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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 that draft eligibility increases incarceration for violent crimes but decreases incarceration for non-violent crimes among whites. This is particularly evident in 1979, where two-sample instrumental variable estimates indicate that military service increases the probability of incarceration for a violent crime by 0.34 percentage points and decreases the probability of incarceration for a nonviolent crime by 0.30 percentage points. 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.

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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 causal effects of individuals’ immediate environments.¹ Quasi-experimental studies that explore how individuals’ backgrounds affect criminal behavior are more rare with a handful of studies on neighborhoods (Oreopoulos 2006), education (Lochner and Moretti 2004), foster care (Doyle 2008), peers (Bayer, Hjalmarsson, and Pozen’s 2009), and beauty (Mocan and Tekin 2010) providing notable exceptions. In this paper, we add to this strand of the literature by exploiting the randomness of the national Vietnam draft lotteries to examine the effects of military service on subsequent incarceration.

Our study also has implications for the military and for the treatment of veterans. First, this paper can be thought of as exploring a potentially important long-term cost of military engagements that might be important for comprehensive cost-benefit considerations. Second, our results speak to what types of special accommodations might be reasonably made for those who have served in the military. This is an issue that has been taken quite seriously in the criminal justice system, as special courts that focus on rehabilitation have been set up to try cases involving non-violent veteran offenders. Further, the results of our analysis

¹For example, researchers have considered the effects of punishments for infractions (Levitt 1998; Drago Galbiati, and Vertrova 2009), policing (Levitt 1997; Levitt 2002; McCrary 2002; Yang 2008), punishment (Lee and McCrary 2009; Hansen 2011) temporary income shocks (Miguel 2005; Foley 2011), unemployment (Gould, Weinberg, and Mustard 2006; Mocan and Bali 2010), inequality (Kelly 2000), drugs and alcohol (Grogger and Willis 2000; Carpenter 2007; Carpenter and Dobkin 2008), neighborhoods (Ludwig, Duncan, and Hirschfield 2001; Kling, Ludwig, and Katz 2004), guns (Duggan 2001; Duggan, Hamjalmarrson, and Jacob forthcoming), sporting events and movies (Rees and Schnepel 2009; Card and Dahl 2009; Dahl and DellaVigna 2009), casinos (Grinols and Mustard 2006), and incapacitation (Jacob and Lefgren 2003; Dahl and DellaVigna 2009).

can inform the extent to which resources ought to be allocated towards the treatment of veterans who might exhibit signs of instability.

While we consider the impacts of military service on multiple types of crimes, our primary focus is on violent crimes. Although this would be a natural choice for any study considering the effects of military service on crime since the military trains soldiers to engage in violence, the Vietnam Era provides an especially interesting context. Notably, the Vietnam Era coincided with an important shift in military training motivated by S.L.A. Marshall's pioneering research documenting extremely-low firing rates for U.S. soldiers serving in World War II. In order to overcome soldiers reluctance to fire at enemy combatants, in the late-1960s the military began making conscious efforts to provide more realistic training scenarios (Grossman 2009).² While this desensitization to engaging in violence may be crucial to survival in a combat zone, it is easy to see how it might lead to problems after a soldier returns to civilian life.³

Of course, there are several other possible mechanisms through which military service might affect crime. Engagements with real-enemy combatants in the combat zone has been shown to have impacts over and above the effects of being in the military (Rohlf's 2010; Galiani, Rossi, and Schargrodsky 2011; Cesur, Sabia, and Tekin 2011). In addition, military service may increase crime because it precludes labor market experience and thus reduces wages (Angrist 1990; Imbens and van der Klaauw 1995; Abadie 2002; Angrist and Chen

²For example, using silhouettes in place of bulls-eye targets. Slone and Friedman (2008) describe modern training as preparing soldiers "to react within a split-second of any provocative activity and [to shut down] emotions."

³In a similar fashion, this training may in part be responsible for some of the violent conflicts 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.

2011; Siminski and Ville 2011) or because of possible effects on opiate use (Robins, Davis, and Goodwin 1974). On the other hand, the discipline imparted by the military environment may make individuals less likely to commit crimes. Further, military service could reduce criminality via an incapacitation effect, as individuals are in the military environment at the ages at which they are at highest risk of incarceration.

A sizable literature links military service to criminal behavior, particularly to violent behavior, but much of the prior work on this topic lacks plausibly exogenous variation and focuses on small non-random samples. Exogenous variation in military service is crucial since men who are more likely to engage in criminal activities may be disproportionately likely to enlist. Galiani, Rossi, and Schargrotsky (2011) overcome this selection bias using variation driven by Argentina’s draft lotteries. Relative to our study, this earlier work has the advantage of being able to explore cohorts serving the Malvinas War and others serving during peacetime. However, it is somewhat limited in its ability to measure impacts by type of crime, which can only be identified for those going through the criminal justice system approximately 20–30 years after service. Our results suggest that this limitation is not trivial, as we find offsetting effects on incarceration for violent and nonviolent crimes seven to nine years after conscription.⁴

In this paper, we also use variation provided by draft lotteries but focus on the U.S. context. 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

⁴Rohlf (2006) is the only prior work to use plausibly exogenous variation to consider the effects of military service on incarceration in the U.S. In this study, in which he compares the fraction of Vietnam Era draft eligible inmates in prison to the fraction expected based on cohorts not subjected to the drafts, he finds imprecise effects on overall rates of incarceration. Our study offers several advantages over this work. First, we improve precision by using within cohort variation provided by the draft lotteries instead of a cross-cohort difference-in-differences framework. This further enables us 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. In addition, our outcome variable lends itself to a natural interpretation, providing a direct estimate of the effect of draft eligibility on the probability of incarceration in the survey years. Finally, we present a more-comprehensive exploration of the effects of draft eligibility on crime by separately considering its effects on violent crime, drug-related crime, property-related crime, and public-order crime.

that the military drafted men, starting with the lowest lottery numbers, until manpower requirements were met each year. Utilizing this exogenous variation in draft status, we are able to determine the extent to which military service affects criminal behavior by comparing the probability of incarceration (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 (SISFCF) with data from the Vital Statistics of the United States to create measures of incarceration probabilities for each day of birth for the cohorts affected by the draft lotteries. We supplement this analysis with data on prison admissions from 1983–1991 via the National Corrections Reporting Program (NCRP).

While these inmate data are well-suited to identifying the effect of draft eligibility, they are not well-suited to *directly* estimating the effect of *military service*. In particular, it would be inappropriate to estimate the first-stage effect of draft eligibility on military service using an endogenously-selected subsample of individuals exposed to the draft, such as a sample of inmates. For this reason, we obtain first-stage estimates for the overall population using restricted U.S. Census data from 2000. Combining the estimates from each of these sources, we obtain two-sample instrumental-variable estimates of the effect of military service on incarceration. We discuss potential threats to the validity of this approach in Section 4.

We find evidence of positive impacts on incarceration for violent crimes among whites and offsetting impacts of a similar magnitude on incarceration for nonviolent crimes. This is particularly evident in 1979, where two-sample instrumental variable estimates indicate that military service increases the probability of incarceration for a violent crime by 0.34 percentage points and decreases the probability of incarceration for a nonviolent crime by 0.30 percentage points. We find less convincing evidence of impacts on nonwhites.

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 empirical strategy. Section 5 presents our results and robustness checks. Section 6 discusses our results and concludes.

2 Background on the Draft Lotteries

In an attempt to fairly allocate military service in Vietnam, a total of seven national lottery drawings were held to determine who would serve in the military—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 individuals with RSNs less than or equal to the APN as “draft eligible.”

While the issue was addressed for later drawings, there was a noteworthy 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

thus 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 to which 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 and address this potential issue by controlling for year by month of birth fixed effects in our analysis (Conley and Heerwing 2009, Eisenberg and Rowe 2009, Angrist, Chen, and Frandsen 2010, Angrist and Chen 2011).⁵

For multiple reasons, 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 eligible days could fail the medical exams, refuse to report, or apply for various exemptions. Despite these issues, the draft had a significant effect on military service, the magnitude of which is discussed in Section 5.1.

3 Data Description and Construction

Our primary analysis uses data on incarceration from the 1979, 1986, and 1991 Surveys of Inmates in State and Federal Correctional Facilities (SISFCF), which are representative of the prison population in state and federal correctional facilities. Although it would be desirable to use the 1974 survey to consider potential incapacitation effects, exact dates of birth are not available for this survey year. In addition to exact dates of birth, the survey waves we use contain information on each prisoner's 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 define a prisoner as incarcerated for a violent crime if any of the listed offenses involve violence and as incarcerated for a nonviolent

⁵Information 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).

crime if none of the listed offenses involve violence.

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 an appropriate sample to analyze, there is a tradeoff between ease of interpretation of the results and sample size. The most-straightforward 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, the estimates will provide 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 prison 7–9, 14–16, and 19–21 years later. On the other hand, pooling survey years can improve precision. For this reason, we present estimates that utilize all of the available data and estimates stratified on survey years.

Limiting the sample to males, we conduct the analysis separately for whites and non-whites at the date of birth by survey year level. Each observation represents a collapsed cell measuring the probability of incarceration 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 date of birth d , calculated using the SISFCF’s sampling weights, by the number of males that were born in the United States on day d :

$$IncarcerationProbability_{sd} = \frac{\#ofInmates_{sd}}{\#ofBirths_d}. \quad (1)$$

The denominator for the equation above comes from the Vital Statistics of the United States (VSUS) which reports births by race, gender, and month. Since the VSUS only reports births by month prior to 1969, we 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.

The data used to estimate the first-stage effect of draft eligibility on military service are from the 2000 Census long-form sample, which includes approximately one-sixth of U.S. households. For more details on these data, see Angrist and Chen (2011) whose sample is identical.

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 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

$$IncarcerationProbability_{sd} = \theta + \alpha * VeteranProbability_d + u_{sd}, \quad (2)$$

where $IncarcerationProbability_{sd}$ is the fraction of individuals born on day d who are incarcerated in survey year s and $VeteranProbability_d$ 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 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.⁶ Given the random assignment of *draft eligibility* by date of birth, we instead estimate

$$IncarcerationProbability_{sd} = \phi + \gamma * DraftEligible_d + \epsilon_{sd} \quad (3)$$

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. The parameter γ provides the estimate of the average reduced-form effect of draft eligibility on the probability of incarceration. 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 effects and survey year fixed effects where applicable.⁷

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

$$VeteranProbability_d = \eta + \beta * DraftEligible_d + \omega_d. \quad (4)$$

Because an unbiased estimate of β requires data on a random sample of the population, as opposed to an endogenously-selected subsample of inmates, we estimate the person-level analogue of Equation 4 using restricted-use U.S. Census data from 2000.⁸ We then obtain the two-sample instrumental-variable estimate by taking the ratio of the reduced-form estimate and the first-stage estimate,

$$\hat{\alpha}_{TSIV} = \frac{\hat{\gamma}}{\hat{\beta}}, \quad (5)$$

⁶Note that, although aggregation to the date-of-birth level may lessen such concerns, they would likely remain because of seasonal patterns in fertility that are associated with socioeconomic status (Buckles and Hungerman 2010). This approach is also problematic because, outside of the cohorts subject to the draft lotteries, there is unlikely to be much variation in veteran status across dates of birth.

⁷While it is desirable to control for other covariates to increase the precision of estimates, Angrist (1989) suggests that this is not necessary to avoid bias since there is no correlation between draft lottery status and characteristics besides subsequent veteran status.

⁸That is, we regress whether an individual is a veteran on whether an individual was draft eligible.

and estimate its standard error using the delta method.⁹ The standard-error estimates for the first-stage and reduced-form estimates used in this calculation are clustered on lottery numbers to address the fact that the former is based on individual-level data while the latter is based on data aggregated to the birth-date level.

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 the probability of incarceration. We acknowledge that $\hat{\alpha}$ will be biased if draft eligibility also affects incarceration probabilities through other mechanisms. It has been documented that eligibility had a positive impact on educational attainment (Angrist and Krueger 1992, Card and Lemieux 2001; and Angrist and Chen 2011).¹⁰ To the extent that increased education levels lead to decreased crime (Lochner and Moretti 2004) the extra education conferred by draft eligibility should bias our estimates of α downward. Another potential issue is that military service might affect incarceration through its impacts on mortality; however researchers have found little evidence that military service affects health (Conley and Heerwig 2009; Dobkin and Shabani 2009; Siminski and Ville 2011), which might be explained by the generous health benefits that tend to be provided to veterans.¹¹ In addition, the fact that our data exclude those serving in military prisons may cause us to understate the effect of military service on criminal behavior. In addition, we acknowledge that impacts on crime may diverge from impacts on incarceration if military service affects the probability of getting caught conditional on committing a crime or if veterans receive differential treatment from law enforcement officers or judges. We should also note that this instrumental variable approach identifies the local average treatment effect

⁹In particular, we assume $cov(\hat{\gamma}, \hat{\beta}) = 0$, which is likely to hold since the estimates are based on independent samples, yielding $var(\hat{\alpha}_{TSIV}) = \frac{var(\hat{\gamma})}{\hat{\beta}^2} + \frac{\hat{\gamma}^2 * var(\hat{\beta})}{\hat{\beta}^4}$. Bootstrapping produces nearly identical standard-error estimates.

¹⁰In contrast to these studies focusing on the United States, Siminski (forthcoming) finds no evidence of similar effects for Australia where there was no GI Bill.

¹¹Bedard and Deschênes (2006) provide a notable exception, finding that military service in World War II and the Korean War led to increased mortality due to increased smoking.

(LATE), or the effect of military service on those individuals who can be compelled to enter the military by the draft lotteries.

5 Results

This section is organized into multiple parts. We begin by presenting estimates of the first-stage effect of draft eligibility on military service. Next, we show summary statistics for incarceration probabilities and provide visual evidence supporting our main results. We then present our main results, which are followed by robustness checks to verify that these results are not driven by the particular birthdays that were drawn in any given lottery or by avoidance behaviors among eligible men. Finally, we conduct a supplementary analysis using prison admissions data from 1983–1991.

5.1 First Stage Effect of Eligibility on Military Service

As described above, an unbiased estimate of the effect of the Vietnam draft lotteries on military service requires a random sample of individuals exposed to the draft. We obtain these estimates using restricted-use U.S. Census data from 2000.¹²

Table 1 shows how draft eligibility affected military service for the 1944–1952 cohorts. As demonstrated in earlier studies, draft eligibility did not have a significant impact on the earliest of these cohorts subject to the national lottery—this is not surprising because a large share of the capable men in these cohorts were already called to serve via local drafts. In subsequent sections we follow the existing literature and focus on the 1948–1952 cohorts, for whom the first-stage estimate is clearly strong for both whites and nonwhites.

¹²Because of confidentiality requirements, we do not have direct access to this data. These results are based on specifications that Josh Angrist and Stacey Chen have generously run for us. Angrist and Chen (2011) 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 two-sample instrumental-variable estimates.

For these cohorts, eligibility increased the probability of military service by approximately 11 percentage points for whites and 7 percentage points for nonwhites, on average, with especially large impacts for those born 1950–1952.

5.2 Summary Statistics

Table 2 presents incarceration probabilities by survey wave, race, and draft eligibility status.¹³ The table separately considers incarceration 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 contribute to multiple lines in the table. In most cases, the statistics in Table 2 suggest that induction had only a small effect, if any, on criminality among whites. On the other hand, they suggest that induction increased incarceration for violent crimes by approximately 15 percent.

5.3 Visual Evidence

Figures 1 through 4 plot estimated probabilities of male incarceration by quarter of birth and draft eligibility. Although the data can be used to construct more-disaggregated (e.g., month, week, day) plots, aggregating to the quarterly level better allows us to discern patterns in the data; however, we also present month-level plots in the appendix.

Figure 1 focuses on white males incarcerated for violent crimes, with separate panels for those incarcerated in 1979, 1986, and 1991. Though the estimates are somewhat noisy, incarceration probabilities in 1979 appear systematically higher for those who are draft eligible relative to those whose birthdays made them ineligible. In particular, the estimated

¹³In the Appendix, tables A1, A2, and A3 present similar statistics by draft cohort.

¹⁴We 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.

probability of incarceration is higher among those who are draft eligible for 15 of 20 birth quarters. It is also the case that incarceration probabilities are usually higher among those who are draft eligible in 1986 and 1991 although this is less clear to the eye.

Figure 2 shows similar graphs for white males incarcerated for nonviolent crimes. The estimates are again imprecise but collectively suggest that incarceration probabilities for nonviolent crimes are lower in 1979 and 1986 among those were draft eligible.

Figures 3 and 4 show similar graphs for nonwhite males incarcerated for violent and non-white violent crimes, respectively. These graphs largely suggest that there is no relationship between draft eligibility and incarceration among nonwhites. The one exception, investigated in more detail below, is that incarceration for violent crimes appears higher among the draft eligible population than the ineligible population in 1991.

These figures provide suggestive evidence of effects on criminal behavior, though the means presented in these figures are rather noisy. For this reason, we pool all of the cohorts together in the next section to improve precision.¹⁵

5.4 Main Results

Table 3 reports the estimated effects of draft eligibility and military service on incarceration probabilities among whites, with separate panels for violent crimes, nonviolent crimes, and all crimes. The data are aggregated to 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 that later birth months had a higher probability of being drawn in the 1969 draft due to mechanical problems with the lottery board's randomization method.

Column 1 shows estimates that pool data from the three survey years while also controlling for survey year fixed effects. These estimates echo the results presented in the previous

¹⁵With more data, it would be quite informative to obtain separate estimates for each cohort, especially because they had differing experiences as the war was winding down when the youngest cohorts served.

section. The estimated impact on incarceration for a violent crime is significant at the ten-percent level, indicating that eligibility increased the probability of incarceration by approximately 0.03 percentage points. The corresponding two-sample instrumental-variables estimate indicates that Vietnam Era military service increased the probability of incarceration for a violent crime by 0.27 percentage points. In contrast, these data indicate a *negative* effect on incarceration for a nonviolent crime, although this estimate is not close to being statistically significant at any conventional level. That said, because of this offsetting impact, the estimated effect on the probability of incarceration for any crime (Panel C, Column 1) is close to zero.

Columns 2 through 4 stratify on the three survey years, with the most precise estimates using data from 1979 and the least precise estimates using data from 1991. To put these results into context, it is important to keep in mind that the men conscripted by the lotteries would have finished their mandatory service five to seven years before the 1979 survey was conducted.

The estimates using data from 1979 (Column 2) are qualitatively similar but stronger than the estimates that pool together the three survey years. The estimated impact of Vietnam Era military service on incarceration for a violent crime is 0.34 percentage points and significant at the five-percent level. The estimated impact on incarceration for a nonviolent crime is of a similar magnitude (-0.30 percentage points) and significant at the ten-percent level. Not surprisingly then, the estimated impact on incarceration for any crime is close to zero.¹⁶

The estimates using data from 1986 are qualitatively similar but do differ in important ways. In particular, the estimated impact on incarceration for a violent crime is smaller in magnitude (0.15 percentage points) and the estimated impact on incarceration for a

¹⁶Correlational evidence based on the 1980 Census suggests a small but significant negative effect of service in Vietnam on being observed in a correctional facility.

nonviolent crime is larger in magnitude (-0.47 percentage points). However, these estimates are not close to being statistically distinguishable from those focusing on incarceration in 1979.

The estimates using data from 1991 suggest a positive effect on incarceration for a violent crime and no effect on incarceration for a non-violent crime. That said, these estimates the least precise among those shown in Table 3, with standard error estimates two- to three-times larger than similar estimates using data from 1979.

Table 4 presents estimates for nonwhite men. These estimates suggest there is no effect of Vietnam Era service on incarceration for violent crimes in 1979 or 1986 but, curiously, indicate an large effect in 1991. These estimates suggest that there was either a large delayed impact on nonwhite males that manifested in the late 1980s or that the 1991 estimate is a statistical artifact. The results in Section 5.7, where we estimate impacts on prison admissions from 1983–1991, suggest that the latter explanation is most likely.

The estimated effects on nonwhite incarceration for nonviolent crimes are never statistically significant and, like the estimated effects on incarceration for violent crimes, vary in sign. That said, it is important to note that the first stage is relatively small for nonwhites and thus the confidence intervals relatively large. We cannot rule out that the effects of military service are the same for whites and nonwhites.

5.5 Estimates Using More-Narrow Crime Categories

In order to shed light on our main results, tables 5 and 6 show the effect of draft eligibility on subcategories of violent, property, and drug related crimes. Because incarceration probabilities are small for these narrowly-defined categories, these tables report estimated effects *per 10,000 births* instead of per person. These estimates should be interpreted with caution because the sample size of inmates contributing to each estimate is relatively small when the data has been disaggregated in this fashion. As a result, the estimates rarely rise to the

level of statistical significance and often change signs when considering data from different survey years.

The estimates that are relatively robust for whites (Table 5) suggest that the overall impact on violent crime among whites is driven by incarcerations for murder, robbery and kidnapping offenses. In contrast, the estimated impacts on nonviolent crime categories are not sufficiently robust to yield insight into our earlier results. The estimates for nonwhites (Table 6) demonstrate that the estimated impact on violent crime in 1991 among nonwhites is driven by robberies. More broadly, the estimated effects on these narrow categories of crime are not robust across survey years for nonwhites, with the exception of burglary for which we sometimes see significantly elevated rates among the draft eligible population.

5.6 Robustness Checks Using Lotteries for Unaffected Cohorts

In this section, we conduct two falsification tests, similar in spirit to those in Galiani, Rossi, and Schargrodsky (2011), in order to address potential concerns regarding the use of the lottery for identification.

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 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 disproportionately came from disadvantaged backgrounds.

To verify that this type of phenomenon is not driving our results, we apply each of the three lotteries to cohorts that the given lottery did not affect and conduct the analysis as before. 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 1941–1942 and 1951–1959 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 all of the unaffected cohorts that our data sets allow us to cover, ranging from 1942–1959.¹⁷

The results of this falsification exercise, by race and crime type, are presented in Table 7. Consistent with random assignment, the estimates are neither uniformly positive nor uniformly negative. Further, just two of the 48 “placebo tests” are significant at the ten-percent level.

A second possible concern with our empirical strategy relates to the validity of the exclusion restriction for the two-sample instrumental-variable 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.¹⁸ 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 draft lottery numbers in the four non-binding lotteries that took 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 8 shows these results by race and crime type. Again, the results are not consistently positive or negative and just two of 48 are statistically significant at the ten-percent level.¹⁹

¹⁷We cannot use earlier cohorts in this falsification exercise because earlier Vital Statistics of the United States reports do not provide birth data by month, gender, and race.

¹⁸Of 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. She also examines Georgia prison admissions data and finds that men with low lottery numbers in the non-binding 1972 lottery were over-represented. We also examine the 1972 lottery as a robustness check and find no detectable relationship between low lottery number and being incarcerated for the serious crimes that would have kept an offender in prison until the 1979 inmate survey. One possible reconciliation of our findings is that while some men may have “dodged down” into prison to avoid conscription, they did not commit the serious crimes with multi-year sentences we examine here.

¹⁹As 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.

5.7 Analysis of Prison Admissions Data, 1983–1991

In this section we use data from the NCRP to further investigate some of the results presented in prior sections. The NCRP provides data on all prisoners admitted to state correctional facilities on an annual basis. Although these data track all movements across prisons, we focus on admissions that are due to court commitments to reduce the likelihood of “double counting” prisoners. As in previous sections, we combine these data with vital statistics data, which are used for the denominator of the outcome variable. However, here we use the number of number of individuals admitted *per 10,000 births* at the exact date of birth level since the number of inmates admitted into prison per year is relatively small.

Panels A and B of Table 9 show no systematic evidence that draft eligibility is related to new admission of white prisoners in the mid-1980s to early 1990s, for violent or nonviolent crimes. In light of the results shown in the previous sections, there are two potential explanations for this finding. It may be the case that the effects of military service on criminal behavior fade out as veterans spend more time as civilians. Or this finding may simply reflect an incapacitation effect—we may be less likely to observe impacts on prison admissions in the 1980s because men who were affected most were already incarcerated in earlier years, as evidenced by the significant impacts we found on the prison population in 1979.

Panels C and D focus on admissions of nonwhite prisoners. Here, we again see no systematic evidence of a relationship between draft eligibility and prison admissions in the mid-1980s to early 1990s. This suggests that the statistically significant effect on of eligibility on incarceration for violent crimes that manifests in the 1991 prisoner survey data is likely a statistical artifact. Further corroborating this interpretation, we find similar results when we focus on robberies, the category that drove the aforementioned estimate in the prisoner data.

6 Discussion and Conclusion

Our results highlight the importance of one’s background on criminal behavior. We find that military service increases the probability of incarceration for violent crimes among whites, with point estimates suggesting an impact of 0.27 percentage points. Putting aside differences between the United States and Argentina, these results may initially seem to be at odds with Galiani, Rossi, and Schargrotsky (2011) who also exploit a draft lottery but do not find any evidence that military service affects violent crime. However, our analysis suggests that the effects on violent crime manifest soon after military service is complete, as they are present in 1979 for cohorts who served in the early 1970s. This is critical, as Galiani, Rossi, and Schargrotsky (2011) would be unable to detect such effects in their analysis that identifies the 1958–1962 cohorts going through the criminal justice system from 2000–2005.

We also find evidence of offsetting impacts on incarceration for nonviolent crimes among whites. This suggests that military service may not change an individual’s propensity to commit crime but instead may cause them to commit more-severe crimes involving violence.

While our identification strategy only allows us to estimate the effects of military service on conscripts during the Vietnam Era, multiple features of today’s military suggest that our results may be relevant today. The military has continued and escalated the use of highly realistic training simulations, a legacy of late-1960s efforts to desensitize soldiers to engaging with enemy combatants. For example, the military currently uses Iraqi nationals as role-players in training exercises in order to help cadets “put a human face and picture on Iraqi society.”²⁰ In addition, the rates of posttraumatic stress disorder for veterans of Iraq and Afganistan (14 to 25 percent) are quite similar to the rates for those who served in the Vietnam War (18 to 20 percent).²¹

²⁰For more details, see <http://www.army.mil/-news/2010/06/17/40960-iraqi-role-players-add-realism-to-cadet-training/>.

²¹These statistics are congressional testimony by Thomas R. Insel before the Committee on Oversight and Government Reform in 2007. Available online at: <http://www.hhs.gov/asl/testify/2007/05/t20070524a.html>

Further, today's 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. For this reason, each branch of the military has programs to help ease the transition. Although research highlights 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).²² Coupled with this mixed evidence on the efficacy of the treatment provided to 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.

Finally, our results have important implications for the legal system, which has 23 recently-established pilot courts that try only cases in which the offender is a veteran.²³ 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.

²²In 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.

²³Details 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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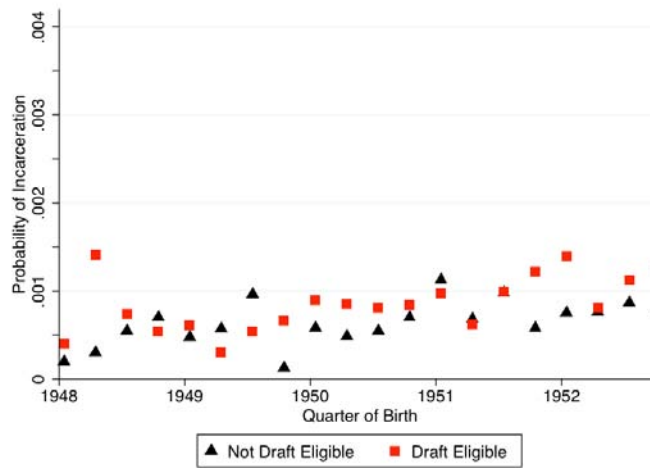
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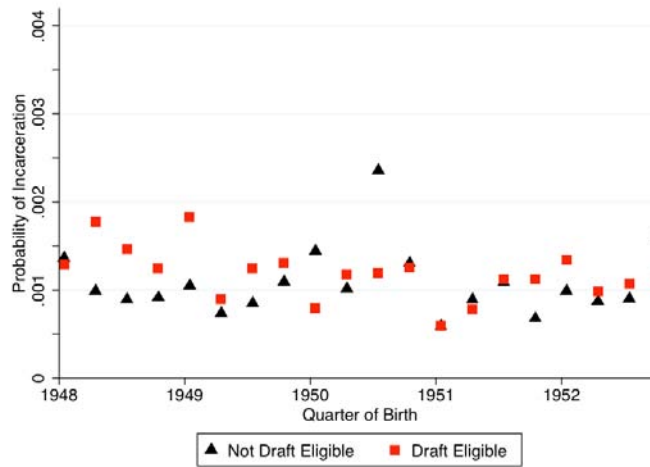
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Figure 1
Probability of Incarceration for a Violent Crime by Draft Eligibility, White Males

Panel A: 1979 Data



Panel B: 1986 Data



Panel C: 1991 Data

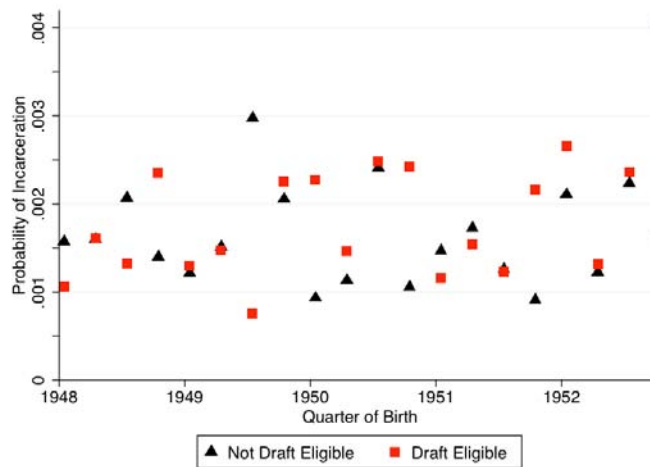
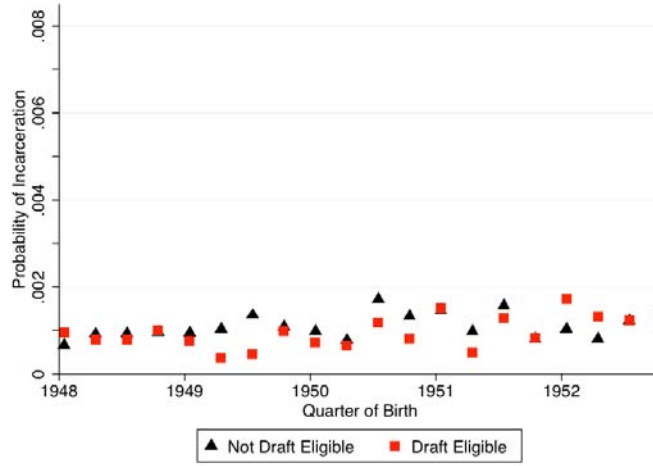
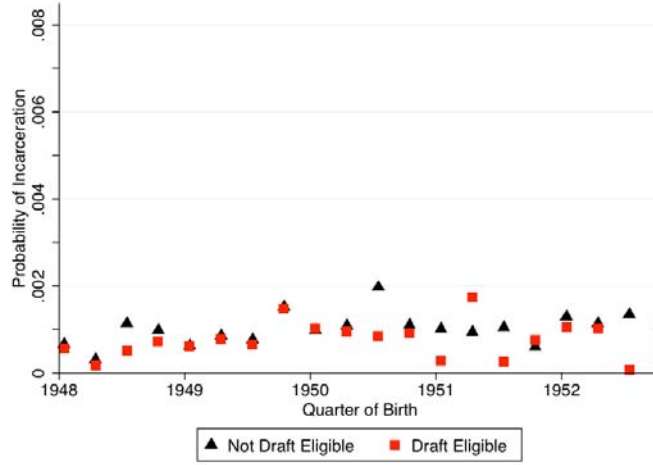


Figure 2
Probability of Incarceration for a Nonviolent Crime by Draft Eligibility, White Males

Panel A: 1979 Data



Panel B: 1986 Data



Panel C: 1991 Data



Figure 3
Probability of Incarceration for a Violent Crime by Draft Eligibility, Nonwhite Males

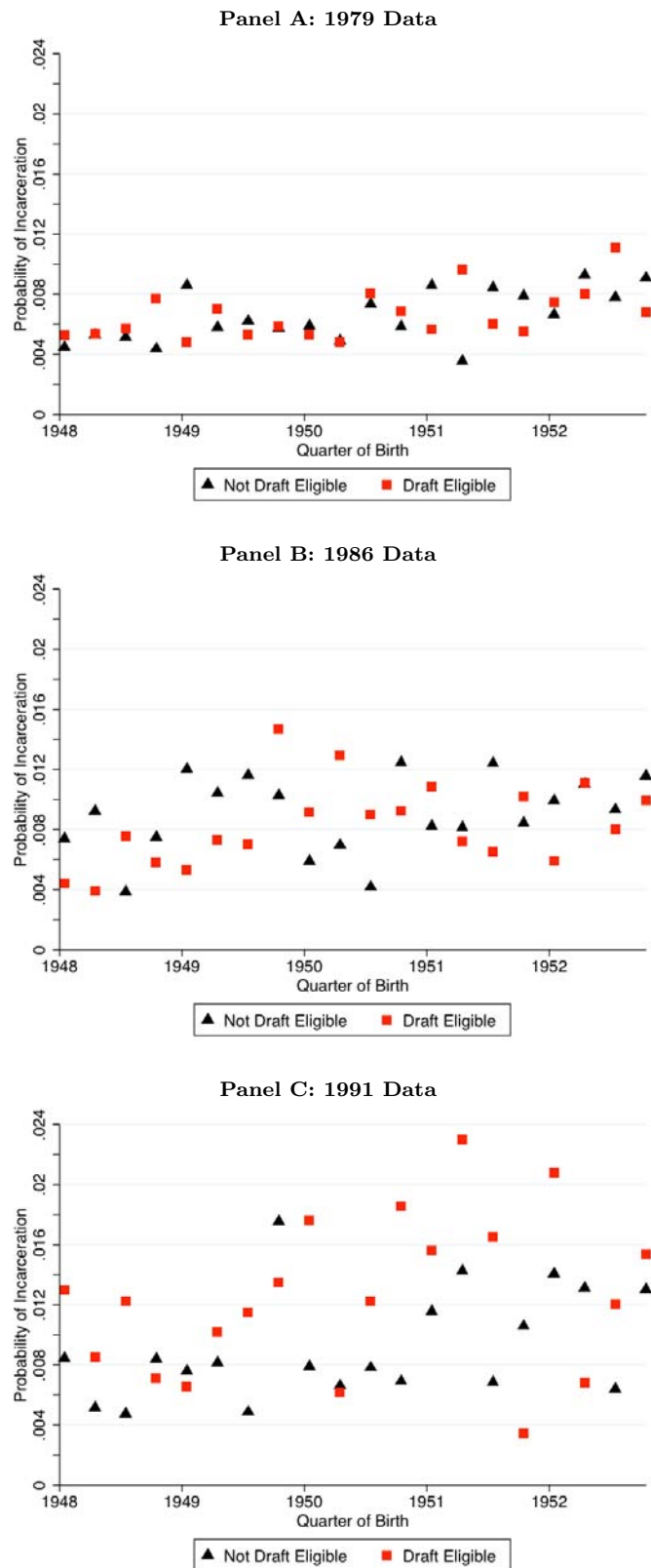


Figure 4
 Probability of Incarceration for a Nonviolent Crime by Draft Eligibility, Nonwhite Males

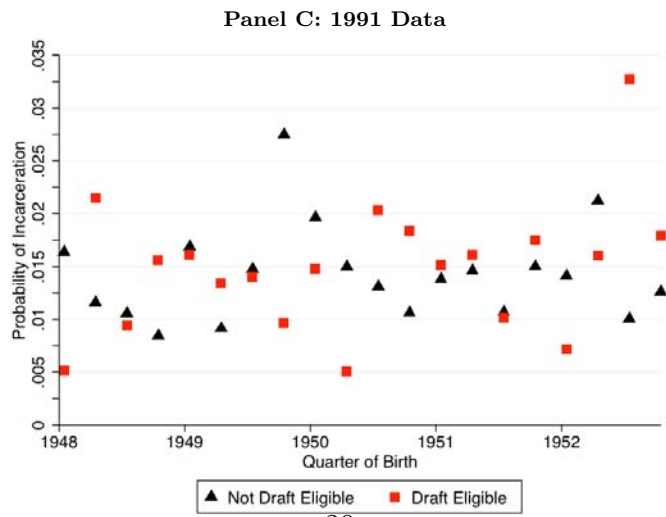
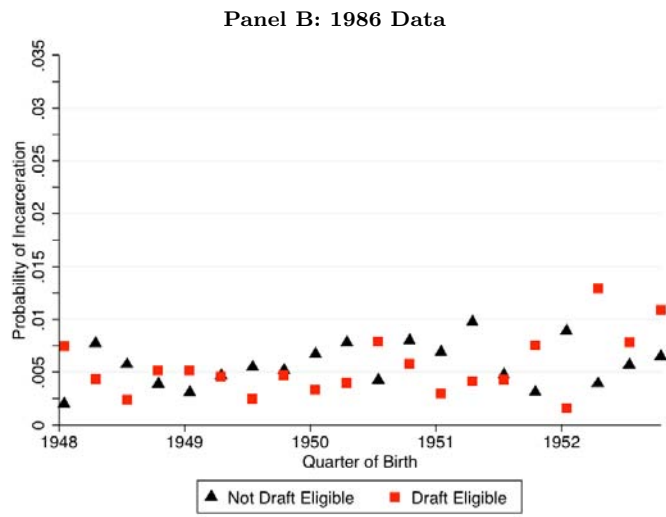
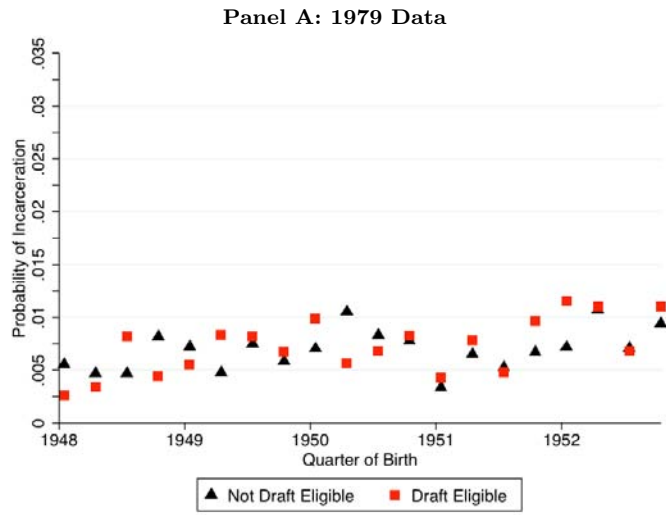


Table 1
Estimated First-Stage Effects of Draft Eligibility on Military Service

<i>Cohort:</i>	1944 (1)	1945 (2)	1946 (3)	1947 (4)	1948 (5)	1949 (6)	1950 (7)	1951 (8)	1952 (9)	1948-52 (10)
<i>Panel A: Whites</i>										
Draft-eligibility effect	-0.0047* (0.0027)	0.0021 (0.0028)	0.0145*** (0.0026)	0.0344*** (0.0026)	0.0577*** (0.0023)	0.0743*** (0.0027)	0.1332*** (0.0028)	0.1384*** (0.0028)	0.1685*** (0.0030)	0.1134*** (0.0018)
Observations	174,222	172,160	207,805	234,219	220,891	224,130	223,984	232,348	240,198	1,141,551
F-statistic	3	1	31	179	616	753	2213	2522	3146	3869
<i>Panel B: Nonwhites</i>										
Draft-eligibility effect	0.0031 (0.0076)	-0.0028 (0.0075)	0.0056 (0.0077)	0.0212*** (0.0072)	0.0327*** (0.0067)	0.0492*** (0.0067)	0.0893*** (0.0059)	0.0959*** (0.0060)	0.0964*** (0.0064)	0.0734*** (0.0028)
Observations	20,500	21,405	23,454	27,008	28,272	30,321	31,942	31,162	33,113	154,810
F-statistic	0	0	1	9	24	54	230	256	228	707

Notes: Results are based on restricted-use U.S. Census data from 2000. Estimates show the impact of draft eligibility on military service by birth cohort and race. Specifications are at the individual level, include month-by-year of birth fixed effects, cluster standard error estimates on lottery numbers, and are weighted using Census sampling weights.

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

Table 2
Estimated Incarceration Probabilities, Males Born 1948-1952

<i>Race:</i>	White		Nonwhite	
<i>Draft Eligibility:</i>	Eligible	Ineligible	Eligible	Ineligible
<i>Panel A: Aggregated Survey Waves</i>				
All Crime	0.00601 (0.0102)	0.00601 (0.0101)	0.0337 (0.0543)	0.0323 (0.0521)
Violent Crime	0.00240 (0.00548)	0.00209 (0.00512)	0.0174 (0.0376)	0.0161 (0.0352)
All Nonviolent Crime	0.00362 (0.00840)	0.00392 (0.00845)	0.0163 (0.0382)	0.0162 (0.0383)
Drug Crime	0.00202 (0.00678)	0.00211 (0.00683)	0.00637 (0.0276)	0.00638 (0.0277)
Property Crime	0.00333 (0.00764)	0.00323 (0.00739)	0.0200 (0.0420)	0.0192 (0.0410)
Public Order Crime	0.000769 (0.00348)	0.000798 (0.00358)	0.00429 (0.0204)	0.00350 (0.0184)
<i>Panel B: 1979 Survey</i>				
All Crime	0.00318 (0.00454)	0.00328 (0.00469)	0.0254 (0.0322)	0.0257 (0.0330)
Violent Crime	0.00150 (0.00314)	0.00124 (0.00288)	0.0123 (0.0221)	0.0130 (0.0234)
All Nonviolent Crime	0.00168 (0.00316)	0.00204 (0.00370)	0.0131 (0.0226)	0.0128 (0.0227)
Drug Crime	0.000309 (0.00140)	0.000304 (0.00138)	0.00182 (0.00855)	0.00137 (0.00752)
Property Crime	0.00143 (0.00300)	0.00133 (0.00298)	0.0129 (0.0237)	0.0133 (0.0242)
Public Order Crime	0.000249 (0.00127)	0.000177 (0.00115)	0.00158 (0.00850)	0.00127 (0.00724)
<i>Panel C: 1986 Survey</i>				
All Crime	0.00364 (0.00584)	0.00383 (0.00598)	0.0256 (0.0391)	0.0282 (0.0418)
Violent Crime	0.00225 (0.00463)	0.00197 (0.00423)	0.0161 (0.0315)	0.0175 (0.0329)
All Nonviolent Crime	0.00139 (0.00369)	0.00185 (0.00427)	0.00957 (0.0246)	0.0106 (0.0265)
Drug Crime	0.000362 (0.00174)	0.000576 (0.00236)	0.00208 (0.0117)	0.00238 (0.0125)
Property Crime	0.00196 (0.00429)	0.00211 (0.00439)	0.0158 (0.0312)	0.0175 (0.0334)
Public Order Crime	0.000561 (0.00234)	0.000590 (0.00236)	0.00302 (0.0145)	0.00296 (0.0140)
<i>Panel D: 1991 Survey</i>				
All Crime	0.0112 (0.0148)	0.0109 (0.0146)	0.0501 (0.0767)	0.0429 (0.0717)
Violent Crime	0.00344 (0.00755)	0.00305 (0.00712)	0.0238 (0.0519)	0.0178 (0.0455)
All Nonviolent Crime	0.00778 (0.0127)	0.00786 (0.0126)	0.0263 (0.0557)	0.0251 (0.0554)
Drug Crime	0.00538 (0.0108)	0.00546 (0.0108)	0.0152 (0.0443)	0.0154 (0.0444)
Property Crime	0.00658 (0.0115)	0.00626 (0.0110)	0.0314 (0.0597)	0.0267 (0.0570)
Public Order Crime	0.00150 (0.00533)	0.00163 (0.00553)	0.00827 (0.0306)	0.00626 (0.0275)

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

Table 3
 Estimated Effects of Draft Eligibility and Military Service on the Probability of Incarceration,
 White Males Born 1948–1952

<i>Survey Years:</i>	All	1979	1986	1991
	(1)	(2)	(3)	(4)
<i>Panel A: Incarceration for a Violent Crime</i>				
Estimated effect of eligibility	0.00030*	0.00038**	0.00016	0.00036
	(0.00016)	(0.00016)	(0.00023)	(0.00036)
TSIV estimated of effect of service	0.00269*	0.00340**	0.00145	0.00323
	(0.00142)	(0.00144)	(0.00204)	(0.00322)
Observations	5481	1827	1827	1827
<i>Panel B: Incarceration for a Nonviolent Crime</i>				
Estimated effect of eligibility	-0.00026	-0.00033*	-0.00053***	0.00009
	(0.00024)	(0.00018)	(0.00019)	(0.00064)
TSIV estimated of effect of service	-0.00228	-0.00299*	-0.00469***	0.00084
	(0.00211)	(0.00164)	(0.00172)	(0.00568)
Observations	5481	1827	1827	1827
<i>Panel C: Incarceration for Any Crime</i>				
Estimated effect of eligibility	0.00005	0.00005	-0.00036	0.00046
	(0.00028)	(0.00025)	(0.00030)	(0.00072)
TSIV estimated of effect of service	0.00041	0.00041	-0.00324	0.00407
	(0.00252)	(0.00226)	(0.00264)	(0.00641)
Observations	5481	1827	1827	1827

Notes: Reduced-form estimates use observations at the exact day of birth by survey year level. Incarceration data are from the 1979, 1986, and 1991 Surveys of Inmates in State and Federal Correctional Facilities and birth data are 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-variable estimates of the effect of military service on incarceration use the first-stage estimates shown in Table 1.

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

Table 4
 Estimated Effects of Draft Eligibility and Military Service on the Probability of Incarceration,
 Nonwhite Males Born 1948–1952

<i>Survey Years:</i>	All	1979	1986	1991
	(1)	(2)	(3)	(4)
<i>Panel A: Incarceration for a Violent Crime</i>				
Estimated effect of eligibility	0.00183* (0.00097)	-0.00058 (0.00114)	-0.00093 (0.00150)	0.00698*** (0.00247)
TSIV estimated of effect of service	0.02537* (0.01354)	-0.00799 (0.01582)	-0.01288 (0.02085)	0.09697*** (0.03434)
Observations	5481	1827	1827	1827
<i>Panel B: Incarceration for a Nonviolent Crime</i>				
Estimated effect of eligibility	0.00024 (0.00115)	0.00047 (0.00118)	-0.00029 (0.00134)	0.00055 (0.00293)
TSIV estimated of effect of service	0.00335 (0.01601)	0.00647 (0.01638)	-0.00400 (0.01867)	0.00759 (0.04068)
Observations	5481	1827	1827	1827
<i>Panel C: Incarceration for Any Crime</i>				
Estimated effect of eligibility	0.00207 (0.00156)	-0.00011 (0.00172)	-0.00121 (0.00191)	0.00753* (0.00388)
TSIV estimated of effect of service	0.02872 (0.02161)	-0.00152 (0.02387)	-0.01687 (0.02657)	0.10456* (0.05389)
Observations	5481	1827	1827	1827

Notes: See Table 3.

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

Table 5
 Estimated Effects of Draft Eligibility on the Probability of Incarceration (per 10,000),
 White Males Born 1948–1952,
 Narrow Crime Definitions

<i>Survey Years:</i>	All	1979	1986	1991
	(1)	(2)	(3)	(4)
<i>Panel A: Violent Crimes</i>				
Sex Crime	0.03374 (0.31716)	-0.13205 (0.22670)	0.03552 (0.51963)	0.19774 (0.76831)
Murder	0.40634 (0.34170)	0.17080 (0.34953)	0.41639 (0.50867)	0.63181 (0.84605)
Manslaughter	0.13382 (0.13928)	0.21524 (0.19748)	-0.10981 (0.28355)	0.29604 (0.24733)
Kidnapping	0.45351** (0.19080)	0.48381** (0.21809)	0.56226* (0.29373)	0.31447 (0.38196)
Extortion	0.01105 (0.06992)	-0.04555 (0.03297)	0.07837 (0.05609)	0.00034 (0.20128)
Robbery	0.85441* (0.45519)	0.81421* (0.43910)	0.63964 (0.56057)	1.10938 (1.09291)
Assault	0.10965 (0.26486)	0.67652** (0.31373)	-0.23495 (0.42690)	-0.11262 (0.61535)
<i>Panel B: Property Crimes</i>				
Burglary	-0.07211 (0.31622)	0.08748 (0.40238)	-0.97245* (0.57465)	0.66864 (0.67238)
Auto Theft	0.04796 (0.12995)	0.10163 (0.13440)	-0.10132 (0.14794)	0.14358 (0.33755)
Arson	0.06069 (0.15979)	0.06986 (0.10230)	-0.05977 (0.20011)	0.17199 (0.41651)
Fraud	0.07990 (0.19691)	0.13674 (0.23200)	0.06775 (0.26385)	0.03521 (0.45214)
Larceny	-0.11887 (0.19405)	0.17309 (0.21484)	-0.61080* (0.34528)	0.08111 (0.47256)
Stolen Property Offense	0.00665 (0.13211)	0.26884* (0.16185)	-0.43651 (0.26825)	0.18762 (0.23332)
Property Damage	-0.06099 (0.05184)	0.01960 (0.04740)	-0.21908 (0.13522)	0.01649 (0.06973)
Illegal Entry	-0.08197** (0.04055)	-0.00436 (0.04258)	-0.11659* (0.06728)	-0.12495 (0.09374)
<i>Panel C: Drug Crimes</i>				
Drug Trafficking	0.39876 (0.75063)	0.07963 (0.25094)	-0.34441 (0.36512)	1.46104 (2.18537)
Drug Possession	-0.03203 (0.43720)	0.03931 (0.20912)	-0.51008* (0.29064)	0.37467 (1.27514)

Notes: See Table 3.

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

Table 6
 Estimated Effects of Draft Eligibility on the Probability of Incarceration (per 10,000),
 Nonwhite Males Born 1948–1952,
 Narrow Crime Definitions

<i>Survey Years:</i>	All	1979	1986	1991
	(1)	(2)	(3)	(4)
<i>Panel A: Violent Crimes</i>				
Sex Crime	0.17018 (0.26033)	0.38616 (0.31434)	-0.51407 (0.44293)	0.63844 (0.57886)
Murder	0.17532 (0.31573)	-0.12156 (0.39409)	-0.20916 (0.55667)	0.85668 (0.75920)
Manslaughter	0.06989 (0.15359)	-0.07122 (0.12900)	-0.01048 (0.27544)	0.29136 (0.35614)
Kidnapping	0.17064 (0.18084)	0.07405 (0.15431)	-0.04711 (0.21741)	0.48497 (0.48335)
Extortion	-0.00447 (0.05761)	0.06481 (0.04665)	-0.08023 (0.05642)	0.00200 (0.15825)
Robbery	0.78956 (0.52098)	-0.66841 (0.57406)	-0.60538 (0.70337)	3.64246*** (1.33056)
Assault	-0.02074 (0.26517)	-0.07555 (0.30925)	0.49501 (0.46501)	-0.48168 (0.57096)
<i>Panel B: Property Crimes</i>				
Burglary	0.84968** (0.33497)	0.87866* (0.46072)	1.04852* (0.61290)	0.62186 (0.61581)
Auto Theft	0.06487 (0.13398)	0.07476 (0.11717)	0.06209 (0.11602)	0.05776 (0.37490)
Arson	0.00660 (0.13530)	-0.02283 (0.07513)	0.11411 (0.12706)	-0.07148 (0.38480)
Fraud	-0.18897 (0.19793)	-0.19071 (0.18708)	-0.77946*** (0.28498)	0.40326 (0.49588)
Larcency	-0.18514 (0.23576)	0.04169 (0.28554)	-0.23127 (0.45778)	-0.36585 (0.48803)
Stolen Property Offense	0.25446* (0.14225)	0.10392 (0.14835)	0.02929 (0.22070)	0.63017* (0.34554)
Property Damage	-0.02929 (0.05190)	-0.02694 (0.03873)	0.02904 (0.07813)	-0.08998 (0.13224)
Illegal Entry	0.03374 (0.07518)	0.07862 (0.09159)	-0.04960 (0.20067)	0.07222 (0.07176)
<i>Panel C: Drug Crimes</i>				
Drug Trafficking	0.04325 (0.45920)	0.07496 (0.25159)	0.20255 (0.33590)	-0.14777 (1.37148)
Drug Possession	0.12018 (0.28846)	0.19166 (0.17275)	-0.14244 (0.24559)	0.31132 (0.81631)

Notes: See Table 3.

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

Table 7
Robustness Check Applying Lotteries to Unaffected Cohorts
Estimated Effects of Draft Eligibility Placebo on the Probability of Incarceration

Cohort's Lottery Applied: Cohorts Used In Analysis:	1944-1950		1951		1952								
	1942, 1943, 1951-1959	1942-1950, 1952-1959	1942-1950, 1952-1959	All	1942-1951, 1953-1959	1991							
Survey Years:	All (1)	All (2)	All (3)	All (4)	All (5)	All (6)	All (7)	All (8)	All (9)	All (10)	All (11)	All (12)	
<i>Panel A: Incarceration for a Violent Crime, White Males</i>													
Est. effect of eligibility	-0.00012 (0.00011)	-0.00004 (0.00011)	-0.00001 (0.00015)	-0.00031 (0.00025)	0.00000 (0.00008)	-0.00001 (0.00008)	-0.00001 (0.00008)	-0.00012 (0.00013)	0.00014 (0.00021)	0.00008 (0.00010)	-0.00000 (0.00009)	0.00020 (0.00015)	0.00004 (0.00024)
Observations	12051	4017	4017	4017	18615	6205	6205	6205	6205	18624	6208	6208	6208
<i>Panel B: Incarceration for a Nonviolent Crime, White Males</i>													
Est. effect of eligibility	-0.00005 (0.00015)	-0.00009 (0.00013)	-0.00005 (0.00016)	-0.00001 (0.00042)	-0.00001 (0.00012)	-0.00001 (0.00010)	0.00019 (0.00012)	-0.00022 (0.00034)	-0.00004 (0.00014)	-0.00004 (0.00014)	-0.00009 (0.00011)	0.00005 (0.00014)	-0.00009 (0.00038)
Observations	12051	4017	4017	4017	18615	6205	6205	6205	18624	6208	6208	6208	6208
<i>Panel C: Incarceration for a Violent Crime, Nonwhite Males</i>													
Est. effect of eligibility	-0.00045 (0.00066)	0.00048 (0.00069)	0.00104 (0.00114)	-0.00288* (0.00155)	0.00047 (0.00059)	0.00061 (0.00067)	0.00013 (0.00092)	0.00066 (0.00130)	-0.00051 (0.00060)	-0.00014 (0.00061)	0.00019 (0.00097)	0.00019 (0.00097)	-0.00157 (0.00131)
Observations	12051	4017	4017	4017	18615	6205	6205	6205	18624	6208	6208	6208	6208
<i>Panel D: Incarceration for a Nonviolent Crime, Nonwhite Males</i>													
Est. effect of eligibility	-0.00011 (0.00074)	-0.00037 (0.00070)	0.00052 (0.00087)	-0.00049 (0.00190)	-0.00100 (0.00061)	-0.00028 (0.00061)	0.00006 (0.00069)	-0.00278* (0.00156)	0.00050 (0.00070)	-0.00033 (0.00064)	0.00027 (0.00074)	0.00027 (0.00074)	0.00155 (0.00185)
Observations	12051	4017	4017	4017	18615	6205	6205	6205	18624	6208	6208	6208	6208

Notes: See Table 3.

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

Table 8
Robustness Check Using Nonbinding Lotteries for 1953-56 Birth Cohorts
Estimated Effects of Draft Eligibility on the Probability of Incarceration

Highest APN Applied: Survey Years:	95					125					195					
	All (1)	1979 (2)	1986 (3)	1991 (4)	All (5)	1979 (6)	1986 (7)	1991 (8)	All (9)	1979 (10)	1986 (11)	1991 (12)	All (9)	1979 (10)	1986 (11)	1991 (12)
<i>Panel A: Incarceration for a Violent Crime, White Males</i>																
Est. effect of eligibility	-0.00002 (0.00020)	0.00014 (0.00018)	0.00005 (0.00031)	-0.00025 (0.00043)	-0.00022 (0.00018)	-0.00000 (0.00017)	0.00002 (0.00028)	-0.00068* (0.00040)	0.00008 (0.00017)	0.00008 (0.00016)	0.00017 (0.00025)	0.00001 (0.00040)	0.00008 (0.00017)	0.00008 (0.00016)	0.00017 (0.00025)	0.00001 (0.00040)
Observations	4383	1461	1461	1461	4383	1461	1461	1461	4383	1461	1461	1461	4383	1461	1461	1461
<i>Panel B: Incarceration for a Nonviolent Crime, White Males</i>																
Est. effect of eligibility	0.00033 (0.00031)	-0.00022 (0.00023)	0.00043 (0.00031)	0.00079 (0.00080)	0.00041 (0.00027)	-0.00019 (0.00021)	0.00042 (0.00027)	0.00100 (0.00071)	0.00049** (0.00025)	0.00009 (0.00020)	0.00028 (0.00024)	0.00109* (0.00066)	0.00049** (0.00025)	0.00009 (0.00020)	0.00028 (0.00024)	0.00109* (0.00066)
Observations	4383	1461	1461	1461	4383	1461	1461	1461	4383	1461	1461	1461	4383	1461	1461	1461
<i>Panel C: Incarceration for a Violent Crime, Nonwhite Males</i>																
Est. effect of eligibility	0.00142 (0.00132)	-0.00088 (0.00145)	0.00130 (0.00197)	0.00386 (0.00306)	0.00094 (0.00119)	-0.00040 (0.00139)	0.00164 (0.00179)	0.00157 (0.00272)	-0.00075 (0.00107)	-0.00087 (0.00129)	0.00140 (0.00172)	-0.00276 (0.00244)	-0.00075 (0.00107)	-0.00087 (0.00129)	0.00140 (0.00172)	-0.00276 (0.00244)
Observations	4383	1461	1461	1461	4383	1461	1461	1461	4383	1461	1461	1461	4383	1461	1461	1461
<i>Panel D: Incarceration for a Nonviolent Crime, Nonwhite Males</i>																
Est. effect of eligibility	0.00067 (0.00129)	0.00214 (0.00154)	-0.00103 (0.00149)	0.00090 (0.00328)	0.00105 (0.00122)	0.00134 (0.00134)	-0.00099 (0.00139)	0.00280 (0.00328)	0.00000 (0.00116)	0.00168 (0.00118)	0.00082 (0.00138)	-0.00249 (0.00296)	0.00000 (0.00116)	0.00168 (0.00118)	0.00082 (0.00138)	-0.00249 (0.00296)
Observations	4383	1461	1461	1461	4383	1461	1461	1461	4383	1461	1461	1461	4383	1461	1461	1461

Notes: See Table 3.

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

Table 9
 Analysis Using NCRP Prison Admissions Data
 Estimated Effects of Draft Eligibility and Military Service on Incarceration

Years:	1983	1984	1985	1986	1987	1988	1989	1990	1991
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
<i>Panel A: Incarceration for a Violent Crime, White Males</i>									
Est. effect of eligibility per 10,000	0.150 (0.134)	-0.034 (0.138)	0.144 (0.140)	-0.142 (0.140)	-0.017 (0.129)	-0.092 (0.147)	-0.304** (0.147)	0.024 (0.138)	0.144 (0.134)
Observations	1827	1827	1827	1827	1827	1827	1827	1827	1827
<i>Panel B: Incarceration for a Nonviolent Crime, White Males</i>									
Est. effect of eligibility per 10,000	0.003 (0.191)	0.266 (0.176)	0.237 (0.193)	0.158 (0.236)	-0.390* (0.235)	-0.304 (0.252)	-0.067 (0.232)	0.010 (0.272)	-0.092 (0.240)
Observations	1827	1827	1827	1827	1827	1827	1827	1827	1827
<i>Panel C: Incarceration for a Violent Crime, Nonwhite Males</i>									
Est. effect of eligibility per 10,000	-0.258 (0.779)	-0.520 (0.851)	-0.259 (0.975)	-1.287 (0.934)	-1.269 (0.893)	0.618 (1.034)	-0.708 (0.835)	-0.611 (0.879)	1.277 (0.862)
Observations	1827	1827	1827	1827	1827	1827	1827	1827	1827
<i>Panel D: Incarceration for a Nonviolent Crime, Nonwhite Males</i>									
Est. effect of eligibility per 10,000	-1.459 (1.122)	1.161 (1.222)	0.523 (1.382)	0.315 (1.446)	-1.468 (1.461)	-0.195 (1.818)	-0.138 (1.613)	2.181 (1.791)	0.867 (1.595)
Observations	1827	1827	1827	1827	1827	1827	1827	1827	1827

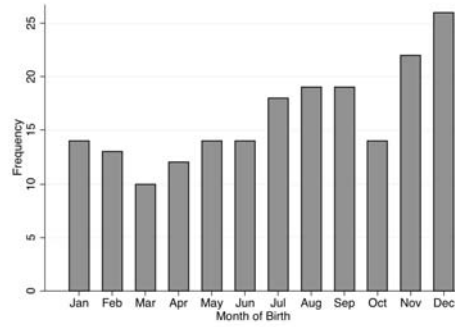
Notes: NCRP prison admissions data is restricted to individuals who are admitted due to a court commitment. The analysis is conducted in the manner described in Table 3.

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

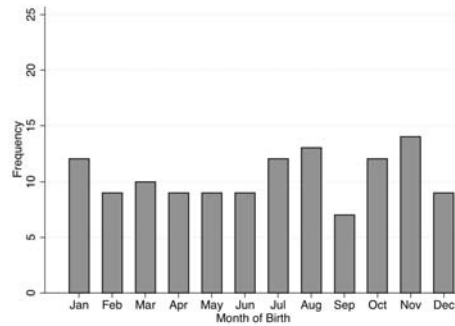
Appendix 1: Additional Figures and Tables

Figure A1
Lottery Numbers Draft Eligible By Birth Month

1969 Lottery



1970 Lottery



1971 Lottery

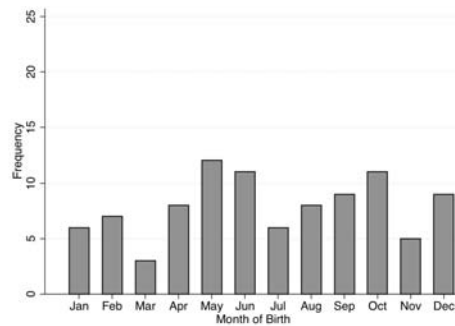


Figure A2
Probability of Incarceration for a Violent Crime by Draft Eligibility, White Males

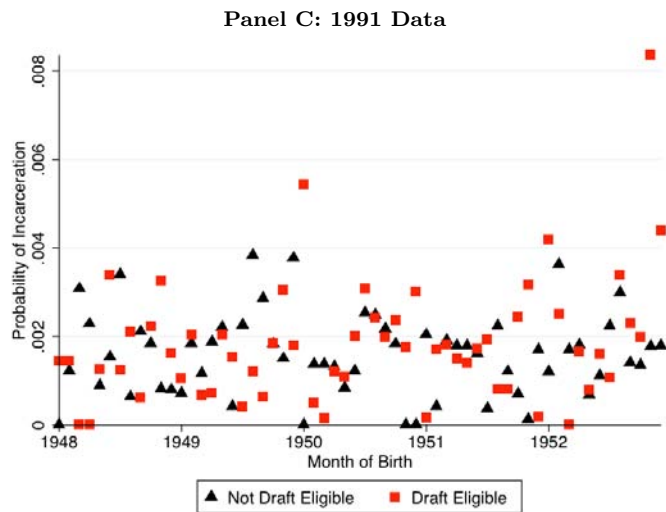
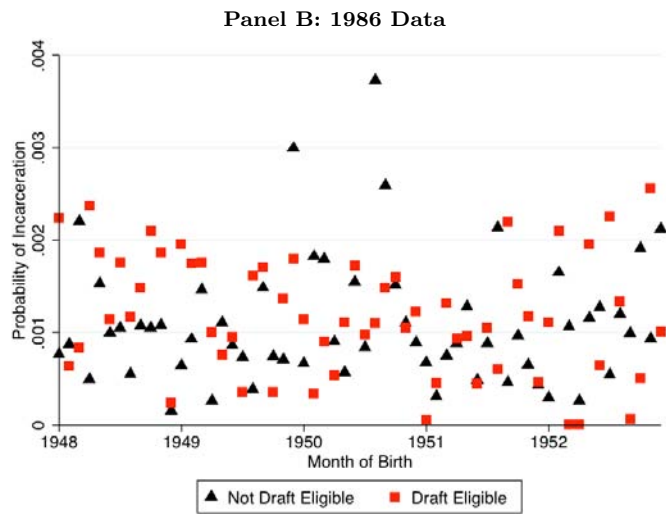
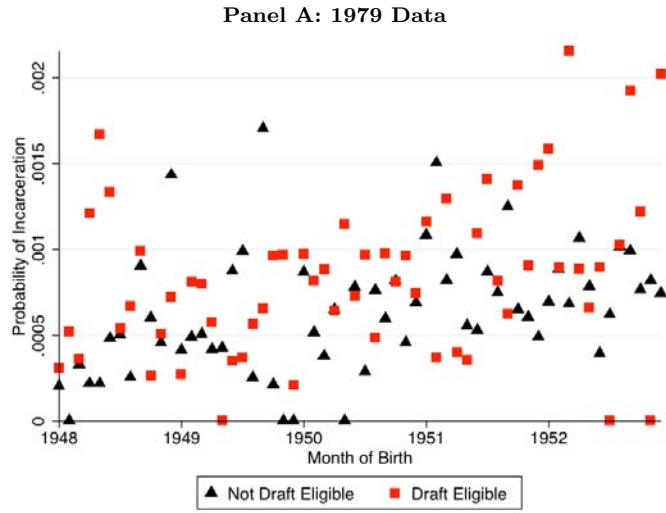


Figure A3
Probability of Incarceration for a Nonviolent Crime by Draft Eligibility, White Males

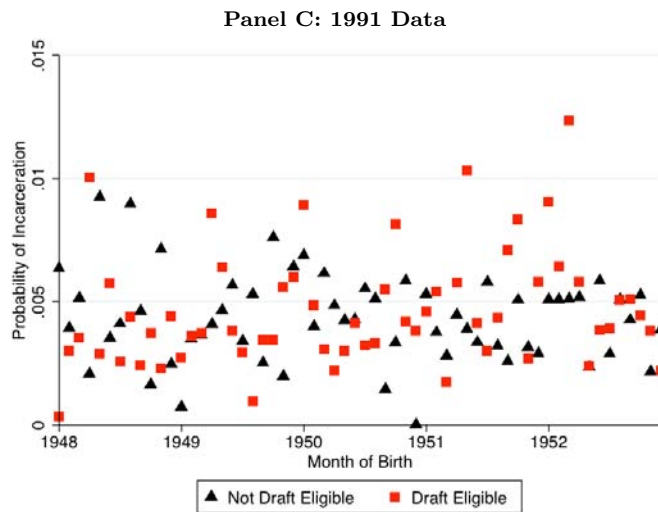
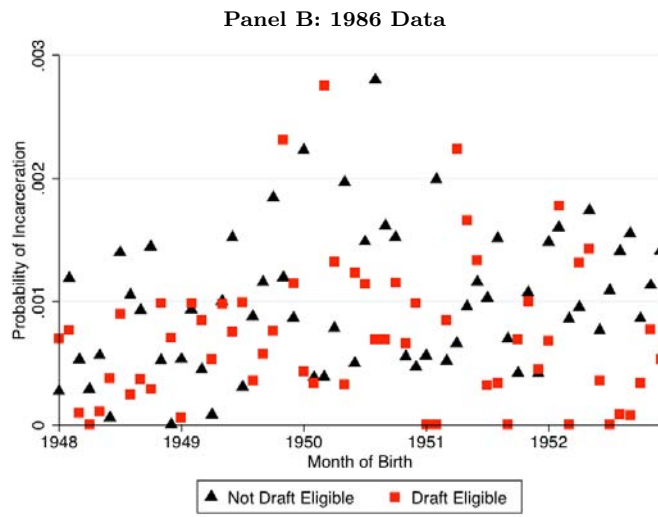
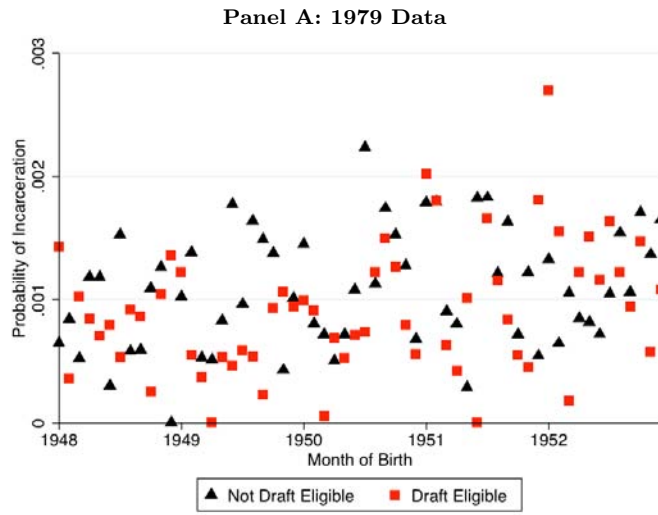


Figure A4
Probability of Incarceration for a Violent Crime by Draft Eligibility, Nonwhite Males

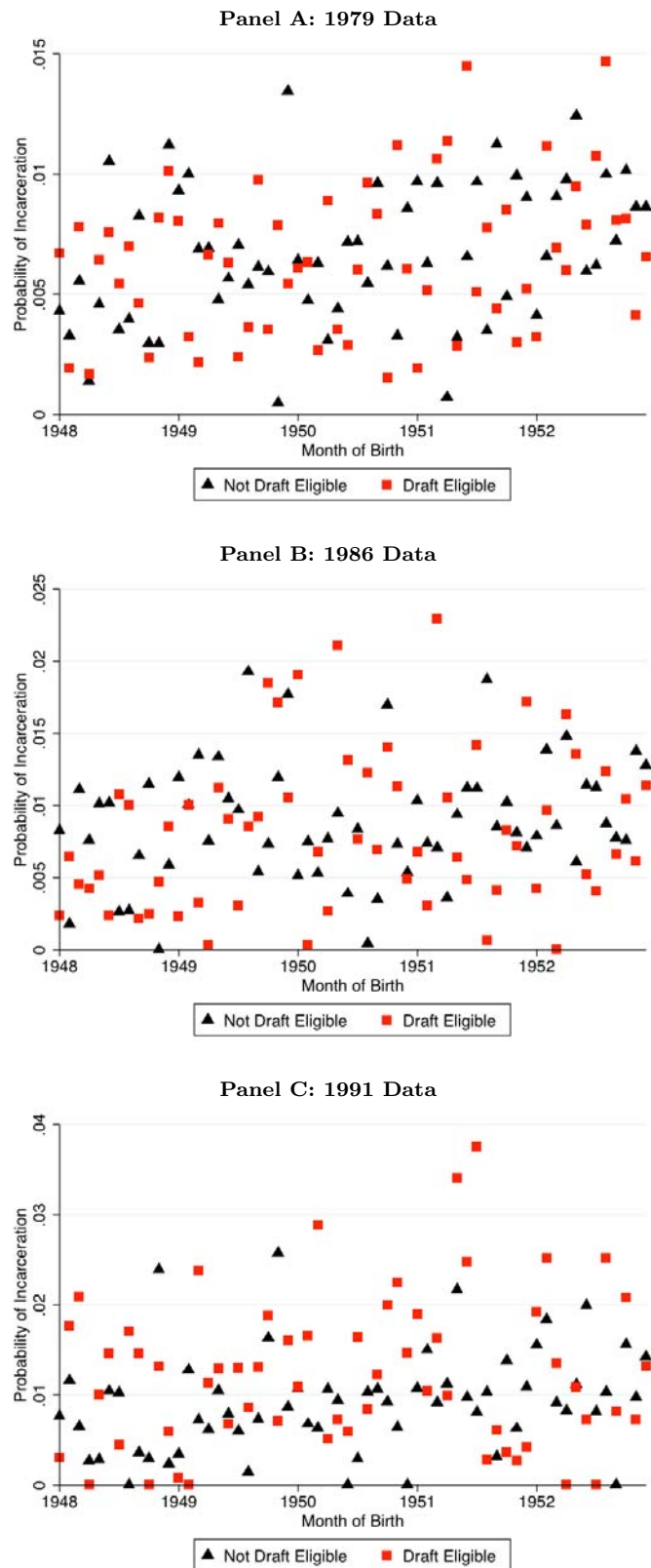


Figure A5
Probability of Incarceration for a Nonviolent Crime by Draft Eligibility, Nonwhite Males

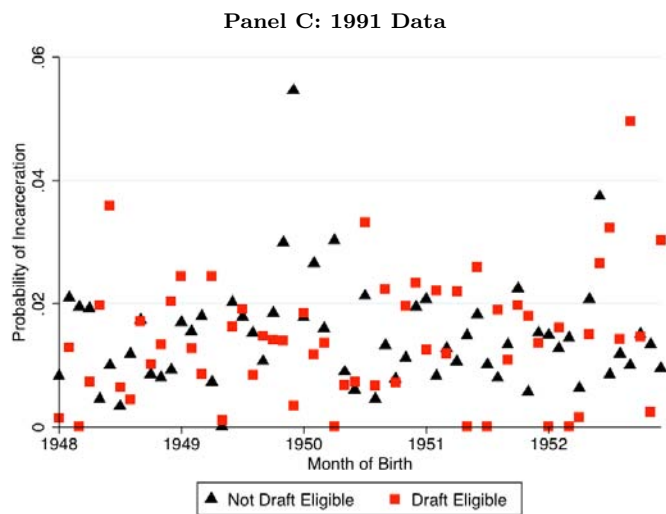
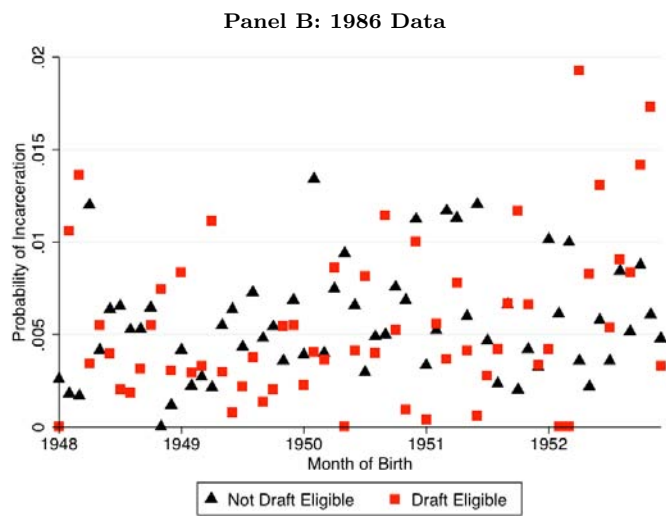
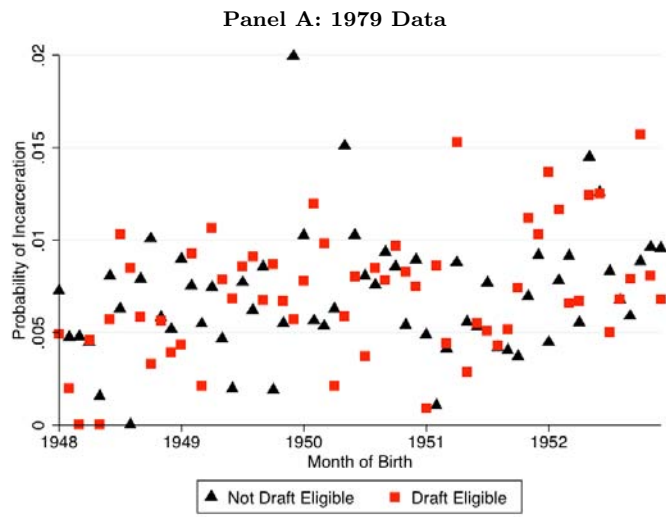


Table A1
Mean Incarceration Rates (Per 10,000), Males Born 1948-1950

<i>Race:</i>	White		Nonwhite	
<i>Draft Eligibility:</i>	Eligible	Ineligible	Eligible	Ineligible
<i>Panel A: Aggregated Survey Waves</i>				
All Crime	0.00581 (0.0102)	0.00605 (0.0104)	0.0319 (0.0534)	0.0301 (0.0512)
Violent Crime	0.00237 (0.00552)	0.00205 (0.00529)	0.0167 (0.0378)	0.0139 (0.0337)
All Nonviolent Crime	0.00344 (0.00838)	0.00400 (0.00893)	0.0152 (0.0370)	0.0162 (0.0398)
Drug Crime	0.00181 (0.00669)	0.00223 (0.00726)	0.00601 (0.0268)	0.00680 (0.0294)
Property Crime	0.00312 (0.00751)	0.00315 (0.00772)	0.0187 (0.0411)	0.0174 (0.0408)
Public Order Crime	0.000729 (0.00338)	0.000819 (0.00378)	0.00419 (0.0205)	0.00299 (0.0181)
<i>Panel B: 1979 Survey</i>				
All Crime	0.00281 (0.00417)	0.00281 (0.00445)	0.0239 (0.0318)	0.0237 (0.0331)
Violent Crime	0.00132 (0.00290)	0.000936 (0.00250)	0.0116 (0.0219)	0.0111 (0.0232)
All Nonviolent Crime	0.00149 (0.00296)	0.00188 (0.00358)	0.0123 (0.0222)	0.0126 (0.0232)
Drug Crime	0.000277 (0.00132)	0.000290 (0.00139)	0.00192 (0.00885)	0.00166 (0.00837)
Property Crime	0.00124 (0.00278)	0.000888 (0.00232)	0.0115 (0.0234)	0.0116 (0.0239)
Public Order Crime	0.000235 (0.00121)	0.000154 (0.00113)	0.00145 (0.00840)	0.000882 (0.00619)
<i>Panel C: 1986 Survey</i>				
All Crime	0.00385 (0.00608)	0.00392 (0.00609)	0.0243 (0.0388)	0.0259 (0.0425)
Violent Crime	0.00241 (0.00492)	0.00217 (0.00443)	0.0157 (0.0315)	0.0161 (0.0339)
All Nonviolent Crime	0.00144 (0.00371)	0.00175 (0.00420)	0.00854 (0.0232)	0.00984 (0.0255)
Drug Crime	0.000316 (0.00165)	0.000530 (0.00238)	0.00178 (0.0104)	0.00212 (0.0119)
Property Crime	0.00205 (0.00438)	0.00201 (0.00446)	0.0150 (0.0310)	0.0156 (0.0332)
Public Order Crime	0.000573 (0.00230)	0.000670 (0.00264)	0.00252 (0.0134)	0.00250 (0.0122)
<i>Panel D: 1991 Survey</i>				
All Crime	0.0108 (0.0149)	0.0114 (0.0151)	0.0477 (0.0754)	0.0406 (0.0694)
Violent Crime	0.00337 (0.00755)	0.00304 (0.00747)	0.0228 (0.0524)	0.0144 (0.0413)
All Nonviolent Crime	0.00740 (0.0128)	0.00838 (0.0134)	0.0249 (0.0540)	0.0262 (0.0584)
Drug Crime	0.00485 (0.0108)	0.00587 (0.0114)	0.0143 (0.0432)	0.0166 (0.0474)
Property Crime	0.00606 (0.0114)	0.00656 (0.0117)	0.0296 (0.0582)	0.0249 (0.0569)
Public Order Crime	0.00138 (0.00518)	0.00163 (0.00579)	0.00860 (0.0314)	0.00558 (0.0281)

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

Table A2
Mean Incarceration Rates (Per 10,000), Males Born 1951

<i>Race:</i>	White		Nonwhite	
<i>Draft Eligibility:</i>	Eligible	Ineligible	Eligible	Ineligible
<i>Panel A: Aggregated Survey Waves</i>				
All Crime	0.00623 (0.0102)	0.00545 (0.00933)	0.0346 (0.0568)	0.0320 (0.0515)
Violent Crime	0.00211 (0.00482)	0.00187 (0.00467)	0.0188 (0.0391)	0.0172 (0.0358)
All Nonviolent Crime	0.00412 (0.00851)	0.00358 (0.00757)	0.0157 (0.0379)	0.0148 (0.0360)
Drug Crime	0.00260 (0.00685)	0.00165 (0.00554)	0.00624 (0.0259)	0.00599 (0.0265)
Property Crime	0.00367 (0.00748)	0.00291 (0.00671)	0.0195 (0.0415)	0.0199 (0.0417)
Public Order Crime	0.000697 (0.00314)	0.000585 (0.00296)	0.00516 (0.0228)	0.00364 (0.0180)
<i>Panel B: 1979 Survey</i>				
All Crime	0.00383 (0.00484)	0.00385 (0.00524)	0.0244 (0.0309)	0.0236 (0.0302)
Violent Crime	0.00186 (0.00342)	0.00159 (0.00330)	0.0123 (0.0227)	0.0136 (0.0229)
All Nonviolent Crime	0.00197 (0.00347)	0.00226 (0.00410)	0.0121 (0.0209)	0.00996 (0.0190)
Drug Crime	0.000250 (0.00122)	0.000389 (0.00152)	0.00186 (0.00846)	0.00116 (0.00671)
Property Crime	0.00187 (0.00322)	0.00162 (0.00338)	0.0139 (0.0240)	0.0129 (0.0233)
Public Order Crime	0.000263 (0.00129)	0.000249 (0.00139)	0.00140 (0.00781)	0.00104 (0.00654)
<i>Panel C: 1986 Survey</i>				
All Crime	0.00308 (0.00516)	0.00310 (0.00557)	0.0250 (0.0353)	0.0289 (0.0400)
Violent Crime	0.00175 (0.00361)	0.00149 (0.00351)	0.0167 (0.0320)	0.0178 (0.0304)
All Nonviolent Crime	0.00133 (0.00354)	0.00161 (0.00411)	0.00832 (0.0201)	0.0111 (0.0302)
Drug Crime	0.000535 (0.00209)	0.000474 (0.00193)	0.00248 (0.0112)	0.00261 (0.0146)
Property Crime	0.00191 (0.00443)	0.00166 (0.00387)	0.0138 (0.0246)	0.0198 (0.0343)
Public Order Crime	0.000440 (0.00229)	0.000330 (0.00169)	0.00387 (0.0174)	0.00346 (0.0174)
<i>Panel D: 1991 Survey</i>				
All Crime	0.0118 (0.0146)	0.00940 (0.0134)	0.0543 (0.0834)	0.0435 (0.0726)
Violent Crime	0.00273 (0.00669)	0.00254 (0.00647)	0.0275 (0.0542)	0.0202 (0.0488)
All Nonviolent Crime	0.00907 (0.0125)	0.00687 (0.0111)	0.0268 (0.0574)	0.0233 (0.0502)
Drug Crime	0.00703 (0.0103)	0.00409 (0.00880)	0.0144 (0.0416)	0.0142 (0.0419)
Property Crime	0.00722 (0.0110)	0.00544 (0.00998)	0.0306 (0.0617)	0.0270 (0.0584)
Public Order Crime	0.00139 (0.00469)	0.00118 (0.00459)	0.0102 (0.0342)	0.00641 (0.0248)

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

Table A3
Mean Incarceration Rates (Per 10,000), Males Born 1952

<i>Race:</i>	White		Nonwhite	
<i>Draft Eligibility:</i>	Eligible	Ineligible	Eligible	Ineligible
<i>Panel A: Aggregated Survey Waves</i>				
All Crime	0.00690 (0.0104)	0.00641 (0.0102)	0.0427 (0.0552)	0.0362 (0.0539)
Violent Crime	0.00294 (0.00599)	0.00234 (0.00517)	0.0193 (0.0347)	0.0190 (0.0368)
All Nonviolent Crime	0.00395 (0.00839)	0.00407 (0.00832)	0.0233 (0.0442)	0.0172 (0.0378)
Drug Crime	0.00244 (0.00715)	0.00232 (0.00707)	0.00863 (0.0338)	0.00601 (0.0256)
Property Crime	0.00407 (0.00848)	0.00365 (0.00736)	0.0284 (0.0467)	0.0216 (0.0406)
Public Order Crime	0.00109 (0.00435)	0.000946 (0.00371)	0.00372 (0.0154)	0.00425 (0.0193)
<i>Panel B: 1979 Survey</i>				
All Crime	0.00445 (0.00579)	0.00360 (0.00454)	0.0350 (0.0347)	0.0310 (0.0347)
Violent Crime	0.00207 (0.00394)	0.00147 (0.00306)	0.0162 (0.0219)	0.0156 (0.0241)
All Nonviolent Crime	0.00238 (0.00373)	0.00213 (0.00355)	0.0188 (0.0260)	0.0154 (0.0245)
Drug Crime	0.000566 (0.00198)	0.000254 (0.00123)	0.00124 (0.00688)	0.00107 (0.00664)
Property Crime	0.00195 (0.00373)	0.00184 (0.00349)	0.0190 (0.0241)	0.0164 (0.0251)
Public Order Crime	0.000310 (0.00153)	0.000156 (0.000944)	0.00255 (0.00984)	0.00214 (0.00917)
<i>Panel C: 1986 Survey</i>				
All Crime	0.00316 (0.00521)	0.00430 (0.00609)	0.0341 (0.0446)	0.0314 (0.0421)
Violent Crime	0.00198 (0.00400)	0.00204 (0.00442)	0.0171 (0.0313)	0.0198 (0.0331)
All Nonviolent Crime	0.00118 (0.00382)	0.00225 (0.00450)	0.0170 (0.0347)	0.0116 (0.0249)
Drug Crime	0.000406 (0.00175)	0.000744 (0.00266)	0.00328 (0.0181)	0.00260 (0.0115)
Property Crime	0.00154 (0.00349)	0.00267 (0.00464)	0.0232 (0.0389)	0.0189 (0.0327)
Public Order Crime	0.000649 (0.00261)	0.000675 (0.00232)	0.00474 (0.0160)	0.00333 (0.0136)
<i>Panel D: 1991 Survey</i>				
All Crime	0.0131 (0.0144)	0.0113 (0.0147)	0.0588 (0.0749)	0.0463 (0.0750)
Violent Crime	0.00478 (0.00846)	0.00352 (0.00703)	0.0247 (0.0463)	0.0216 (0.0487)
All Nonviolent Crime	0.00830 (0.0124)	0.00782 (0.0124)	0.0341 (0.0620)	0.0247 (0.0546)
Drug Crime	0.00634 (0.0112)	0.00595 (0.0110)	0.0214 (0.0533)	0.0144 (0.0412)
Property Crime	0.00873 (0.0126)	0.00644 (0.0108)	0.0430 (0.0646)	0.0294 (0.0561)
Public Order Crime	0.00232 (0.00675)	0.00201 (0.00578)	0.00387 (0.0191)	0.00730 (0.0289)

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

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:

$$Births_{ym} = \alpha + \beta * WeekendDays_{ym} + v_y + \delta_m + \epsilon_{ym}. \quad (6)$$

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.