

of birth dates that we would expect to be associated with higher rates of crime even if no one was called to serve in the military. For example, this could occur if men born on dates with the earliest lottery numbers tended to come from disadvantaged backgrounds.

To verify that this type of phenomenon is not driving our results, we apply each of the three lotteries to adjacent cohorts that the given lottery did not affect and conduct the analysis as before.[22] For example, we test the 1969 draft that applied to the 1944–1950 cohorts by matching the 1969 lottery numbers to the birth dates in the 1943 and 1951 cohorts and testing for effects. Since the 1969 lottery did not actually apply to these cohorts, we should not find significant effects unless the 1969 lottery suffered from the potential problem described above. We test each lottery using the two adjacent cohorts for whom the robustness check is most likely valid. To improve precision, we also estimate the effects using all of the unaffected cohorts that our data sets allow us to cover, ranging from 1942–1959.[23]

These results (focusing on violent crimes) are presented in Table 8. The odd-numbered columns test each lottery using all of its unaffected cohorts and the even-numbered columns test each lottery using the two adjacent unaffected cohorts. Consistent with random assignment, the estimates are neither uniformly positive nor uniformly negative and none are statistically significant.

A second possible concern with our empirical strategy relates to the validity of the exclusion restriction for the TSIV estimates. In particular, one might be concerned that draft-eligible men may have engaged in draft avoidance behaviors that could affect their probability of incarceration.[24] Using hypothetical APNs taken from the 1969, 1970, and 1971 drawings, we test for this possibility by considering possible effects on men who were assigned low

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[22]The robustness checks presented in this section focus on whites but the results are qualitatively similar for nonwhites.

[23]In particular, the Vital Statistics of the United States do not provides birth data by month, gender, and race for earlier or later cohorts.

[24]Of particular concern, although the evidence is based on a very small sample, Kuziemko (2008) presents suggestive evidence that men with low lottery numbers may have engaged in delinquent behaviors to avoid being drafted.

draft lottery numbers in the four non-binding lotteries taking place in 1972–1975. Since these lottery numbers were assigned but their results were not used to induct men into the military, we expect to see no link between low lottery numbers and violent crime unless lottery numbers affected criminality through mechanisms besides military service. Table 9 shows these results for violent crime among those born 1953–1956. Again, the results are not consistently positive or negative and none are statistically significant.[25]

7 Discussion and Conclusion

Like Lochner and Morreti (2004), our results highlight the importance of "nurture" beyond an individual's immediate circumstances. Also like Lochner and Morreti, we find especially large effects on violence. We find robust evidence that military service increases the probability of incarceration for violent crimes among whites, with point estimates suggesting an impact of 0.27 percentage points.[26] Perhaps not surprisingly, our estimates suggest that military service has a much greater impact on criminal behavior than Lochner and Morreti find for education. To put the relative magnitudes into context, our estimates suggest that military service is equivalent a twelve year reduction in schooling for whites.[27]

While our estimation strategy only allows us to estimate the effects of military service during the Vietnam Era, there is good reason to expect that our results may be relevant for the modern military as well. Between 14 and 25 percent of veterans returning from Iraq and

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[25]As another robustness check, we have considered the interaction between incarceration for a violent crime and non-Army military service as an outcome. Since nearly all drafted men served in the Army, we should not find significant effects on this outcome. Indeed, we find draft eligibility significantly raises the probability of being a violent offender and an army veteran and has no effect on being a violent offender and a veteran from another branch of service.

[26]These findings are broadly consistent with much of the prior literature that has considered military service as a determinant of violent behavior. Most notably similar to our study in terms of the treatment, Rohlfs (2010) finds that combat exposure leads to increases in self-reported violent crimes among both whites and nonwhites.

[27]This comparison is solely meant for illustrative purposes. It is likely that there are nonlinearities in the effect of education on crime that Moretti and Lochner (2004) are unable to identify with their empirical strategy.

Afghanistan have problems related to posttraumatic stress disorder. This is quite similar to the estimated 18 to 20 percent of Vietnam veterans exhibiting the same symptoms.[28]

The military readily acknowledges that soldiers often struggle with the transition to civilian life and that skills that promote success in combat can translate into unhealthy behaviors at home. While each branch of the military has programs to help ease the transition with some promising results for the average soldier (Castro et al. 2006; Adler et al. 2009), recent evidence raises serious concerns about the treatment of servicemen with the most-severe mental problems (Stahl 2009).[29] Coupled with this mixed evidence on the efficacy of the treatment of soldiers at risk of mental health problems, our results, which demonstrate grave consequences of military service, highlight the need for further research in this area.

Our results also have at least one additional important policy implication for the military. In particular, our results reveal social costs of military service that are not directly accounted for in military policy. Based on our more robust estimates that focus on whites, a conservative back of the envelope calculation similar to Lochner and Morreti (2004) suggests that increasing the number of soldiers in the Army by 10,000 entails a $30.5 million cost in terms of increased violent crimes.[30] If these costs are not considered, the optimal size of the military and the optimal number of military endeavors will both be overstated.

Finally, our results have important implications for the legal system, which has 23 recently

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[28]These statistics are taken from a 2008 U.S. Department of Health and Human Services report.

[29]In response to a survey from the Warrior Transition Unit at Fort Hood, where physically and mentally wounded soldiers are sent to heal, 41 percent of commanding officers thought more than half of soldiers claiming to have symptoms of posttraumatic stress disorder were faking or exaggerating versus 11 percent of nurse case managers. Both officers and nurses agreed that soldiers with behavioral health problems were overmedicated—nearly 50 percent in each group responded that they were "extremely confident" (10 on a scale of 1–10) that these soldiers were prescribed too many pain killers.

[30]The U.S. Army currently has more than one million troops. This calculation is based on an estimated impact of military service on incarceration for murder of 0.090 percentage points, an estimated impact on sex crimes of 0.015 percentage points, and the remaining overall effect on violent crimes (0.27 percentage points) being apportioned to robbery and assault. These imprecise estimates that are further stratified are available upon request. This estimate is based only on costs to victims and incarceration costs and assuming that all crimes lead to incarceration. Costs reported in Lochner and Morreti (2004) are $3,024,359 for murder, $89,221 for rape, and approximately $9,500 for robbery and assault.

established pilot courts that try only cases in which the offender is a veteran.[31] Possibly out of some sense of society's responsibility for their behavior, these courts focus on rehabilitation and treatment programs instead of incarceration. In 2008, senators Kerry and Murkowski introduced legislation to extend the program nationally. The existence of this special court system implicitly creates a separate legal class for veterans and tacitly acknowledges that military service can have negative consequences that manifest in criminal behavior once servicemen return home. But these courts exclude the violent offenders. Our analysis suggests that these are the offenses for which military service is most clearly responsible.

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[31]Details on these courts can be found at the Veterans Treatment Court Clearinghouse which is hosted by the National Association of Drug Court Professionals.

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Figure 1

White Incarceration Rates (Per 10,000) by Draft Eligibility and Birth Cohort

Panel A: Overall Incarceration Rates

Panel B: Incarceration Rates for Violent Crimes
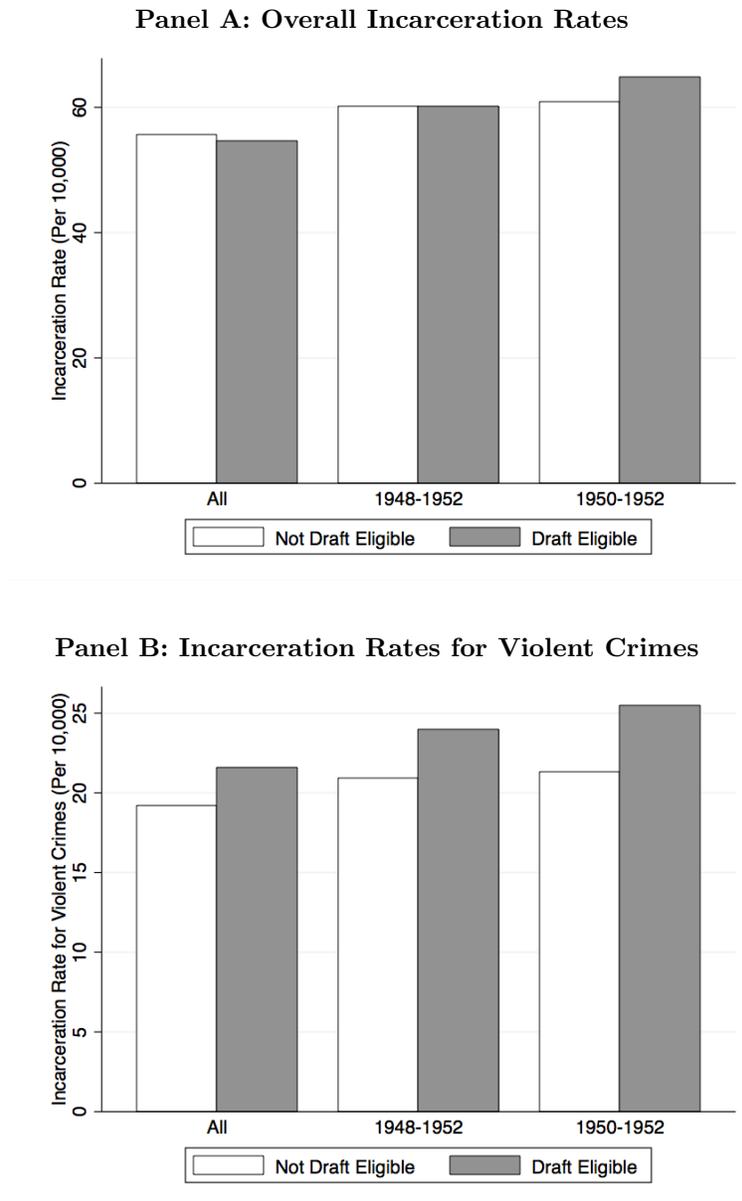
Notes: Incarceration data is from the 1979, 1986, and 1991 Surveys of Inmates in State and Federal Correctional Facilities and birth data is from Vital Statistics of the United States. All drafted cohorts include birth years ranging from 1944 to 1953.

Table 1

Angrist and Chen's (2008) Estimated Effects of Draft Eligibility on Military Service

Cohorts	1948–1952 (1)	1950–1952 (2)	1950 (3)	1951 (4)	1952 (5)
Panel A: Whites					
Draft-eligibility effect	0.112***	0.145***	0.133***	0.138***	0.168***
	(0.0010)	(0.0013)	(.0024)	(.0023)	(.0022)
Panel B: Nonwhites					
Draft-eligibility effect	0.072***	0.094***	0.090***	0.096***	0.096***
	(0.0028)	(0.0034)	(0.0059)	(0.0060)	(0.0063)

Notes: Angrist and Chen (2008) controlled for year, state, and month of birth and used sampling weights in calculating these estimates. Their sample was based on restricted use data from the U.S. Census. While we are unable to control for state of birth in estimates that use our data, random assignment of lottery numbers implies that this should not lead to bias.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 2

Mean Incarceration Rates (Per 10,000) for Whites

	Draft Eligible	Not Draft Eligible
All Crime	54.7	55.6
Violent Crime	21.6	19.1
Drug Crime	18.0	19.3
Property Crime	10.1	10.4
Public Order Crime	7.1	8.2

Notes: Observations are at the exact day of birth by survey year level. Incarceration data is from the 1979, 1986, and 1991 Surveys of Inmates in State and Federal Correctional Facilities and birth data is from the Vital Statistics of the United States.

Table 3

Estimated Effects of Draft Eligibility and Military Service on Incarceration
for Any Crime for Whites

Cohorts	All (1)	1948–1952 (2)	1950–1952 (3)
Estimated effect of eligibility per 10,000	-0.301 (2.105)	0.461 (2.818)	4.690 (3.579)
% Impact	-0.5	0.8	7.7
TSIV estimated of effect of service	—	0.0004 (0.0025)	0.0032 (0.0025)
Observations	9,864	5,481	3,288

Notes: Observations are at the exact day of birth by survey year level. Incarceration data is from the 1979, 1986, and 1991 Surveys of Inmates in State and Federal Correctional Facilities and birth data is from the Vital Statistics of the United States. All specifications include month-by-year of birth fixed effects and survey year fixed effects and weight by the number of individuals represented by the cell. All drafted cohorts include birth years ranging from 1944 to 1952. Estimated standard errors, clustered on lottery number, are shown in parentheses. The two sample instrumental variables (TSIV) estimates of the effect of military service on incarceration use Angrist and Chen's (2008) estimates of the effects of draft eligibility on veteran status.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 4
Estimated Effects of Draft Eligibility and Military Service on Incarceration
for Violent Crimes for Whites

Cohorts	All (1)	1948–1952 (2)	1950–1952 (3)
Estimated effect of eligibility per 10,000	2.562**	3.015*	4.038*
	(1.133)	(1.586)	(2.062)
% Impact	13.5	14.5	19.1
TSIV estimated of effect of service	—	0.0027*	0.0028*
		(0.0014)	(0.0014)
Observations	9,864	5,481	3,288

Notes: See Table 3.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 5

Estimated Effects of Draft Eligibility and Military Service on Incarceration
By Crime Type for Whites

Crime	Drug			Property			Public Order		
Cohorts	All (1)	1948–1952 (2)	1950–1952 (3)	All (4)	1948–1952 (5)	1950–1952 (6)	All (7)	1948–1952 (8)	1950–1952 (9)
Est. effect of eligibility per 10,000	-0.363 (1.282)	0.448 (1.860)	2.562 (2.361)	0.148 (0.728)	-0.078 (1.003)	-0.095 (1.328)	-1.085 (0.877)	-0.183 (1.013)	-0.352 (1.323)
% Impact	-1.9	2.1	11.8	1.4	-0.7	-0.7	-13.4	-2.3	-4.2
TSIV estimated of effect of service	—	0.0004 (0.0017)	0.0018 (0.0016)	—	-0.0000 (0.0009)	-0.0000 (0.0009)	—	-0.0002 (0.0009)	-0.0002 (0.0009)
Observations	9,864	5,481	3,288	9,864	5,481	3,288	9,864	5,481	3,288

Notes: See Table 3.

* significant at 10%; ** significant at 5%; *** significant at 1%

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Table 6

Estimated Effects of Draft Eligibility and Military Service on Incarceration for Violent Crimes

By Cohort and Survey Year for Whites

Cohort Survey Years	1948–1952 All (1)	1950–1952 All (2)	1950 All (3)	1951 All (4)	1952 All (5)	1948–1952 1979 (6)	1948–1952 1986 (7)	1948–1952 1991 (8)
Est. effect of eligibility per 10,000	3.015* (1.586)	4.038* (2.062)	3.763 (3.277)	2.098 (3.116)	6.606 (4.247)	3.804** (1.611)	1.625 (2.282)	3.617 (3.617)
% Impact	14.5	19.1	18	11.2	28.3	31	8.3	11.9
TSIV estimated of effect of service	0.0027 0.0014	0.0028 0.0014	0.0028 0.0025	0.0015 0.0023	0.0039 0.0025	0.0034 0.0014	0.0015 0.0020	0.00323 0.0032
Observations	5,481	3,288	1,095	1,095	1,098	3,288	3,288	3,288

Notes: See Table 3.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 7

Estimated Effects of Draft Eligibility and Military Service on Incarceration
By Crime Type, Cohort, and Survey Year for Nonwhites

| Cohort | 1948–1952 | 1950–1952 | 1950 | 1951 | 1952 | 1948–1952 | 1948–1952 | 1948–1952 |
| Survey Years | All | All | All | All | All | 1979 | 1986 | 1991 |
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: All Crimes								
Est. effect of eligibility per 10,000	20.678	42.153**	47.905	29.987	49.783	-1.097	-12.148	75.280*
	(15.563)	(20.664)	(31.369)	(34.188)	(39.418)	(17.187)	(19.134)	(38.798)
% Impact	6.4	12.5	15.2	9.4	13.7	-0.4	-4.3	17.5
TSIV estimated of effect of service	0.0287	0.0448	0.0532	0.0312	0.0519	-0.0015	-0.0169	0.1046
	(0.0216)	(0.022)	(0.0350)	(0.0357)	(0.0412)	(0.0239)	(0.0266)	(0.0540)
Observations	5,481	3,288	1,095	1,095	1,098	1,827	1,827	1,827
Panel B: Violent Crimes								
Est. effect of eligibility per 10,000	18.265*	29.382**	67.455***	17.433	-0.837	-5.752	-9.271	69.818***
	(9.747)	(12.331)	(20.939)	(23.973)	(24.946)	(11.393)	(15.010)	(24.725)
% Impact	11.4	17.4	52.2	10.1	-0.4	-4.5	-5.3	39.3
TSIV estimated of effect of service	0.0254	0.0313	0.07495	0.0182	-0.0009	-0.0080	-0.0129	0.0970
	(0.0136)	(0.0132)	(0.0238)	(0.0250)	(0.0260)	(0.0158)	(0.0209)	(0.0345)
Observations	5,481	3,288	1,095	1,095	1,098	1,827	1,827	1,827

Notes: See Table 3.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 8
Robustness Check Applying Lotteries to Unaffected Cohorts
Estimated Effects of Draft Eligibility and Military Service Incarceration for Violent Crimes

Cohort's Lottery Used	1944–1950		1951		1952	
Cohort's In Analysis	1942, 1943 1951–1959	1943, 1951	1942–1950 1952–1959	1950, 1952	1942–1951 1953–1959	1951, 1953
	(1)	(2)	(3)	(4)	(5)	(6)
Est. effect of eligibility per 10,000	-1.173 (1.082)	1.080 (2.070)	0.010 (0.832)	2.646 (2.643)	0.802 (0.987)	2.334 (2.569)
% Impact	-4.7	6.6	0.0	11.4	3.6	11.7
TSIV estimated of effect of service	-0.0010 (0.0010)	0.0010 (0.0018)	0.0000 (0.0007)	0.0024 (0.0024)	0.0007 (0.0009)	0.0021 (0.0023)
Observations	12,051	2,190	18,627	2,193	18,624	2,190

Notes: See Table 3. Estimates use only observations for whites.

* significant at 10%; ** significant at 5%; *** significant at 1%

Table 9

Robustness Check Using Nonbinding Lotteries for 1953-56 Birth Cohorts

Estimated Effects of Draft Eligibility and Military Service Incarceration for Violent Crimes

| *Highest APN Applied*	95	125	195
	(1)	(2)	(3)
Estimated effect of eligibility per 10,000	-0.179	-2.206	0.843
	(1.958)	(1.818)	(1.716)
% Impact	-0.7	-8.3	3.3
Observations	4,383	4,383	4,383

Notes: See Table 3. Estimates use only observations for whites.

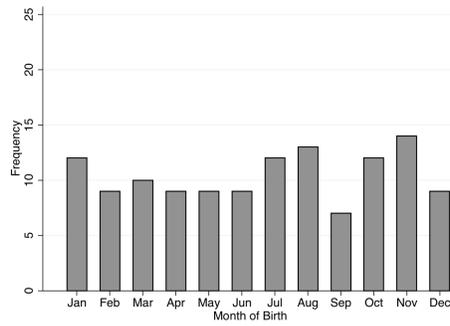* significant at 10%; ** significant at 5%; *** significant at 1%

Appendix 1: Additional Figures and Tables

Figure A1
Lottery Numbers Draft Eligible By Birth Month
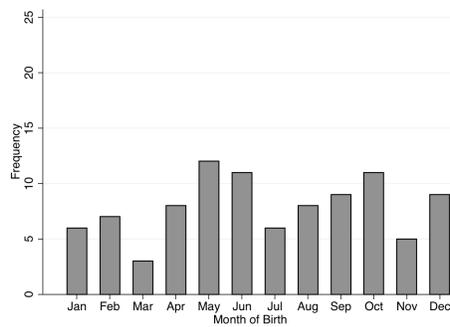
1969 Lottery
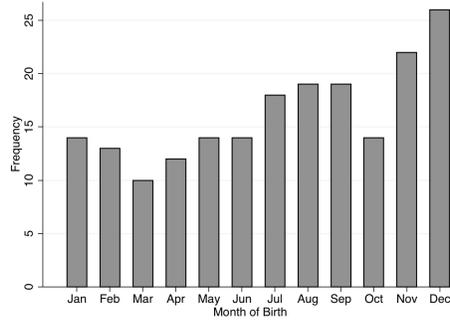
1970 Lottery

1971 Lottery

Table A1

Estimated Effects of Draft Eligibility and Military Service on Incarceration for Any Crime

By Cohort and Survey Year for Whites

Cohort	1948–1952	1950	1951	1952	1948–1952	1948–1952	1948–1952	1948–1952
Survey Years	All	All	All	All	1979	1986	1991	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Est. effect of eligibility per 10,000	0.461	4.690	0.335	7.805	6.022	0.456	-3.629	4.556
	(2.818)	(3.579)	(5.949)	(5.981)	(6.544)	(2.526)	(2.956)	(7.175)
% Impact	0.8	7.7	0.5	14.3	9.4	1.4	-9.5	4.2
TSIV estimated of effect of service	0.0004	0.0032	0.0003	0.0057	0.0036	0.0004	-0.0032	0.0040
	(0.0025)	(0.0025)	(0.0045)	(0.0043)	(0.0039)	(0.0023)	(0.0026)	(0.0064)
Observations	5,481	3,288	1,095	1,095	1,098	1,827	1,827	1,827

Notes: See Table 3. * significant at 10%; ** significant at 5%; *** significant at 1%

Appendix 2: Alternative Strategies for Calculating Births per Day

As we describe in the main text, in order to calculate incarceration rates for exact dates of birth, we must construct the number of births per day based on the Vital Statistics of the United States, which only reports births per month for the cohorts we consider. The results we show throughout the paper apportion the number of births in each month evenly across the days in each month. In this section, we describe two alternative strategies that give nearly identical results. The first alternative that we have considered accounts for differing birth patterns across weekdays and weekends. It has been documented that in recent periods more cesarean sections and birth inductions take place on each weekday than on each weekend day (Dickert-Conlin and Chandra 1999), possibly because doctors want to schedule these procedures on days when the hospital is more heavily staffed. To account for this weekday-weekend variation, we match each day of the week in the data for our cohorts of interest to the same day of the week in the 1969 data for which we have daily birth counts. The percentage of births in the month that occurred on that day in the later data is used to apportion the total monthly births in the earlier data across days. Consider January 1st, 1950 which was a Sunday. The first Sunday in 1969 was January 5th. In 1969 2.7% of January births occurred on the first Sunday. So 2.7% of the births in January 1950 are assigned to January 1st, 1950. This procedure is repeated for each day and the percentages of birth in each month are normalized to 100. For some years the days in the first or last week of the year are matched forward or backward to find a match. For instance, in 1944 the 53rd week contains a Friday, Saturday, and Sunday. In 1969 the 53rd week only contains a Tuesday and a Wednesday. So for 1944 the last three days are assigned the birth percentages on Friday, Saturday, and Sunday that occurred in the 52nd week instead of the 53rd. Another alternative strategy we have considered recognizes that birth technology has changed over

the 25 years that elapse between the first year of interest and 1969 (the first year for which we have births at the day level, as used in the first alternative strategy above). We can obtain an estimate of the weekend effect that uses only data from the period of interest by exploiting the different number of weekend days that fall on a given month across years. We estimate:

$$Births_{ym} = \beta * WeekendDays_{ym} + v_y + \delta_m + \epsilon_{ym}. \tag{6}$$

This is a regression of the number of births in each month-year on the number of Saturdays and Sundays in the month with fixed effects for month and year. The coefficient β gives the decrease in the number of births when a month has one additional weekend day. January 1948 had one more Sunday than January 1947. The number of white births in January 1948 was less than the number of white births in January 1947. Some of the decrease in the number of births in January 1948 was due to the weekend effect. Since January had 31 days in both years, some of the decrease in births was due to births being shifted from the extra weekend day at the end of the month into February. The number of births in each month are then apportioned out where each weekend day gets a fewer number of births than each weekday. All weekdays are treated alike and all weekend days are treated alike. The advantage of this strategy is that it does not impose the weekend effect from a later era on the monthly birth data from 25 years earlier. We have also explored a variation of this strategy where the weekend effect is a percentage change in the total monthly births rather than a fixed decrease in the number of births. These strategies likely improve the accuracy of our measures of births per day and, hence, the accuracy of our measures of incarceration rates. However, because they do not change the results, we adopt the simpler and more transparent method described in the main text.