Conscription and Military Service: Do They Result in Future Violent and Non-Violent Incarcerations and Recidivism?[†]

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Abstract

Employing nonparametric bounds, we evaluate the validity of the Vietnam lottery draft as the Instrumental Variable (IV) for the Vietnam Era military service, and re-examine the effect of military service on future incarceration and recidivism outcomes. We allow the lottery draft to have a net (or direct) effect on the outcomes through channels other than its impact on military service, thereby disposing of the exclusion restriction (ER) assumption. Our estimated bounds suggest that the net effect of the lottery draft increases the incarceration and recidivism rate for violent offenses, implying that the ER assumption is not valid in this context. This net effect is especially potent among the 1950 birth cohort: the lottery draft eligibility directly increases their incarceration rates for violent crimes by at least 0.13 percentage points, and of violent recidivists

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(who had criminal justice contacts before the draft) by at least 0.08 percentage points. This conclusion is robust to the use of conservative multiple-testing procedures. For the effect of military service, our estimated bounds for those whose service is induced by the lottery draft (the "compliers") do not rule out a zero effect on their incarceration rate for violent or nonviolent crimes, which contrast to the statistically significant effects that are found when assuming the validity of the ER. Lastly, our estimated bounds for the subpopulation of volunteer Vietnam veterans (the "always takers"), which may be relevant to the current all-voluntary force veterans, suggest that military service has positive effects on the incarceration rate for violent and nonviolent crimes for volunteers in the 1951 and 1952 birth cohorts, whereas only the results for white always-takers remain statistically significant when using multiple-testing procedures. Further analysis of the average characteristics of these volunteers relative to other birth cohorts suggests possible channels for these effects. The results in this paper imply that the social cost of the violent and nonviolent incarcerations caused by Vietnam-era lottery draft and military service was at least $1.1 billion in 2016 U.S. dollars.

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1 Introduction

Applied researchers have used the Vietnam lottery draft as a source of exogenous variation—or an instrumental variable (IV)—for the Vietnam Era military service to estimate the effect of military service on a myriad of post service outcomes (e.g., Angrist, 1990; Dobkin and Shabani, 2009; Angrist and Chen, 2011; Heerwig and Conley, 2013). This approach is used to overcome the missing counterfactual outcome problem (e.g., Manski, 2008)—researchers cannot observe what the veterans' outcomes would have been had they not served in the military. This problem, which results in selection bias, is especially relevant as military enlistment is regularly a decision made by the individual. People with specific pre-induction characteristics correlated with future outcomes may be more inclined to join the military. As a result of this "self-selection", a simple comparison of post service outcomes between veterans and nonveterans does not, in general, reveal the true causal effect of military service on outcomes of interest.

In recent years, the causal effect of military service on post service crimes and incarcerations has attracted attention, likely sparked by increasing reports of violent crime offenses by veterans (e.g., Sontag and Alvarez, 2008). To understand this causal relationship, researchers have used the Vietnam lottery draft as an IV for military service (Lindo and Stoecker, 2014). In this context, self-selection arises because, e.g., individuals with specific pre-induction characteristics, such as higher tendencies toward delinquent behaviors (Teachman and Tedrow, 2014a) or higher tendencies towards violence (Sampson et al. 1997; Shihadeh and Flynn, 1996), may be more inclined to join the military. The Vietnam lottery draft generates exogenous variation in military service because the induction requirements were based on a Random Sequence Number (RSN) that was assigned to potential draftees based solely on birthdays, making it independent of their pre-induction characteristics. In the context of the U.S., Lindo and Stoecker (2014) used this approach and found that, for the demographic group of whites, military service increased violent crime incarcerations by 0.34 percentage points (hereafter p.p.) and decreased nonviolent crime incarceration by 0.30 p.p.

A critical assumption of the IV approach is that the lottery draft eligibility affects the incarceration outcomes exclusively through the mechanism (or indirect) channel of military service (induced by the draft-eligibility). This assumption is referred to as the exclusion restriction (ER). Unfortunately, the ER is likely not satisfied in this context. The leading factors for the potential net (or direct) effect of the draft-eligibility are draft avoidance behaviors. For example, Card and Lemieux (2001) documented that men who had high induction risks in the Vietnam Era had a strong incentive to use the education exemption and enroll in universities to avoid the draft. In this type of avoidance behavior, draft-eligible individuals would attain higher levels of schooling compared to the draft ineligibles. Additionally, Deuchert and Huber (2017) documented that individuals with low draft numbers attained more education in the four years following the draft relative to those with lower numbers, which is a time frame during which the higher educational attainment was less likely to be induced by the GI bill. Given the literature documenting negative correlations between education and crime (e.g., Lochner and Moretti, 2004), this avoidance behavior leads to a negative net effect (through channels other than military service) of the lottery draft on incarceration outcomes.

As another example of an avoidance behavior, Kuziemko (2010) suggested the notion of "dodging down" as an avoidance behavior consisting of delinquencies and criminal activities, because having a criminal record was another way to avoid being drafted into military service by disqualifying the military induction's "moral standards" (Suttler, 1970; Shapiro and Striker, 1970). Kuziemko (2010) documented that, for blacks, delinquency behaviors increased with lower lottery numbers (related to a higher likelihood of being drafted), while white's delinquency behaviors did not depend on lottery numbers. Besides these draft avoidance behaviors, receiving a low lottery number and refusing to serve could also lead to convictions of draft offenders and sentences to serve in prisons according to the draft law (Baskir and Strauss, 1978). At the same time, studies have found evidence that early incarcerations lead to increased recidivism probabilities later in life (Bayer et al., 2009; Aizer and Doyle, 2015), through channels such as adverse peer effects and lower human capital accumulation. Thus, if earlier years' incarcerations caused by the lottery draft could lead to later years' recidivism and compound over time, then the dodging down effect would result in a net effect of the lottery draft on future incarceration outcomes. Under violations of the ER, the IV estimate of the effect of military service will have a bias of the same sign as the net effect of the lottery draft eligibility, independently of the true sign of the effect of military service on incarceration outcomes. Lindo and Stoecker (2014) also recognize that the violation of the ER is one concern for the validity of their 2SLS estimates using the lottery draft as the IV for military service, and perform indirect assessment of this possibility.

In this paper, we employ recently developed nonparametric bounds in Flores and Flores-Lagunes (2013; hereafter FF-L) and Chen et al. (2017; hereafter CFF-L) to evaluate the validity of the lottery draft as an IV for military service and to reexamine the effect of military service on incarceration and recidivism outcomes. By allowing the lottery draft to have a net effect on the outcomes through channels other than its impact on military service, we are able to dispose of the ER assumption. Intuitively, the estimation methodology separates the total effect of the lottery draft on the outcomes of interest into a mechanism effect that works through the channel of the military service that is induced by the lottery draft, and a net effect that does not work through the military service. Given that the same assumptions as the conventional IV estimator are used, with the exception of the ER, if the estimated bounds on the direct effect exclude zero, they imply the failure of the ER. To reexamine the effect of military service on incarceration outcomes, we use the estimated bounds on the mechanism effect of the lottery draft—the only effect assumed to exist under the ER assumption—and estimate bounds on the same effect estimated by the conventional IV estimator. For this effect, though, the bounds we employ require replacing the ER with a mean weak monotonicity assumption that we describe and justify in detail later.

Since individual-level incarceration data that is representative of the U.S. male population containing information necessary to determine lottery draft eligibility is unavailable, we construct population-level incarceration outcomes following the clever approach of Lindo and Stoecker (2014). We do this by combining inmate counts using the Survey of Inmates in State and Federal Correctional Facilities (SISFCF) 1979, 1986, and 1991, with a special version of the 1982-1996 National Health Interview Survey (NHIS) and birth statistics from the Vital Statistics of the United States (VSUS) 1948-1952.

Our estimated bounds and their 95% confidence intervals exclude a zero net effect of the lottery draft on violent incarceration and recidivism outcomes in the sample of nonwhites born in 1950 and in the pooled sample of whites and nonwhites. The effect in the pooled sample is robust to employing multiple testing procedures (i.e., Family-wise Error Rate or FWER, and False Discovery Rate or FDR) across the five birth cohorts in our study (males born in 1948, 1949, 1950, 1951 and 1952) at the significance level of 5% for violent crimes and 10% for violent recidivism. These results suggest the invalidity of the lottery draft as an IV for military service during the Vietnam War for future violent incarceration and recidivism outcomes, casting doubt on the validity of the ER in this setting.

As for the incarceration and recidivism effects of military service for violent or nonviolent crimes by whites or nonwhites whose military service is induced by the lottery draft eligibility (i.e., the compliers), our estimated bounds include zero at conventional statistical levels. One interpretation of these results is that the data and our bounding approach is not able to uncover causal effects of military service on those outcomes. This stands in contrast with the statistically significant effects found when assuming the validity of the ER.

Lastly, looking at the group of individuals who will always serve in the military regardless of their draft-eligibility (i.e., always takers or volunteers), a group that may be informative about the current U.S. all-volunteer forces (AVF), our estimated bounds indicate that military service increases the violent and nonviolent incarceration rates of both white and nonwhite volunteers in the 1951 and 1952 birth cohort. However, only the crime instigation effects for whites remain significant when using multiple-testing procedures across the 5 birth cohorts under study. An analysis of pre-draft average characteristics of volunteers from the different cohorts suggests that drug use, low socioeconomic status, and a disadvantaged family background may have heightened the instigation effects of military service on incarceration rates.

This paper offers a number of contributions. First, it complements the literature on the consequences of the lottery draft during the Vietnam era, in particular on draft avoidance behaviors. Card and Lemieux (2001) and Kuziemko (2010) focused on the schooling and incarceration effects of the lottery draft on the subject cohorts soon after the draft, that is, when the individuals were in their early twenties. We show evidence that the lottery draft may have increased the long-term incarceration for violent crimes 8 to 22 years after the draft, and, importantly, that this effect is separate from actual military service. Specifically, we find that draft eligibility directly increases the violent crime incarceration of white and nonwhite males born in 1950 by at least 0.13 p.p., and increases the nonviolent crime incarceration of nonwhite males born in 1950 by at least 0.39 p.p.—increases of 40.4% and 37.9%, respectively, relative to the mean incarceration of ineligible-to-draft nonveterans—as well as the violent crime incarceration of the 1950 born white and nonwhite males who had criminal justice contact before the age of 18-20 by at least 0.08 p.p.—an increase of 56.9% relative to the mean incarceration of ineligible-to-draft nonveterans who had criminal justice contact before the same age.

Second, the paper contributes to the analysis of the crime and incarceration effects of the Vietnam era military service (e.g., Lindo and Stoecker, 2014; Bouffard, 2014; Teachman and Tedrow, 2014b). In particular, we use the lottery draft to control for self-selection into military service while separating the net effect of the lottery draft from the military service effect. Also, we concentrate on long-term incarceration and recidivism effects 8 to 22 years after the lottery draft. The analysis of Vietnam veterans can be relevant to contemporary policy discussions for at least two reasons. The first reason is that the nature of the likely channels that impact the criminality of veterans in the Vietnam era, such as the detrimental effect of combat exposure on veteran's mental health (Rohlfs, 2010), and the trainings that aim to desensitize the soldiers to violence (Grossman, 2009) are similar in today's military service. Indeed, one potential factor behind the recent veteran crimes that have generated policy discussions, the Post-Traumatic Stress Disorder (PTSD), is estimated to have similar prevalence in Vietnam veterans and in Operation Enduring Freedom/Operation Iraqi Freedom veterans.[1] The second reason is that our approach allows us to analyze a latent subpopulation of individuals who will always serve in the military regardless of

[1] The prevalence of PTSD among Vietnam veterans is estimated at 15.2% in 1986-1988 (Kulka et al., 1990), while the prevalence among the Operation Enduring Freedom/Operation Iraqi Freedom veterans is estimated at 13.8% in 2008 (Tanielian and Jaycox, 2008).

their lottery draft eligibility (the always takers). As this subpopulation consists of volunteers, lessons learned from them could be related to the current U.S. AVF.

Third, the results herein could be relevant to the growing literature that employs military drafts (or similar IVs) in other countries to analyze the effect of military service on crime and incarceration outcomes (e.g., Galiani et al. 2011, Albaek et al. 2016, Siminski et al. 2016, Hjalmarsson and Lindquist, 2016). In general, these studies reach inconclusive results in terms of the sign of the effect of military service on crime and incarceration. Of course, the potential relevance of our findings to other countries has to be evaluated in light of the different institutional contexts and time periods.

Fourth, this paper contributes to the growing literature employing nonparametric bounds in IV models without the ER assumption (e.g., FF-L, 2013; Amin et al., 2016; CFF-L, 2017; Wang et al., 2017). In this regard, we illustrate how this approach can be applied to situations where individual-level data is not available and auxiliary data is employed to undertake statistical inference about the population of interest under a bounded outcome. Lastly, we contribute to the growing literature on statistical methodologies to test the assumptions underlying the conventional IV model with heterogeneous effects (FF-L, 2013; Mourifié and Wan, 2017; Kitagawa, 2015; Huber and Mellace, 2015) by empirically testing the validity of the ER in the current empirical context and controlling for multiple testing.

The rest of the paper is organized as follows. Section 2 offers a brief overview of the Vietnam-era lottery draft. Section 3 describes the nonparametric bounds that are used in the analysis, while Section 4 describes the data sources employed and provides details about the empirical strategy. Section 5 assesses a crucial assumption employed in some of the analysis and presents the main results. Section 6 discusses implications of our results and Section 7 concludes.

2 The Vietnam Lottery Draft

The Vietnam lottery draft was a method adopted during the Vietnam War to fairly allocate military services in the U.S. The Vietnam lottery draft spanned the years 1969-1972, with annual televised drawings conducted on December 1, 1969, July 1, 1970, and August 5, 1971. Each birth date within a year was randomly assigned a random sequence number (RSN). Males with low RSNs were first required to report for induction into the military. The government administration drafted men into military service in the order of the RSNs until the manpower requirements were met. The last lottery numbers called became the ex-post draft eligibility cut-offs. The birth cohorts that were covered in the three draft lotteries were males born between the years of 1944-1952. We follow the large literature on economic analyses of the lottery draft (e.g., Angrist, 1990; Angrist and Chen, 2011; Lindo and Stoecker, 2014) and focus on the 1948-1952 birth cohorts.[2]

Being draft eligible based on the RSNs does not necessarily lead to subsequent military inductions. On the one hand, males could volunteer to serve even when their lottery numbers had not been called; on the other hand, draft-eligible males were subjected to physical examinations and mental aptitude tests to determine their qualifications for military service. Furthermore, draft avoidance strategies such as purposefully failing these pre-induction examinations, obtaining "conscientious objector" status, committing crimes and failing the induction moral standards, or obtaining education deferments were documented to be effective ways for draft-eligible males to escape from the military induction in the Vietnam Era (Suttler, 1970; Shapiro and Striker, 1970; Baskir and Strauss, 1978).

[2] An important reason for the previous studies leaving out males from the 1944-1947 birth cohorts is that the effect of the draft eligibility on military service for them is small (e.g., Angrist and Chen, 2011). Another reason for us to leave them out is that many of the 1944-1947 born had been subjected to the local drafts during the Vietnam War when they were between the age of 18 ½ - 25, before the national lottery draft was implemented. Omitting these birth cohorts avoids potential contamination from the effects of the local drafts.

An issue related to the randomization mechanism of the RSNs in the 1969 lottery draft has been documented (Fienberg, 1971): men with birthdays in later months tended to be drafted (i.e., tended to receive lower lottery numbers) relative to men with birthdays in earlier months.[3] For this reason, the previous literature that employs the Vietnam lottery draft as an IV for military service use birth month-by-year indicators to account for this issue (e.g., Angrist and Chen, 2011; Lindo and Stoecker, 2014). Our methodology, presented below, will also account for this aspect.

3 Econometric Methods

We are interested in the estimation of the effects of military service on different incarceration and recidivism outcomes. To avoid selection bias, we will employ the Vietnam era lottery draft as an IV. However, we are particularly concerned with the validity of the exclusion restriction due to the previously documented factors that can result in net effects of the IV on incarceration and recidivism outcomes, rendering traditional IV estimators biased. For this reason, we adopt the nonparametric bounding techniques in FF-L (2013) and CFF-L (2017). These techniques allow the draft-eligibility IV to have a net effect on the outcomes of interest through channels other than military service, thereby disposing of the ER assumption. The technique in FF-L (2013) allows bounding the local average treatment effect (LATE) for the subpopulation whose enlistment is determined by the draft eligibility—which is the same parameter identified by traditional IV estimators—while the results in CFF-L (2017) allow bounding the LATE for the subpopulation of volunteers. A central idea in these bounding techniques is to separate the total, reduced-form effect of eligibility to draft on the outcomes of interest into a mechanism effect that

[3] Each birthday was coded onto capsules that were added sequentially, January through December, into a drawer. The problem consisted of insufficient mixing of the capsules to overcome the original month by month sequencing before placing them into a jar to perform the final drawings (Fienberg, 1971).

works through the channel of military service (i.e., the military service induced by the draft), and a net effect that does not work through the military service.

3.1 Basic Setup

To introduce the methodology, assume that we have a large random sample from the target population. For each unit *i*, define the Vietnam Era veteran status D_i (D_i =1 for veterans, D_i =0 for nonveterans) as a function of the exogenously assigned draft-eligibility Z_i (Z_i =1 for eligible, Z_i =0 for ineligible): D_{1i}, D_{0i}, where D_{1i} is the veteran status if the individual was eligible to draft, and D_{0i} is the veteran status if the individual was ineligible to draft. We partition the total population into four latent principal strata based on the values of the vector $\{D_{1i}, D_{0i}\}$ (Imbens and Angrist, 1994; Angrist et al., 1996). (1) never-takers (*nt*): individuals who are non-veterans either when eligible or ineligible to draft (D_{1i}=0, D_{0i} =0). If these individuals receive a low lottery number that is probable to be called for induction, they likely undertake strategic actions to avoid the draft.[4] (2) always-takers (*at*): individuals who, regardless of whether they are eligible to draft, will serve in the military (D_{1i}=1, D_{0i} =1). They can be seen as volunteers and they represent a group potentially relevant to the current AVF system. (3) compliers (*c*): individuals that will serve in the military only if their lottery number is called to enlist (D_{1i}=1, D_{0i} =0); and (4) *defiers* (*d*): individuals that will enlist when their lottery numbers are not called for induction, and will avoid enlistment if their lottery numbers are called (D_{1i}=0, D_{0i} =1). The relationship of these latent strata with the observed groups defined by the observed draft-eligibility and Vietnam-era veteran status is illustrated in Table 1.

[4] The *nt* also include individuals whose pre-draft characteristics (e.g., health) prevent them from passing the enlistment physical exams, regardless of draft eligibility.

Define the incarceration outcomes as Y_i (Y_i=1 for individuals incarcerated for a certain type of crime; Y_i=0 for individuals not incarcerated for that type of crime). The potential outcomes as a function of the exogenous draft-eligibility and the potential veteran statuses are denoted as $Y_i(z, D_{zi})$: $Y_i(1, D_{1i}) \equiv Y_i(1)$, $Y_i(0, D_{0i}) \equiv Y_i(0)$, $Y_i(0, D_{1i})$, and $Y_i(1, D_{0i})$. The first two are potential outcomes where the individual is eligible and ineligible to draft, respectively. The last two potential outcomes are counterfactual incarceration outcomes and are never observed in the data. The third potential outcome represents the counterfactual outcome where the individual is ineligible to draft but has the potential veteran status with the value it would have if he was eligible to draft. Analogously, the last potential outcome represents the counterfactual outcome where the individual is eligible to draft but has the potential veteran status with the value it would have if he was ineligible to draft. The last two potential outcomes will be employed to decompose the lottery draft's total effect into a mechanism and a net effect below. In what follows, we assume access to data on (Z_i, D_i, Y_i) where $D_i = Z_i D_{1i} + (1 - Z_i) D_{0i}$ and $Y_i = D_i Y_i(Z_i, 1) + (1 - D_i) Y_i(Z_i, 0)$ and, to simplify notation, we write the subscript i only when deemed necessary.

It is well known (Imbens and Angrist, 1994; Angrist et al., 1996) that traditional IV estimates of the effects of military service using the lottery draft as an IV identify the effect of military service on the outcome for the c stratum ($LATE_c$):

$$LATE_c \equiv E[Y(z, 1) - Y(z, 0) | D_1 - D_0 = 1]. \qquad (1)$$

Identification of $LATE_c$ by the IV estimator relies on four assumptions (Imbens and Angrist, 1994). The first assumption, A1, is the random assignment of the instrument Z (the lottery draft eligibility): $\{Y(1,1), Y(0,0), Y(0,1), Y(1,0), D(0), D(1)\}$ is independent of Z. The Vietnam lottery draft satisfies A1 by design, since the lottery numbers were assigned randomly based on birth dates. The second assumption, A2, is the non-zero average effect of the instrument on the treatment D

(veteran status): $E[D_1 - D_0] \neq 0$. A2 is satisfied given the documented positive and statistically significantly effect that the eligibility to draft had on the Vietnam veteran status (e.g., Angrist, 1990). The third assumption, A3, is the individual-level monotonicity of Z on D: $D_{1i} \geq D_{0i}$ for all i. A3 states that the lottery draft-eligibility weakly affects the veteran status in one direction, implying the nonexistence of the d stratum ($D_{0i} = 1, D_{1i} = 0$). A3 is typically justified on the grounds that it is hard to think that individuals who prefer enlistment when ineligible to draft would not prefer enlistment when they are eligible to draft. The last assumption, which is referred to as the exclusion restriction, ER, states that the lottery draft eligibility affects incarceration outcomes exclusively through military service: $Y_i(0, d) = Y_i(1, d)$ for all i. We interpret this assumption as ruling out a non-zero net effect of the eligibility to draft on incarceration outcomes. Given the earlier arguments about why the ER may not be satisfied in this setting, our methodology will discard this assumption, while maintaining A1 to A3.

3.2 Nonparametric Bounds Disposing of the Exclusion Restriction Assumption

Under Assumptions A1 to A3, the Average Treatment Effect (ATE) of the lottery draft on a given incarceration outcome, $E[Y(1) - Y(0)]$, can be divided into two parts (see FF-L, 2010 and references therein). The first, the net average treatment effect or $NATE^Z$, is the net effect of the lottery draft on incarceration that is the net of the effect that works through the military service:[5]

$$NATE^z = E[Y(1, D_z) - Y(0, D_z)], \text{ for } z = 0, 1. \qquad (2)$$

[5] Note that, although the literature also refers to the net average treatment effect as the "direct effect", this effect does not have to be "direct" in any sense – it may still affect the crime outcomes through channels such as draft avoidance behavior, only that these channels are independent of the actual military service.

The second part is the mechanism effect or mechanism average treatment effect ($MATE^Z$), which is the effect of the lottery draft on the incarceration outcome that works only through the military service mechanism:

$$MATE^z = E[Y(z, D_1) - Y(z, D_0)], \text{ for } z = 0, 1. \quad (3)$$

Note that the definition of these effects depends on the value of the instrument (Z=z), but this dependence can be easily averaged out since the probabilities $\Pr(Z = z)$ are point identified under A1. The conceptual diagram in Figure 1 illustrates the two effects, where the dashed line indicates the flow of $MATE^Z$ and the solid line indicates the flow of $NATE^Z$.

Importantly, note that the ER shuts down the $NATE^Z$ by assumption, that is, it rules out the relevance of any net mechanism for the lottery draft except the one working through military service. In turn, an interpretation of the $MATE^Z$ is that it represents the "good" part of the effect of the lottery draft on the incarceration outcome and can be used to identify the effect of military service in the IV framework. Indeed, FF-L (2013) and CFF-L (2017) show that $MATE^Z$ can be related to $LATE$. To see this, write $MATE^Z$ as follows:

$$\begin{aligned} MATE^Z &= E[Y(z, D_1) - Y(z, D_0)] \quad (4) \\ &= E\{[\mathrm{D}_1 - \mathrm{D}_0] \cdot [\mathrm{Y}(\mathrm{z}, 1) - \mathrm{Y}(\mathrm{z}, 0)]\} \\ &= \Pr(D_1 - D_0 = 1) \cdot \{\Pr(Z = 1) \cdot E[Y(1,1) - Y(1,0) | D_1 - D_0 = 1] \\ &\qquad + \Pr(Z = 0) \cdot E[Y(0,1) - Y(0,0) | D_1 - D_0 = 1]\} - \Pr(D_1 - D_0 = -1) \\ &\qquad \cdot \{\Pr(Z = 1) \cdot E[Y(1,1) - Y(1,0) | D_1 - D_0 = -1] \\ &\qquad + \Pr(Z = 0) \cdot E[Y(0,1) - Y(0,0) | D_1 - D_0 = -1]\}, \end{aligned}$$

for $z = 0, 1$.

In the second line of (4), $MATE^Z$ is written as the expected value of the product of the effect of the draft-eligibility on veteran status times the effect from a change in the veteran status on the crime outcome. The subsequent lines use iterated expectations to make explicit the dependence on the instrument exposure. Recalling that A3 rules out defiers, such that $\Pr(D_1 - D_0 = -1) = 0$, equation (4) can be related to a $LATE$ that depends on the exposure status to the instrument, which we denote as $LATE_c^Z$ (FF-L, 2013):

$$LATE_c^Z \equiv E[Y(z,1) - Y(z,0)|D_1 - D_0 = 1]$$

$$= E[Y(z,1) - Y(z,0)|c] = \frac{MATE^z}{E[D_1 - D_0]}, \text{ for } z = 0,1 \qquad (5)$$

Since the denominator in the last expression is point identified (it is the reduced form effect of the randomized draft-eligibility on military service), the bounds on $MATE^Z$ in FF-L (2010) can be employed to construct bounds on each of the $LATE_c^Z$ for $z=0,1$. Since it will be important to do statistical inference on $LATE_c$ to compare to the traditional IV estimator that identifies (1), we average out Z to obtain estimated bounds on $LATE_c$.

In addition to undertaking inference on $LATE_c^Z$ ($z=0,1$) and $LATE_c$, we are also interested in the effect of military service on incarceration outcomes for the *at* stratum ($LATE_{at}$), since this parameter can be informative about AVF veterans. This effect, which can be bounded as explained below, is expressed as follows:

$$LATE_{at}^Z \equiv E[Y(z,1) - Y(z,0)|at], \quad \text{for } z = 0,1 \ . \qquad (6)$$

The general approach to derive nonparametric bounds on $NATE^Z$ and $MATE^Z$ is to first derive nonparametric bounds on their "local" (strata) versions denoted by $LNATE_k^Z$ and $LMATE_k^Z$ with $k=\{at, c, nt\}$. For instance, $LNATE_{nt}^Z = E[Y(1,D_z) - Y(0,D_z)|nt]$ and $LMATE_{nt}^Z = E[Y(z,D_1) - Y(z,D_0)|nt]$,[6] with similar parameter definitions for the other strata. The bounds

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[6] Note that, since $D_1 = D_0$ for always-takers and never-takers, for them $Y(1) = Y(1,D_z)$ and $Y(0) = Y(0,D_z)$ where $z = 0,1$, $LMATE_k = 0$, and $LNATE_k^1 = LNATE_k^0 = E[Y(1) - Y(0)|k]$ for $k = at, nt$.

derived for these local effects can then be aggregated (using the estimated strata proportions) to obtain bounds on the population-level $NATE^Z$ and $MATE^Z$ (FF-L 2010), and on objects such as $LATE_c{}^Z$ (using equation (5)) and $LATE_{at}{}^Z$ (CFF-L, 2017).

More specifically, under A1-A3, bounds on the $LNATE_{nt}^Z$ and $LNATE_{at}^Z$ can be derived using "trimming bounds" (Lee, 2009; Zhang et al., 2008). To see this, note that A3 eliminates the *d* stratum, enabling point identification of the potential outcomes of the eligible-to-draft never-takers ($E[Y(1)|nt] = E[Y|Z = 1, D = 0]$) and of the ineligible-to-draft always-takers ($E[Y(0)|at] = E[Y|Z = 0, D = 1]$). This follows from Table 1 once the *d* stratum is eliminated. Additionally, the population proportions of the three strata, denoted as π_{at}, π_c and π_{nt}, are also identified (Imbens and Angrist, 1994): letting $p_{d|z} \equiv \Pr(D_i = d|Z_i = z)$ for $d, z = \{0,1\}$, then $\pi_{at} = p_{1|0}$, $\pi_c = (p_{1|1} - p_{1|0})$, and $\pi_{nt} = p_{0|1}$. Also note that, in the observed data, we cannot distinguish the never-takers from compliers when they are both ineligible-to-draft and did not serve in the Vietnam Era (the upper left cell in Table 1), or the always-takers from compliers when they are both eligible-to-draft and served in the Vietnam Era (the lower right cell in Table 1). For this reason, $E[Y(0)|nt]$, $E[Y(0)|c]$, $E[Y(1)|at]$, and $E[Y(1)|c]$ are not point identified, but trimming bounds can be obtained for these objects.

For illustration of the trimming bounds, consider $E[Y(0)|nt]$. The average outcome for the observed group with $\{Z = 0, D = 0\}$ can be written as a function of the average outcomes of the nt and *c* strata (Imbens and Rubin, 1997):

$$E[Y|Z = 0, D = 0] = \frac{\pi_{nt}}{\pi_{nt}+\pi_c} \cdot E[Y(0)|nt] + \frac{\pi_c}{\pi_{nt}+\pi_c} \cdot E[Y(0)|c] \qquad (7)$$

Having two unknowns ($E[Y(0)|nt]$ and $E[Y(0)|c]$), the potential outcome $E[Y(0)|nt]$ can be bounded from above by the expected value of the $\frac{\pi_{nt}}{\pi_{nt}+\pi_c} = p_{0|1}/p_{0|0}$ fraction of the largest values of Y in the observed group with $\{Z = 0, D = 0\}$. Similarly, a lower bound on $E[Y(0)|nt]$ is constructed by using the same fraction of smallest values. The bounds on $E[Y(1)|at]$, $E[Y(0)|c]$, and $E[Y(1)|c]$ are similarly obtained using the appropriate observed group. With all the components in $LNATE_{nt}^{Z}$ and $LNATE_{at}^{Z}$ either point identified or bounded, bounds for these two local effects are obtained (FF-L, 2010).

To formally provide the expression for the bounds on $LNATE_{nt}^{Z}$ and $LNATE_{at}^{Z}$ under A1 to A3, denote y_{τ}^{zd} as the τ-th quantile of *Y* conditional on Z=*z* and D=*d*. The following proposition, adapted from FF-L (2010), presents the expressions of bounds on the objects necessary to bound $LNATE_{nt}^{Z}$ and $LNATE_{at}^{Z}$.

Proposition 1. If Assumptions A1-A3 hold, then $L^{nt} \leq LNATE_{nt}^{Z} \leq U^{nt}$ and $L^{at} \leq LNATE_{at}^{Z} \leq U^{at}$, $z = 0, 1$, where

$$L^{nt} = E[Y|Z = 1, D = 0] - U^{0,nt}; \qquad U^{nt} = E[Y|Z = 1, D = 0] - L^{0,nt}$$

$$L^{0,nt} = E\left[Y \middle| Z = 0, D = 0, Y \leq y^{00}_{\left(\frac{p_{0|1}}{p_{0|0}}\right)}\right]; \quad U^{0,nt} = E\left[Y \middle| Z = 0, D = 0, Y \geq y^{00}_{1-\left(\frac{p_{0|1}}{p_{0|0}}\right)}\right]$$

$$L^{at} = L^{1,at} - E[Y|Z = 0, D = 1]; \qquad U^{at} = U^{1,at} - E[Y|Z = 0, D = 1]$$

$$L^{1,at} = E\left[Y \middle| Z = 1, D = 1, Y \leq y^{11}_{\left(\frac{p_{1|0}}{p_{1|1}}\right)}\right]; \quad U^{1,at} = E\left[Y \middle| Z = 1, D = 1, Y \geq y^{11}_{1-\left(\frac{p_{1|0}}{p_{1|1}}\right)}\right]$$

Furthermore, we have: $L^{0,nt} \leq E[Y(0)|nt] \leq U^{0,nt}$; $L^{1,at} \leq E[Y(1)|at] \leq U^{1,at}$; $L^{0,c} \leq E[Y(0)|c] \leq U^{0,c}$ and $L^{1,c} \leq E[Y(1)|c] \leq U^{1,c}$; where

$$L^{0,c} = E\left[Y \middle| Z = 0, D = 0, Y \leq y^{00}_{1-\left(\frac{p_{0|1}}{p_{0|0}}\right)}\right]; \quad U^{0,c} = E\left[Y \middle| Z = 0, D = 0, Y \geq y^{00}_{\left(\frac{p_{0|1}}{p_{0|0}}\right)}\right]$$

$$L^{1,c} = E\left[Y \middle| Z = 1, D = 1, Y \leq y^{11}_{1-\left(\frac{p_{1|0}}{p_{1|1}}\right)}\right]; \quad U^{1,c} = E\left[Y \middle| Z = 1, D = 1, Y \geq y^{11}_{\left(\frac{p_{1|0}}{p_{1|1}}\right)}\right].$$

Proof. See FF-L (2010).

It is important to note that the bounds for $LNATE_{nt}^{Z}$ and $LNATE_{at}^{Z}$ rely on the same assumptions as the traditional IV estimates minus the ER assumption. Thus, maintaining A1-A3, if the estimated bounds for $LNATE_{nt}^{Z}$ or $LNATE_{at}^{Z}$ exclude zero, this provides statistical evidence on the existence of net effects of the lottery draft on incarceration outcomes for those subpopulations. In turn, given that the ER assumption must hold for every unit in the population, this implies the invalidity of the assumption (FF-L, 2013), that is, the invalidity of using the lottery draft as an IV for military service in the context of incarceration outcomes. Related work that proposes statistical tests for implications of assumptions A1 to A4 are Kitagawa (2015), Mourifié and Wan (2017), and Huber and Mellace, (2015).

Additional assumptions are needed to construct bounds on $LATE_c$ and $LATE_{at}$. The reason is that $Y(1, D_0)$ and $Y(0, D_1)$ for *c*, $Y(z, 0)$ for *at*, and $Y(z, 1)$ for *nt* are never observed in the data. The bounds for the corresponding expectations to these potential or counterfactual outcomes can be constructed under combinations of the following two assumptions employed in prior literature (see, e.g., FF-L 2010; FF-L, 2013; Huber et al., 2017; CFF-L, 2017; CFF-L, 2018). The first assumption is that the outcome is bounded, thus providing a natural bound for the expectations. The second imposes weakly monotonic relationships of average potential outcomes across strata that share the same draft eligibility.[7] More formally, the additional assumptions we employ to construct bounds are as follows:

Assumption A4. (*Bounded Outcome*) $Y(z, d) \in [y^l, y^u]$, for $z, d = \{0,1\}$.

[7] Some of the prior literature (e.g., FF-L 2010; FF-L, 2013; CFF-L, 2017; CFF-L, 2018) has also considered a third assumption that restricts the $LNATE_k^Z$, $LMATE_k^Z$, and $LATE_k^Z$ of each stratum to be either non-positive or non-negative. We do not consider this assumption here since the previous literature on the effects of the lottery draft and the military service on future incarcerations (e.g., Kuziemko, 2010; Lindo and Stoecker, 2014; Albaek et al., 2016) is inconclusive about the effect's sign. Therefore, our results in Section 5 below are based on estimated bounds that do not restrict the sign of the effects of military service and lottery draft on incarceration.

Assumption A5. (*Weak Monotonicity of Mean Potential Outcomes Across Strata*)

(a) $E[Y(1, D_0)|c] \leq E[Y(1)|at]$; (b) $E[Y(0, D_1)|c] \leq E[Y(0)|at]$;

(c) $E[Y(z))|c] \leq E[Y(z)|at]$; (d) $E[Y(z)|at] \leq E[Y(z))|nt]$;

(e) $E[Y(z, 0))|c] \leq E[Y(z, 0)|at]$; (f) $E[Y(z, 0)|at] \leq E[Y(z, 0))|nt]$, where $z=\{0, 1\}$.

Assumption A4 states that the incarceration potential outcomes have a bounded support, which is satisfied in our setting since the outcomes considered are binary indicators. Assumption A5 formalizes the notion that particular strata likely have characteristics that do not make them less likely to be imprisoned than others. More specifically, in our empirical setting we make the assumption that, conditional on the same eligibility to draft status and potential veteran status, the never-takers are not less likely to be incarcerated than always-takers, who in turn are not less likely to be incarcerated than compliers. We extensively discuss and justify this weak ranking of strata in section 5.3.1. An admittedly small but relevant difference between the bounds employed here and the prior literature (e.g., FF-L, 2013 and CFF-L, 2017), is that our ranking of strata under A5 is different. As a result, the expressions for the bounds presented below differ from those in prior literature. The derivations and proofs of the bounds on $LATE_c^Z$ and $LATE_{at}^Z$ under assumptions A1-A5 are presented in Appendix 1. As it was the case before, one can average out Z to obtain estimated bounds on $LATE_c$ and $LATE_{at}$.

The following proposition formally presents the bounds on $LATE_c^Z$ and $LATE_{at}^Z$ under Assumptions A1-A5.

Proposition 2. If Assumptions A1-A5 hold, $L_M \leq MATE^Z \leq U_M$ and

$$\frac{L_M}{E[Y|Z=1] - E[Y|Z=0]} \leq LATE_c^Z \leq \frac{U_M}{E[Y|Z=1] - E[Y|Z=0]}\ , \text{ and } L_{at} \leq LATE_{at}^Z \leq U_{at}\ ,$$

where

$L_M = \Pr(Z=1) \cdot \Delta_3^1 + \Pr(Z=0) \cdot \min\{\Delta_1^0, \Delta_3^0\}$

$$U_M = \Pr(Z=1) \cdot \Upsilon_1^1 + \Pr(Z=0) \cdot \Upsilon_1^0$$

$$\Delta_3^1 = E[Y|Z=1] - p_{1|0} \cdot \min\{U^{1,at}, E[Y|Z=1, D=0]\} - p_{0|1} \cdot E[Y|Z=1, D=0]$$
$$-(p_{1|1} - p_{1|0}) \cdot \min\{\mathrm{U}^{1,\mathrm{at}}, \mathrm{E}[\mathrm{Y}|\mathrm{Z}=1, \mathrm{D}=0]\}$$

$$\Delta_1^0 = (p_{1|1} - p_{1|0}) \cdot (y^l - \min\{E[Y|Z=0, D=0], E[Y|Z=0, D=1]\})$$

$$\Delta_3^0 = p_{1|0} \cdot E[Y|Z=0, D=1] + p_{1|0} \cdot \max\{E[Y|Z=0, D=0], E[Y|Z=0, D=1]\}$$
$$+(p_{1|1} - p_{1|0}) \cdot y^l - E[Y|Z=0]$$

$$\Upsilon_1^1 = (p_{1|1} - p_{1|0}) \cdot (E[Y|Z=1, D=1] - y^l)$$

$$\Upsilon_1^0 = (p_{1|1} - p_{1|0}) \cdot (E[Y|Z=0, D=1] - L^{0,c})$$

$$L_{at} = \Pr(Z=1) \cdot (E[Y|Z=1, D=1] - E[Y|Z=1, D=0]) + \Pr(Z=0)$$
$$\cdot (E[Y|Z=0, D=1] - U^{0,nt})$$

$$U_{at} = \Pr(Z=1) \cdot (\min\{E[Y|Z=1, D=0], U^{1,at}\} - y^l) + \Pr(Z=0)$$
$$\cdot (E[Y|Z=0, D=1] - L^{0,c})$$

and $U^{1,at}$, $U^{0,nt}$, and $L^{0,c}$ are defined as in Proposition 1.

Proof. See Appendix 1.

3.3 Estimation and Inference

We end this section with a discussion about estimation and inference. The nonparametric bounds above will be estimated via the plug-in principle (i.e., by plugging-in the estimated means and trimmed means in their expressions). To conduct statistical inference on the bounds that contain maximum or minimum operators, we rely on the methodology proposed by Chernozhukov, Lee, and Rosen (2013), since for those bounds standard inference breaks down (Hirano and Porter, 2012). In particular, half-median unbiased estimates of the lower- and upper-bounds are obtained, along with valid confidence regions for the true parameter of interest. The specific implementation of this methodology is based on the one used in FF-L (2013). For bounds that do not contain those operators, we construct confidence regions for the true parameter of interest following Imbens and Manski (2004).

Furthermore, since we analyze the net effects of the lottery draft and military service effects in subsamples defined by birth cohorts, we in fact perform multiple testing of null hypotheses. It is well known that the situation of multiple testing increases the risks of falsely rejecting a true null hypothesis of no effect of interest. To control for this potential issue, we adopt three different sequential multiple testing procedures. The first is the sequential Family-wise Error Rate (FWER) testing procedure in Holm (1979) based on a sequential rejective Bonferroni procedure. The second and third procedures are the sequential False Discovery Rate (FDR) in Benjamini and Hochberg (1995) and the sharp sequential FDR in Benjamini, Krieger, and Yekutieli (2006). The latter provides better power than the former. The FWER estimates the probability that the false rejections under a "family" of null hypotheses is greater than zero, while the FDR measures the probability that all the rejections of a "family" of null hypotheses are false. To implement the multiple testing procedures to our bounds, we follow Mourifie and Wan (2017). In the case in which the bounds contain maximum or minimum operators, we obtain the p-value at which each null hypothesis is rejected by the confidence intervals of Chernozhukov, Lee, and Rosen (2013), and then implement the multiple testing procedures on the total null hypothesis tested for each effect across the 5 birth cohorts under analysis.

4. Data and Empirical Strategy

The data we employ comes from three sources and is similar to the data used by Lindo and Stoecker (2014). First, we employ cross-sectional data from the Survey of Inmates in State and

Federal Correctional Facilities (SISFCF) in 1979, 1986, and 1991. The SISFCF is representative of all inmates in the nation's state and federal correctional facilities, and contains extensive information on offenses, criminal history, demographic characteristics (including exact birth dates), and military service records. The survey data are collected through personal interviews with a nationally representative sample of sentenced inmates in state and federal facilities. The 1979 and 1986 survey only selected state facilities, while the 1991 survey selected both state and federal facilities in two separate surveys. The SISFCF provides sampling weights constructed so that the sample is representative of the prison population in the corresponding survey year. This feature enables us to estimate the inmate counts necessary to construct the incarceration rates at the population level, as explained below.

We classify inmates as incarcerated for a violent crime if any of the listed offenses in his record involve violent offenses, and we classify inmates as incarcerated for a nonviolent crime if any of the listed offenses involve nonviolent offenses.[8] Besides incarceration outcomes related to current offenses, we also construct a measure of recidivism in order to analyze the military service effect on this important aspect. For the violent and nonviolent recidivism outcomes, an indicator variable is set to one if the inmate had juvenile criminal justice contacts before the age of 18-20 and is currently incarcerated for violent or nonviolent crimes, respectively, and it is set to zero otherwise. We define "having a juvenile criminal justice contact" as having arrests or probation records before the age of 18, or having ever been incarcerated before the year 1968.[9] The reason

[8] These offenses are coded in the SISFCF using the National Prisoner Statistics offense code categorization. Violent offenses include murder, unspecified homicide, manslaughter, kidnapping, rape, assault, lewd act with children, robbery, forcible sodomy, blackmail/extortion/intimidation, hit and run driving, child abuse, and other violent offenses coded under the same 3-digit code; whereas nonviolent crimes include all other types of crimes.

[9] The recidivism variables are constructed using three SISFCF survey questions on inmates' prior arrests, probations, and incarcerations. The first question is "have you ever been placed on probation, either as a juvenile or adult?", which is combined with "how old were you the first time as a juvenile?" and "How old were you the first time as an adult

we define arrests and probations before the age of 18 as our measure of criminal justice contact before induction or eligibility to draft is that, before the Vietnam Era lottery draft was implemented, the local boards called men between 18 ½ and 25 years old who were classified 1-A (available immediately for military service), and later called up men turning 20 when the lottery draft was imposed. Hence, it is reasonable to use age 18 as the cut-off for the pre-draft arrests and probations. As for the use of the year 1968 for prior incarcerations, it is due to the fact that the exact age is not available, but the year of admission to an incarceration facility is. Thus, 1968 is chosen since the first lottery draft took place in December 1969 and by 1968 the 1948-1952 birth cohort is between the ages of 15-19.

The eligibility to draft (*Z*) is defined as a binary variable taking the value one if the inmate had RSN below the corresponding draft year's eligibility cutoff, and 0 otherwise. The RSN is constructed based on the exact birth date information in the SISFCF and the lottery numbers obtained from the Selective Service System (SSS) website. The veteran status (*D*) is a binary indicator coded based on whether the inmate served in the U.S. armed forces and first entered the military between the years of 1968-1975.

Table 2 presents summary statistics for the SISFCF inmate sample. The sample consists of 2700 white and 2619 nonwhite inmates. The table shows summary statistics on lottery draft-eligibility, veteran status, estimated strata proportions (under A1 and A3), and crime outcomes for white and nonwhite inmates. We see that a higher proportion of white inmates served in the

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[in SISFCF]?" in order to determine the age at the first probation. The second question is "how many times have you ever been arrested, as an adult or a juvenile, before your current incarceration?", which is combined with "how old were you the first time you were arrested for a crime" to determine the age. The third is a set of questions in SISFCF about prior incarcerations. The age for each prior incarceration is not available but we use whether admission to an incarceration facility occurred before 1968 based on the questions "when were you first admitted to that facility: [Year] (for your N-th sentence)?".

Vietnam Era war, and while nonwhite inmates have a higher estimated proportion of never-takers than whites, white inmates have a higher estimated proportion of always-takers. It is also interesting to observe that the estimated proportion of compliers is small for white and nonwhite inmates, and it is not significantly different from zero for white inmates. Regarding the criminal offending status, white inmates exhibit a higher proportion of nonviolent crime offenders and nonviolent crime recidivists relative to nonwhite inmates, while nonwhite inmates exhibit a higher proportion of violent crime offenders, violent crime recidivists, and a higher proportion of inmates who were incarcerated before 1968 regardless of their current criminal offending status. Lastly, white and nonwhite inmates show similar proportions on arrests and probation before 18 years of age.

The individual-level information on inmates in the SISFCF is combined with counts on the population of males born in the U.S. from the Vital Statistics of the United States (VSUS) 1948-1952, following the clever insight of Lindo and Stoecker (2014). Specifically, we combine counts of inmates from the SISFCF with live birth statistics of males from the VSUS by race, birth year, and month to calculate incarceration rates for each day between 1948 and 1952—the years that represent the cohorts affected by the lottery draft.[10] The different incarceration and recidivism outcomes are based on constructing mean incarceration (or recidivism) rates for a certain crime type in survey year s, of males born in birth year y and birth month m, with eligibility to draft z, veteran status d, and belonging to the latent stratum $k=\{at, c, nt\}$:

$$Incarceration\ outcome_{sym}(z, d|k) = \frac{\#of\ inmates_{sym}(z,d|k)}{\#of\ Births_{(z, d|k, y, m)}}. \quad (8)$$

[10] Since VSUS only reports births by month, we construct the number of births by day by apportioning the total births of a month evenly over the month's days. The same procedure was followed by Lindo and Stoecker (2014).

The numerator of the constructed incarceration rate outcome in (8) is the inmate counts by characteristics *s*, *y*, *m*, *z*, *d* and *k* from the SISFCF, obtained by using the appropriate SISFCF-provided sampling weights that make the inmate sample representative of the population of inmates in state and federal prisons in the corresponding survey year.[12] Table 3 summarizes the estimated counts of inmates for males born in 1948-1952, along with counts broken down by draft-eligibility and veteran status. From the inmate counts across survey years, we find no indication of a significant drop in the inmate counts for the 1948-1952 birth cohorts from 1979 to 1991. This may be a result of individuals in this cohort being around the prime criminal offending ages (20-40 years old) in the U.S. (Snyder, 2012) during these surveys years.

The denominator in (8) is the male population in the U.S. defined by characteristics *y*, *m*, *z*, *d*, and *k*. To construct it, we employ the VSUS in combination with a set of population-level estimates of the *k* strata proportions in Wang et al. (2017), obtained from a special version of the 1982-1996 National Health Interview Survey (NHIS) that is representative of the male population in the U.S. This third data source is needed to properly break-up the U.S. population into the *k* latent strata. Thus, we estimate the proportion of the group defined by characteristics *y*, *z*, *d*, and *k* in the U.S. male population using the draft-eligibility and Vietnam Era military service variables in the NHIS. Then, the male population in that group is obtained by multiplying the estimated proportion of that group by the VSUS U.S. male population.

Regarding our recidivism outcomes constructed following equation (8), it is important to note that they are not comparable to the conventional measure of recidivism that expresses the

[12] We have verified that the total inmates' counts computed using the sampling weights correspond to the official inmates' count statistics published by the Bureau of Justice Statistics Inmate Census (Bureau of Justice Statistics, 1982, 1989; Snell, 1993).

number of recidivists divided by the count of previously incarcerated individuals. For us to create a comparable measure to this conventional indicator of recidivism, we would need to divide the inmate count of recidivists by the count of previously incarcerated males for the birth cohorts exposed to the Vietnam-era lottery draft, which is not available to us. Instead, our measure of recidivism following (8) is constructed by dividing the inmate count of recidivists by the U.S. male population count (for each subgroup defined by characteristics y, m, z, d, and k). It is important to keep in mind this distinction when interpreting our results on recidivism.

Table 4 presents the U.S. population-level incarceration rates for violent and nonviolent crime offences by draft-eligibility status. In the third and sixth columns, we present the differences in the incarceration rates between draft-eligible and draft-ineligible males for whites and nonwhites, respectively.[14] These estimates suggest no statistically significant "intention-to-treat" effects of the draft-eligibility on the population incarceration rates of whites or nonwhites.

A final difficulty to overcome in the implementation of the nonparametric bounding technique is that estimation requires, in principle, access to individual-level information on the outcome. This is due to the necessary computation of trimmed means in the bounds' expressions presented in Section 3. While we do not have access to individual-level data on the outcomes of interest—since we construct incarceration and recidivism rates—we are still able to estimate the bounds by exploiting the binary nature of the outcomes of interest and the relative magnitudes of the incarceration rates and the trimming proportions. More specifically, since the average population incarceration and recidivism rates for the observed groups defined by $\{Z = z, D = d\}$

[14] The estimates presented in Table 4 are somewhat different to the estimates provided in Table 2 of Lindo and Stoecker (2014), particularly the estimates corresponding to the 1991 survey year. These differences stem from differences in the way each study constructs the variables under analysis. A detailed comparison of the two variable construction procedures is available from the authors upon request.

are each strictly smaller than the relative proportions of any strata that constitute the trimming proportions used in computing trimmed means, the trimmed means can be computed without access to individual-level data. To illustrate how the computation of the trimmed means (and thus the estimation of our bounds) proceeds, consider the example of computing trimming bounds for $E[Y(1)|c]$. The trimming bounds for this object use the observed group with $\{Z = 1, D = 1\}$ and the trimming proportion $\pi_c/(\pi_c + \pi_{at})$. Since the average incarceration outcome for the observed group, $E[Y|Z = 1, D = 1]$, is strictly smaller than the trimming proportion $\pi_c/(\pi_c + \pi_{at}) = 1 - \frac{p_{1|0}}{p_{1|1}}$, the $y^{11}_{1-\frac{p_{1|0}}{p_{1|1}}}$-th and $y^{11}_{\frac{p_{1|0}}{p_{1|1}}}$-th quantiles of Y in this observed group must be the value zero. Hence, the upper bound of $E[Y(1)|c]$ can be computed by dividing the estimated number of inmates within the group with $\{Z = 1, D = 1\}$ by the estimated total male population that belongs to the compliers stratum in that observed group; and the lower bound of $E[Y(1)|c]$ is zero following the argument above. The same procedure is employed whenever trimmed means are necessary to estimate the bounds in Propositions 1 and 2.

5. Results

5.1 Raw Relationship Between Lottery Draft and Incarceration and Recidivism Outcomes

To explore the potential variations of incarceration and recidivism rates within eligible and ineligible lottery numbers, we start by plotting the estimated incarceration and recidivism rates for violent and nonviolent offenses within 12 lottery number intervals in Figure 2 (whites) and Figure 3 (nonwhites). Each one of the intervals contains 30 lottery numbers, except the last interval that consists of 36 lottery numbers. The y-axis represents the population level incarceration or recidivism rate, while in the x-axis we use the largest lottery number of each interval to indicate that lottery interval (e.g., 30 is for the 1-30 lottery interval and so on). The incarceration and recidivism rates for each survey year are presented by connected dots.

In principle, receiving a low lottery number likely leads to military induction or potential draft avoidance behaviors as suggested in Card and Lemiuex (2001) and Kuziemko (2010). If both the military service and the potential draft avoidance behaviors have a similar effect on the future incarceration and recidivism rates (e.g., crime instigation), one may hypothesize that the incarceration rates may exhibit a pattern from high to low as lottery draft numbers increase. Figure 2 shows these estimated rates on the four crime outcomes in our analysis for white males born in 1948-1952. From the figure, it is hard to discern any trends in incarceration and recidivism rates from low to high lottery numbers in any of the survey years, with perhaps the exception of violent crime in the first panel. A similar lack of trend can be seen in the nonwhite incarceration rates in Figure 3. Therefore, we find no strong incarceration and recidivism rate trends within eligible or ineligible lottery numbers along the 12 lottery number intervals. These results, however, do not imply a zero net effect of the lottery draft, as the net effect and the mechanism effect (through the military service) may work in opposite directions, potentially cancelling each other out.

In the next subsection, we employ the bounds presented in Section 3 to disentangle the net effect of the lottery draft on incarceration and recidivism outcomes from its mechanism effect through the channel of military service. In producing our results, we will account for the potential correlation between the lottery numbers and the birth months in the 1969 draft mentioned in Section 2. We do this by estimating our bounds within birth month-by-year cohorts and subsequently aggregating them using weights based on the male population born in each month-by-year. We also note that, since the draft-eligibility is randomly assigned and thus independent of pre-military service characteristics (except for the potential correlation to birth month-by-year), it is not necessary to control for covariates measured prior to the military service (e.g., family structure, income, risky behaviors) to avoid the self-selection into military service.

5.2 Net Effects of the Lottery Draft on Incarceration and Recidivism Outcomes

In Figure 4, we present estimated bounds for the net effect of the lottery draft on the never-takers stratum, who are the potential draft avoiders. We define the incarceration and recidivism outcomes at the population level as in equation (8). The shaded bar and the capped intervals represent the estimated bounds and confidence intervals (90 and 95 percent), respectively; the crosses represent the mean incarceration rates of ineligible-to-draft nonveterans. The top two panels in Figure 4 present estimated bounds and confidence intervals on the net effect of the lottery draft for the 1948-1952 born white (Panel A) and nonwhite (Panel B) draft avoiders. The estimated lower bounds for whites suggest that the net effect of the lottery draft on their violent incarceration and recidivism (first and third bars) is an increase of at least 0.02 p.p. (9.6% and 19.7% for violent incarceration and recidivism, respectively, relative to the mean outcome of ineligible-to-draft nonveterans, which is not shown in figures). For nonwhites, the estimated lower bounds are consistent with a net effect on violent recidivism of at least 0.03 p.p. (3.4%; the third bar) and on nonviolent recidivism of at least 0.06 p.p. (10.2%; the fourth bar). However, the four previously discussed bounds are not precisely estimated, as their 90% confidence intervals do not exclude a zero net effect. The estimated lower bounds on the other outcomes presented in Panels A and B of Figure 4 are negative, thus not excluding a zero net effect. Therefore, using the combined 1948-1952 birth cohorts, we do not find evidence of non-zero net effects of the lottery draft on the outcomes of interest.

We explored the potential heterogeneity in the net effect of the lottery draft for each of the cohorts exposed to the draft in 1969 (born in 1948, 1949 and 1950), 1970 (born in 1951), and 1971 (born in 1952). In principle, there could be differences in the avoidance behaviors of each cohort caused by the different timing of the draft and/or by the perceptions or uncertainty attached to the unknown draft cutoff in each lottery. The most potent net effects of the lottery draft were found for the cohort born in 1950, one of the first cohorts exposed to the draft. Panels C and D in Figure 4 present the estimated bounds and confidence intervals for the net effect of the lottery draft on this cohort, while the corresponding results for other birth cohorts are available upon request.

Panel C of Figure 4 presents estimated bounds and confidence intervals for the net effect of the lottery draft for the 1950-born white never-takers. The estimated lower bounds suggest that the net effect of the lottery draft increases the incarceration rates for violent crimes for white never-takers by at least 0.07 p.p. (38.4%; the first bar), although the 90 percent confidence interval marginally includes zero. The estimated lower bounds for the net effects of the lottery draft also imply increases in violent recidivism and nonviolent recidivism of at least 0.03 p.p. (40%; the third bar) and 0.002 p.p. (2.0%; the fourth bar); however, the confidence intervals on these estimated bounds do not rule out a zero net effect. For nonviolent crime incarceration, the estimated bounds include a zero net effect.

Panel D of Figure 4 presents estimated bounds and confidence intervals for the net effect of the lottery draft for the 1950-born nonwhite never-takers. For them, the estimated lower bounds imply a net effect of the lottery draft on violent crime incarceration of at least 0.46 p.p. (40.8%; the first bar), and for nonviolent crime incarceration of at least 0.39 p.p. (37.9%; the second bar). For these two effects, the 95 percent confidence intervals exclude zero. The estimated lower bound on the net effect of the lottery draft on violent crime recidivism implies an increase of at least 0.37 p.p. (69.0%; the third bar), with the 95 percent confidence interval excluding a zero net effect. Lastly, the estimated lower bound for the net effect of the lottery draft on nonviolent crime recidivism implies an increase of at least 0.7 p.p. (13.8%), but the corresponding 90 percent confidence interval does not exclude a zero net effect.

The significant net effects of the lottery draft on the violent crime incarceration and recidivism of the 1950 born white and nonwhite never-takers become more precise in the pooled sample. In Figure 5, the first and the third bar show that the estimated lower bounds imply that draft-eligibility increases violent crime incarceration and recidivism by at least 0.13 p.p. (40.4%) and 0.08 p.p. (56.9%), respectively, with the corresponding 95 percent confidence intervals excluding zero. The estimated lower bounds on the second and third bars also show that the draft eligibility increases nonviolent crime incarceration and recidivism by at least 0.01 p.p. (4.0%) and 0.01 p.p. (7.6%), respectively, although for their 90 percent confidence intervals do not exclude a zero net effect.

Thus far, we have documented statistically significant net effects of the lottery draft on the violent incarceration and recidivism outcomes of never-takers born in 1950 (but not other cohorts) who were exposed to the first military draft. A valid concern, however, is that we have conducted tests of hypotheses in several subsamples, and thus rejection of the null of no net effects could occur by chance. Thus, we implement the three multiple testing procedures explained in Section 3 that allow statistically controlling for a valid significance level when simultaneously testing whether the null hypothesis of a zero net effect over all birth cohorts considered. Table 5 presents the conclusions reached when using each of the three multiple testing procedures considered in the specific case of the net effect for the never takers in the1950-born cohort. For the pooled sample of white and nonwhite never-takers born in 1950, we are able to reject the null hypotheses of zero net effect of the lottery draft at the 5 percent level for violent crime incarceration and at the 10 percent level for the violent crime recidivism. The three multiple testing procedures agree in this conclusion. Conversely, applying the same robust procedures, we are not able to reject the null hypotheses that the net effect of the lottery draft is zero for any of the incarceration and recidivism outcomes in the separate samples of 1950-born white or nonwhite never-takers.

Overall, we find statistical evidence that the net effect of the lottery draft (independent of military service) is to increase future violent crime incarceration and recidivism for never-takers in the 1950-born cohort subjected to the first Vietnam-era lottery draft. For other cohorts and outcomes analyzed, while most of the estimated lower bounds on the net effects are positive, their confidence intervals include zero (i.e., the bounds are imprecisely estimated). Considering that the ER assumption in conventional IV methods must be satisfied by every unit, this evidence implies the invalidity of this assumption in the context of violent crime incarceration and recidivism. As a consequence, point estimates of the effect of military service on violent incarceration and recidivism outcomes based on conventional IV methods using the lottery draft IV could be misleading. For this reason, in the following section, we analyze the military service effects on incarceration and recidivism outcomes using the lottery draft as an IV but allowing for violations of the ER.

5.3 Local Average Treatment Effects of Military Service on Incarceration and Recidivism Outcomes

5.3.1 Discussion and Justification of Assumption A5

In Section 3, we discussed and justified assumptions A1 to A4. In this subsection, we give detailed consideration to the plausibility of A5, which implies that, conditional on the same eligibility to draft status and potential veteran status, the never-takers are not less likely to be incarcerated than always-takers, who in turn are not less likely to be incarcerated than compliers. We focus on three types of arguments. One is based on information provided by estimated pre-draft incarceration outcomes for each stratum. The idea is that looking at pre-draft incarceration outcomes by strata can inform the proposed ranking in A5. A second argument is based on extant substantive literature about the overall characteristics of Vietnam-era veterans and males affected by the lottery draft as they relate to incarceration outcomes. Finally, the last argument is based on a testable implication of the bounds presented in Proposition 2, which can be used to "falsify" the set of assumptions A1 to A3 plus A5.

The estimation of average pre-draft outcomes for each stratum is possible under assumptions A1 to A3 using individual-level data from the population of interest (FF-L, 2010; FF-L, 2013; CFF-L, 2018). Intuitively, for the *nt* stratum, the average pre-draft outcomes correspond to the mean pre-draft outcomes of eligible-to-draft nonveterans, while the average pre-draft outcomes for the *at* stratum correspond to the mean pre-draft outcomes of ineligible-to-draft veterans. The average pre-draft outcomes for the *c* stratum can be estimated given that compliers are mixed with *at* in the group of eligible-to-draft veterans and with *nt* in the group of ineligible-to-draft nonveterans, and both the strata proportions and the average pre-draft outcomes of *at* and *nt* are identified.

However, the individual-level data available to us is only for inmates. As a result, we consider two ways to undertake this analysis, each of which is imperfect since the resulting estimated average pre-draft outcomes will likely be biased. The first is to employ data exclusively on inmates from the SISFCF. In this case, the pre-draft outcomes are estimated using the observed inmates' draft eligibility and veteran status. A problem with using exclusively inmate data is that the estimates will likely not be representative of the U.S. male population due to self-selection into incarceration. The second way in which we compute average pre-draft outcomes consists of scaling the estimated inmate counts that belong to a particular stratum by the estimated U.S. male population of the corresponding group using data from the VSUS and statistics from the special version of NHIS. In this case, the bias in the estimates may arise because of incarceration rates' differences among the total population with different pre-draft outcomes. As a result, the estimated average pre-draft outcomes by strata may not be representative of those in the U.S. male population. Nevertheless, we still employ them as suggestive evidence. Interestingly, we find that, even though the biases in the two methods are generally different from each other, the estimated average pre-draft strata outcomes point to the same conclusion of lending indirect support to the weak ranking of the strata in A5.[15]

The estimated average pre-draft outcomes using the two methods explained above are presented in Panel A and Panel B of Table 6, respectively.[16] We focus on three estimated average pre-draft outcomes related to contacts with the criminal justice system: arrests, probation, and incarceration before turning 18 years old or before the year 1968. The pre-draft averages of these three measures indicate that never-takers were more likely to have been arrested, on probation, and incarcerated before they were subjected to the draft, relative to compliers and always-takers (columns 5 and 6). These results hold under either of the two methods to estimate the average pre-draft outcomes described above. It is likely that individuals with pre-draft contacts with the criminal justice system will, on average, also show higher probabilities of incarceration in adulthood (e.g., Bayer et al., 2009; Aizer and Doyle, 2013). Thus, these estimates offer indirect support to the weak ranking of strata postulated in A5 involving the never-taker stratum: this

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[15] Appendix 2 presents the formal mathematical expressions of the possible biases in these two ways of obtaining average pre-draft outcomes by strata for the U.S. male population.

[16] We note that, in contrast to other papers using nonparametric bounds (FF-L, 2010; FF-L, 2013; Bampasidou et al., 2014; Amin et al., 2016), we do not report the estimated average pre-draft outcomes for the compliers stratum. Instead, we report estimated pre-draft outcomes for the groups consisting of always-takers & compliers, and never-takers & compliers. The reason is that the proportion of compliers in the inmate sample is extremely low: for the 1948-1952 born male inmates in the pooled SISFCF 1979-1991 sample, the proportions of compliers are 1.78% for whites and 2.98% for non-whites (see Table 2). These small proportions do not allow the estimation of the complier's average pre-draft outcomes with any precision.

stratum likely does not have lower incarceration and recidivism rates in the years after the military draft, compared to always-takers and compliers, conditional on their draft-eligibility.[17]

Turning to the weak monotonicity relationship between always-takers and compliers in terms of their potential average incarceration and recidivism outcomes in assumption A5, Table 6 suggests that always-takers have higher average rates of probation before the draft, relative to a group that combines always-takers and compliers (bold figures in column 7).[18] Importantly, none of the other differences between the same two groups in Table 6 (column 7) are significantly negative, which would contradict the weak ranking of these strata in A5. Thus, overall, we find indirect evidence supporting the weak ranking of strata postulated in A5, while we do not find statistically significant evidence directly contradicting such ranking.

To present arguments supporting A5 based on substantive literature, a key point to keep in mind is that always-takers are essentially enlisted volunteers, while compliers join the military only if nudged by the lottery draft. There is an extant literature documenting evidence of a myriad of important individual characteristics that are positively correlated to voluntary military enlistment, such as unemployment (Horne, 1985), low academic abilities (Hosek and Peterson, 1985), low family income (Baskir and Strauss, 1978), and experience of child abuse (Khawand, 2009). In turn, those same individual characteristics have been linked to higher rates of crime and

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[17] Additional indirect supporting evidence for never-takers having no less criminality outcomes than the other two strata is as follows. Based on statistics published by the Bureau of Justice Statistics, the incarceration rate of veterans was 43% lower compared to nonveterans in 1985 (Noonan and Mumola, 2007). This lends suggestive evidence that the post-draft criminality (conditional on draft eligibility) of always-takers and compliers should be no higher than never-takers, since Vietnam veterans are composed of the former two strata. (The year 1985 is chosen here since it falls around the middle of our time period, but the figures are similar for other years.)

[18] We estimated, but do not report, other pre-draft characteristics such as the probability of being physically or sexually abused before age 18. They suggest that always-takers are more likely to be the victims of physical and sexual abuse before age 18 relative to a group including both always-takers and compliers. At the same time, Widom (1989) documents positive correlations between childhood abuse victimization and adulthood criminal behavior. Thus, this pre-draft characteristic suggests that always-takers have no lower incarceration probabilities relative to compliers.

incarceration. For instance, Lin (2008) documents the positive correlation between unemployment and crime; Engelhardt (2010) documents a negative correlation between unemployment spells and recidivism of released inmates; Lochner and Moretti (2004) document a negative correlation between schooling and crime; and Heller et al. (2011) point to negative correlations between family income and crime. Based on this literature linking individual characteristics to both voluntary enlistment and higher probability of incarceration, always-takers—who are volunteers—are likely to have no lower average incarceration outcomes compared to compliers.

The final evidence supporting assumption A5 that we advance relies on one testable implication that results from using assumptions A1 to A3 plus A5 in constructing the bounds in Proposition 2, which can be used to "falsify" those assumptions.[19] Recall that A5 indicates that the potential incarceration rate outcome of never-takers (conditional on draft eligibility and veteran status) should not be lower than those for the compliers and the always-takers. The testable implication states that the conditional mean $E[Y|Z=1, D=0]$, which is the estimate of the crime outcomes of draft-eligible never-takers $E[Y(1)|nt]$, must not be smaller than the conditional mean $E[Y|Z=1, D=1]$, which is the estimate of the draft-eligible always-takers and compliers, $E[Y(1)|at, c]$. In Table 7, we present estimates of $E[Y|Z=1, D=0] - E[Y|Z=1, D=1]$ using the four incarceration and recidivism outcomes in our analysis, for the groups of whites and non-whites. These estimated differences are all positive and statistically significant in most instances. Thus, they do not statistically reject the testable implication for any of the outcomes or analysis groups. The same conclusion is reached when these groups are broken down by birth cohorts (not shown).

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[19] More specifically, if the data statistically rejects the testable implication then the assumptions do not hold; but if the testable implication is satisfied, then we can only say that the data is consistent with the assumptions.

5.3.2 Local Average Treatment Effects of Military Service on Incarceration and Recidivism Outcomes of Compliers

In this subsection, we focus on the effect of military service on compliers – the subpopulation who would serve only when they were eligible to draft. Table 8 presents point estimates of the average effect of draft eligibility (the conventional "intention-to-treat" or ITT effect) and the conventional IV estimates under the exclusion restriction. These point estimates indicate that both the total effect of draft-eligibility (i.e., the ITT) and the military service effect significantly increases violent crimes and recidivism in the sample of the 1950-born nonwhites and in the pooled sample of the 1950 born whites and nonwhites. We will contrast below the IV point estimates under the ER with the estimated bounds that do not require such assumption.

Given the evidence presented in the last section pertaining a significant net effect of the lottery draft on the outcomes of interest for never-takers, the ER may not be satisfied, and thus IV estimates of the military service effect may be biased. To obtain more robust statistical inference on the military service effect, we employ the nonparametric bounds (under Assumptions A1-A5) that do not employ the ER assumption. Figure 6 and Figure 7 show the estimated bounds and their estimated confidence intervals on the $LATE_c$ of military service on the incarceration outcomes for white and nonwhite compliers born in 1948-1952 and in1950 (Figure 6) and the pooled sample of compliers born in 1950 (Figure 7). For comparison, these figures show, with a cross, the corresponding conventional IV estimates (obtained employing two-stage least squares). Note that these estimated bounds are for the same $LATE_c$ as the one estimated by the conventional IV estimator when the ER holds.

Looking at Figure 6 and Figure 7, it is evident that in all cases except one, the estimated bounds include a zero military effect on the corresponding outcome. Also, none of the corresponding confidence intervals rule out a zero military effect. While this contrasts with the significant military effects when using the conventional IV point estimates, we note that the estimated bounds indicate that the effect of military service on crime outcomes can be as large as the upper bound and as low as the lower bound. In this regard, the data is just not informative about the military effect under assumptions A1 to A5. Still, in 11 out of 20 cases considered in Figures 6 and 7, the conventional IV estimates of the military service effect on the crime outcomes of compliers fall outside the estimated bounds' confidence intervals.

We now focus attention on the instances where the IV estimates in Table 8 suggested significant military service effects on the corresponding crime outcomes. Panel D in Figure 6 (for 1950-born nonwhites), where the IV estimates indicate that military service significantly increases violent crime incarceration (the first cross) and violent recidivism (third cross) by 5.0 p.p. and 3.9 p.p., respectively, the upper bound estimates suggest that the military service effects on these two outcomes are at most 1.4 p.p. and 0.43 p.p., respectively. The effects implied by the estimated upper bounds are significantly lower than the IV point estimates according to their 95 percent confidence intervals. Also, in Figure 7 (for the pooled sample of white and nonwhite compliers born in 1950), where the IV estimates indicate that military service increases violent crime incarceration (the first cross) and violent recidivism (the third cross) by .91 p.p. and 0.64 p.p., respectively, the estimated upper bounds suggest that the military service effects on these two outcomes are at most .36 p.p. and 0.11 p.p., respectively. Again, the effects implied by the estimated upper bounds are significantly lower than the IV estimates according to their 95 percent confidence intervals.

5.3.3 Local Average Treatment Effects of Military Service on Incarceration and Recidivism Outcomes of Volunteers

We now move on to the always-taker subpopulation—the draft volunteers—for whom we have discovered heterogeneous results on the effect of military service on incarceration and recidivism outcomes for the cohorts born in 1948-1952, 1950, 1951, and 1952. The results for whites and nonwhites are presented in Figures 8 and 9, respectively. In those figures, the crosses represent the mean incarceration rates of nonveterans, reported for reference.

Results for white and nonwhite always-takers who were born in 1948-1952 and 1950 are presented in Panel A and Panel B of Figure 8 and Figure 9, respectively. All the corresponding estimated bounds include zero, with the exception of those on the nonviolent crime incarceration and recidivism for the 1950 born nonwhites. However, the 90 percent confidence intervals on all the previous bounds do not rule out a zero military effect. Thus, the results for the 1948-1952 and 1950 born whites and nonwhites suggest that the military service crime effect is potentially nonexistent.

In contrast, turning to the 1951 and 1952 birth cohorts (Panel C and Panel D in the two figures, respectively), most of the estimated bounds on the $LATE_{at}$—and some of their estimated confidence intervals—exclude zero. Specifically, for white always-takers born in 1951 and 1952, the estimated bounds indicate that military service increases the incarceration rates for violent crimes by at least 0.20 p.p. and 0.31 p.p., as can be seen in the first bars in Panel C and Panel D of Figure 8, respectively. These are potentially large effects as they represent at least 144 and 160 percent of the mean outcome of nonveterans, respectively. For the outcome of nonviolent crime incarceration for the white always-takers born in 1951 and 1952 (second bars in Panel C and Panel D of Figure 8, respectively), the estimated bounds indicate that military service increases the incarceration rates by at least 0.12 p.p. (60.6% relative to the mean outcome of nonveterans, and hereafter) and 0.25 p.p. (111%). Furthermore, the 95% confidence intervals on the four estimated bounds just discussed exclude zero. As for the recidivism outcomes for white always-takers born in 1951 and 1952 (last two bars in Panel C and Panel D of Figure 8, respectively), the estimated bounds indicate that the effect of military service is to increase violent recidivism by at least 0.03 p.p. (44.3%) and 0.08 p.p. (80.2%), and nonviolent recidivism by at least 0.05 p.p. (61.5%) and 0.07 p.p. (61.6%), respectively. In this case, however, the 90% confidence intervals are not able to rule out a zero effect on recidivism outcomes, except for the effect on the 1952-born white always-takers.

The results for nonwhite always-takers born in 1951 and 1952 are presented in Panel C and Panel D of Figures 9, respectively. The estimated bounds on the military service effect on incarceration outcomes are all positive and exclude zero. The bounds for the 1951-born always-takers (Panel C) indicate an increase in violent incarceration rates of at least 0.32 p.p. (22.8%), and an increase in nonviolent incarceration rates of at least 0.20 p.p. (14.1%). The estimated bounds' 90% confidence intervals, however, do not exclude zero. For the 1952 birth cohort (Panel D), military service increases violent crime incarceration of nonwhite always-takers by at least 0.71 p.p. (43.9%) and their nonviolent incarceration rate by 0.67 p.p. (47.5%). The 95 percent confidence intervals of these two sets of bounds exclude zero. As for the effects on the recidivism outcomes of nonwhite always-takers, three out of the four sets of estimated bounds include zero and none of the four corresponding confidence intervals exclude zero. The one set of estimated bounds excluding zero is that for nonviolent recidivism of the 1951 nonwhite always-takers, which indicates an increase in recidivism by at least 0.15 p.p. (22.3%). In sum, the evidence of military service effects on the incarceration and recidivism outcomes of nonwhite always-takers is more tenuous than for whites.

We also perform statistical inference robust to multiple testing for the estimated military service effect in this section for the same reason we used those methods for the estimated net effect for the never-takers in section 5.2. As shown in Table 9, after employing the three multiple testing methods across the five birth cohorts, we are able to reject the null hypothesis of zero military service effects for always-takers on the violent crime incarceration for the 1951- and 1952-born whites and for the nonviolent crime incarceration of the 1952-born whites. However, we are no longer able to reject the null hypothesis of a zero military service effect on the 1952-born nonwhite always-takers' crime outcomes or the 1951-born white always-takers' nonviolent crime outcomes at conventional significance levels.

6. Discussion

6.1 Net Effects of the Lottery Draft

The results on the net effect of the lottery draft for never-takers ($LNATE_{nt}$) suggest that it directly increased the incarceration and recidivism rates of certain subgroups of males in the subject birth cohorts. This positive net effect is particularly significant for the 1950-born white and nonwhite never-takers' violent incarceration and recidivism rates. Recall that the bounds on $LNATE$ for the never-takers employ the same assumptions as traditional IV methods with the exception of the ER assumption, and that the ER is imposed on all individuals. Therefore, these results imply that the lottery draft is an invalid IV for military service when estimating the military service effects on incarceration and recidivism outcomes for these groups using traditional IV methods. Estimates of these effects from traditional IV methods are potentially unreliable.

What channels might explain the presence of a net effect on the violent crime incarceration and recidivism rates of never-takers? One plausible explanation is related to the "dodging-down" avoidance behavior (Kuziemko, 2010), namely, the increased delinquency and arrests among potential draftees with low SES to avoid the military draft. The idea is that early delinquency and incarceration may potentially increase later years' recidivism. The notion of increased criminal behavior as an adult after contacts with the judicial system as a youth has been documented in Bayer et al. (2009) and Aizer and Doyle (2015). Both papers document that earlier year incarcerations significantly increase recidivism both for violent and nonviolent crime types. Alternatively, another channel through which the lottery draft may have a net effect on incarceration and recidivism outcomes is the "dodging-up" avoidance behavior, such as obtaining admissions into college to avoid the draft (e.g., Card and Lemieux, 2001). This type of avoidance behavior, resulting in higher educational attainment, is predicted to reduce incidence of criminal activities given the positive relationship between education and crime (Lochner and Moretti, 2004; Amin et al., 2016). In this regard, our results suggest that the "dodging down" dominates the "dodging up" avoidance behavior in the current context.[21] Indeed, the counteracting effects of these avoidance behaviors may be a reason why the estimated bounds do not exclude zero more often.

It is also interesting to note that the results on the net effect of the lottery draft suggest that the most significant increases in incarcerations are for the 1950-born never-takers. This cohort was subjected to the first lottery draft that occurred in 1969. One conjecture for the stronger net effects on this cohort is that the first lottery draft gave the subjected individuals a relatively short time to react on their assigned draft-eligibility, compared to the birth cohorts that were subjected to the latter two lottery drafts. The 1969 lottery numbers were drawn in December and men were called

[21] It is somewhat suggestive that the results in Lochner and Moretti (2004) on the crime reduction effects of high school graduation are only significant for whites but not for nonwhites, and the crime reduction effects are only significant for drug sales crimes of whites. In contrast, the evidence on the adverse effects of juvenile incarcerations on adult-life recidivism appear more robust since they are found for both violent and nonviolent types of crime.

for physical examinations and inductions starting in January of 1970, while the 1970 and 1971 lottery numbers were drawn in July and August, respectively, and the call for inductions begun at the start of the following year. Thus, it is plausible that under a shorter time constraint, "dodging-down" avoidance behaviors were more likely to be taken by never-takers compared to "dodging-up" avoidance behaviors. As a consequence, the crime instigation effect of the lottery draft on incarceration and recidivism outcomes would be more likely to dominate any of its crime reduction effects.

A second related conjecture is that the avoidance behaviors undertaken by never-takers born in 1950 may have been contingent—to a larger degree—on the specific lottery numbers drawn, relative to the never-takers subjected to subsequent years' lottery drafts. The reason for this is also related to the shorter reaction time that never-takers in the first lottery draft had. Later birth cohorts may have taken avoidance behaviors that depended less on the specific lottery numbers drawn, such as obtaining fatherhood or educational deferments, even before their lottery numbers were drawn. Additionally, for the 1948-1949 birth cohorts, it is possible that they had already taken draft avoidance behaviors towards the local drafts when they reached 18.5-years-old and before the first lottery draft was implemented in December of 1969. Thus, if the avoidance behaviors of the 1948-1949 and 1951-1952 born never-takers were less contingent on their lottery numbers, then their net effect of the lottery draft may have been weakened. Unfortunately, we lack individual-level population data necessary to formally evaluate these two conjectures related to the lottery numbers drawn. Further analysis along these lines is desirable to inform the channels through which the lottery draft impacted incarceration and recidivism outcomes.[22]

6.2 LATE of Military Service on Compliers

The conventional IV estimates (under the ER assumption) suggest that military service has statistically significant crime instigation effects in the sample of the 1950-born nonwhites and the pooled sample of 1950-born whites and nonwhites. In contrast, the statistical inference based on the estimated bounds is not able to rule out zero military effects. Furthermore, the results on the effect of military service for the subpopulation of 1950 born compliers on their incarceration rates show that the conventional IV estimates are outside of the 95 percent confidence intervals of the estimated bounds that allow for violations of the ER (and impose assumption A5). A possible explanation for the larger IV estimates relative to the corresponding estimated upper bounds is the potential presence of upward biases in the IV estimates induced by the positive net effect of the lottery draft on the incarceration outcomes for violent offenses documented in Figure 4 and Figure 5.

6.3 LATE of Military Service on Volunteers

The results of the military service effect on incarceration and recidivism outcomes for white always-takers ($LATE_{at}$) indicate that the estimated bounds for the 1948-1952 and the 1950 cohort do not exclude zero (Panel A and Panel B in Figure 8 and Figure 9), while those for the 1951 and 1952 cohorts on the violent and nonviolent incarceration rates are predominantly positive and exclude zero (Panel C and Panel D in Figure 8 and Figure 9). One potential explanation for

[22] We also estimated the direct effect of the lottery draft on never-takers crime outcomes for the 1953 birth cohort, whose lottery numbers were assigned but were never drafted. The 90% confidence intervals do not reject zero direct effect of the lottery draft on all outcomes.

the different results across birth cohorts relates to the differential amount of Vietnam war casualties: the in-service casualties for the 1948-1950 cohort are substantially higher than those occurring during the service time of the 1951 and 1952 cohorts.[25] Since our estimated bounds do not control for the mortality of always-takers before the survey years 1979-1991 of the SISFCF, they could be affected by the potentially higher mortality of always-takers in the 1948-1950 birth cohorts during their Vietnam conflict service. If the effect of military service on the incarceration rate of those always-takers is positive, then the estimated bounds could be positive and exclude zero.

In an effort to understand the factors that may be behind the difference in the results on the effect of military service on the incarceration rates between the 1951-1952 and other birth cohorts of always-takers, we use the inmate's data to compare several of their average characteristics in Table 10. It should be stressed that by using the sample of SISFCF inmates we are likely using a non-representative sample of the population (see section 5.3.1), and thus the lessons from this exercise should be regarded as suggestive. The average characteristics are estimated using the ineligible-to-draft veterans ($Z = 0, D = 1$), a group that consists exclusively of always-takers in the sample (under A1-A3). The choice of characteristics to be compared is guided by what the literature has documented as likely channels through which military service affects crime outcomes: combat exposure (Rohlfs, 2010), drug use (Robins, 1973), pre-service arrests and offending (Albæk et al., 2016), childhood physical abuse victimization and maltreatment (Khawand, 2009), and family background (Hjalmarsson and Lindquist, 2016).

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[25] According to the Defense Casualty Analysis System (DCAS) Extract Files (The U.S. National Archives and Records Administration, 2016), the Vietnam conflict record counts by incidence or death date shows 34,852 death records in the period 1968-1970, which corresponds to the years in which the 1948-1950 cohort would likely have served after they reached 20 years of age (of course, some always-takers may have volunteered before the first lottery draft in 1969). In contrast, the corresponding record counts during the period 1971-1975, when the 1951 and 1952 cohorts would have likely served, 3,304 death records are documented.

The first set of characteristics relate to violence exposure and include whether the inmate was stationed in Vietnam, whether he had seen combat during military service, and whether he served on or before 1970 (when most U.S. casualties took place).[26] For each of these three violence exposure measures, the always-takers in the 1948-1950 cohort have higher averages relative to the always takers in the 1951 and 1952 cohorts (for both whites and nonwhites). This may appear counterintuitive given some of the extant literature documenting a positive relationship between combat exposure and violent crime (e.g., Killgore et al. 2008; Rholfs, 2010; Sreenivasan et al. 2013). Nevertheless, there is also an existing body of studies on the effects of military service during the Vietnam and AVF eras documenting that, for example, Vietnam veterans experienced psychological benefits (i.e., affirmation to patriotic beliefs, self-improvement, and solidarity with others) that are positively associated with a myriad of traumatic exposures (i.e., fighting, killing, perceived threat to oneself, death/injury of others) in the war zone (Fontana and Rosenheck, 1998). Another example is Dohrenwend et al. (2004), who document that 70.9% of the US male Vietnam veterans appraised the impact of their service on their present lives as mainly positive. For military service during the AVF Era, Anderson and Rees (2015) document that units that were never-deployed contributed more to community violent crime (e.g., murders and rapes) relative to the contribution of the units that were deployed. The positive impacts of violence exposure on post-military service life may be explained by the post-traumatic growth[27] effect of wartime combats

[26] The "stationed in Vietnam" variable is constructed using the question "were you stationed in Vietnam, Laos, or Cambodia; stationed in the waters around these countries; or did you fly in missions over these areas (during your military service in 1968-1975)?" in SISFCF 1979-1997. The "combat" exposure variable is constructed using the question "Did you see combat in a combat or line unit while stationed in this region (Vietnam, Laos, or Cambodia)?" in SISFCF 1991 only. The "served on or before 1970" indicator is constructed using the question in SISFCF 1979-1991 "what was the year you entered the military?".

[27] Posttraumatic growth is defined as positive psychological changes in response to trauma (Tedeschi and Calhoun, 1995).

on veterans (Maguen et al., 2006; Forstmeier et al., 2009), which may in addition improve veterans' post-service adaptation to civilian life and reduce their tendency to commit crimes. Thus, it is possible that combat exposure may be related to lower incarceration rates for always-takers.

The second set of characteristics compared in Table 10 pertain to drug use, and include ever using drugs, age at which drugs were used for the first time, and whether the inmate used drugs during the month before the current offense.[28] Table 10 shows that inmate always-takers born in 1951-1952 are more likely to ever have used drugs (significant among whites only), have used drugs at a younger age (significant among nonwhites only), and are more likely to have been using drugs during the month before the current offense (significant among nonwhites only). Given the extant literature documenting the prevalent use and addiction to drugs among U.S. troops during the Vietnam War (e.g., Robins et al., 1975; Stanton, 1976), and the documented positive relationship between drug use and criminal offenses (e.g., Ellinswood, 1971; Tinklenberg, 1973), this evidence seems consistent with the previously documented differential effects of military service on the incarceration rates of different cohorts of always-takers. For instance, Robins and Slobodyan (2003) document that one of the factors that significantly increased the probability of post-service heroin injection use among the veterans while in Vietnam was having a history of using non-opiate illegal drugs before they entered the military service. One may conjecture that the easy access to illicit drugs during service in Vietnam may have reinforced the post-service drug abuse of always-takers who had been taking drugs before the military service. Furthermore, recent

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[28] The variable "ever used drugs" is constructed using the variables in SISFCF 1979-1991 on "Have you ever used heroin/other opiate or methadone outside a treatment program? Have you ever used methamphetamine or amphetamines without a doctor's prescription? Have you ever used methaqualone/barbiturates without a doctor's prescription? Have you ever used crack/cocaine/LSD or other Hallucinogens/Marijuana or Hashish/any other drug?". The "age first used drug" is constructed using the variables in SISFCF 1979-1991 on "At what age did you first use [drug names from above]". The "using drugs before the current offense" is constructed using the variables in SISFCF 1979-1991 on "During the month before your arrest on current offense, were you using drugs?"

studies show a positive relationship between drug use and criminal offending, including robberies, burglaries (Corman and Mocan, 2000) and income generating crimes in general (Gottfredson et al., 2008).

Another important characteristic analyzed in Table 10 is the involvement with the criminal justice system as a juvenile. The estimated averages indicate that inmate always-takers born in 1951-1952 were more likely to have had criminal justice contacts (arrests, probation, and incarceration) when they were juveniles relative to the earlier cohort 1948-1950, a difference significant only among whites. Based on this evidence, one may conjecture that a longer criminal history prior to military service may contribute to a larger crime instigation effect of the Vietnam military service, which is consistent with the differential effects found for always-takers in different cohorts. This is also consistent with similar evidence reported in Hjalmarsson and Lindquist (2016) in the context of the mandatory military service in Sweden. In contrast, there is also evidence that military service reduces the crime rate of juvenile offenders: both Albaek et al. (2016)—in the context of peace time military service in Denmark—and Teachman and Tedrow (2014b)—in the context of the AVF military service in US—have found that military service reduces the crime rates of prior juvenile offenders.

The last set of characteristics analyzed in Table 10 are three indicators related to family background and socioeconomic status. The estimated averages for the different cohorts suggest that the white inmate always-takers in the 1951-1952 cohort are more likely to have experienced physical abuse before age 18, and also have fathers that attained less schooling compared to their counterparts in the 1948-1950 cohort. For nonwhite inmates, always-takers born in the 1951-1952 cohorts are more likely to have one or both parents who served time in prison, relative to their counterparts in the 1948-1950 cohort.[29] Therefore, inmate always-takers in the 1951-1952 birth cohorts, on average, tend to have lower socioeconomic status as measured by these three variables, which may be another reason why military service significantly increased these cohorts' incarceration rates relative to the 1948-1952 birth cohorts. The crime literature documents a strong negative correlation between socioeconomic status and incarcerations (e.g., Kearney et al., 2014), while there is also evidence in Hjalmarsson and Lindquist (2016) that military service has the most potent crime instigation effect among men with low socioeconomic status.

To summarize the analysis of average characteristics, inmate always-takers born in 1951-1952, as compared to the 1948-1950 born inmates, are less likely to have seen combats; more likely to have used drugs at a younger age before the induction, to have used drugs before the current offense, and to have juvenile criminal contacts before the induction; and tend to have lower socioeconomic status and worse family background. These disadvantaged characteristics of always-takers from the 1951-1952 cohorts—with the possible exception of combat exposure previously discussed—appear consistent with the finding that the effect of military service on incarceration and recidivism outcomes is stronger for always-takers in the 1951-1952 cohorts relative to the 1948-1950 cohorts. However, it is important to keep in mind that this evidence is to be regarded as suggestive since it is based on the sample of inmates for which we have data available, and not the U.S. population of military service volunteers.

6.4 Monetary Social Costs of Crime from the Lottery Draft and Military Service

[29] The "abused physically before age 18" variable is constructed using the variables "Have you been physically abused" and "Did this occur before or after you were 18 years old?" in SISFCF 1986-1991. The variable "highest grade father attended" is constructed using the same name of variable in SISFCF 1979-1991. The variable "parent served in correctional facilities" is constructed using the variables "Has anyone in your immediate family ever served time in jail or prison?" and "Who was that (who served in jail or prison)?".

Based on our results, we provide estimates of the crime and incarceration consequences of the Vietnam lottery draft and military service in terms of societal monetary costs. To do this, we computed the average violent and nonviolent per unit costs using available estimates in literature. For violent crimes, we use the average estimated unit crime tangible cost estimates in McCollister et al. (2010), which is $380,192 (converted into 2016 U.S. dollars). For nonviolent crimes, we combine the estimates in McCollister et al. (2010) and the costs for drug violations in Delisi and Gatling (2003) and computed an average per unit cost of $144,341 (in 2016 U.S. dollars). The estimated costs include victim costs (e.g., medical expenses, cash losses, property theft or damage), criminal justice system costs, and crime career costs (i.e., productivity losses of the perpetrator), with the exception of the estimated cost for drug violations offenses, which only includes the average criminal justice system costs.[30] The following assumptions are also employed: (i) each inmate committed only one violent/nonviolent offense, and (ii) any inmates observed in any single survey year of SISFCF 1979, 1986 and 1991 are not incarcerated in another survey year. Moreover, our estimated monetary costs are based only on our lower bounds estimates whose 90% confidence intervals exclude zero. Thus, they represent an estimated lower bound of societal monetary costs.

Since we find non-zero net effects of the lottery draft for the 1950 birth cohort, we base our estimates on the induced incarceration costs for them. Based on the estimated lower bound of the net effect of the lottery draft in the pooled white and nonwhite never-takers born in 1950, the estimated increase in the number of violent offenses between 1979 and 1991 is 853 and the corresponding estimated increase in the number of nonviolent offenses for nonwhite never-takers

[30] Other studies (e.g., Rohlfs, 2010) used the victimization social costs of violent acts in Miller, Cohen, and Wiersema (1996). We did not adopt their estimates as they do not include the criminal justice costs.

born in 1950 is 366.[31] We then multiply these estimated increases in violent and nonviolent offenses by their corresponding average per unit crime cost. The result is an estimated incarceration cost of at least $\big((\$380{,}192 * 853) + (\$144{,}341 * 366)\big) = \$377{,}132{,}582$.

For the incarceration effects of military service, we estimate offenses for violent and nonviolent crimes by white always-takers born in 1951-1952 and by non-white always takers born in 1952, using the same procedure above. Based on the corresponding estimated lower bounds, the estimated increase in offenses due to Vietnam military service between the year of 1979 and 1991 is at least 2,469 offenses. The induced total tangible cost is at least $688,927,839.[32] Adding up the incarceration net effect of the lottery draft plus the incarceration effect of the military service, the social costs of increased incarceration caused by the Vietnam lottery draft is at least $1,066,060,421.

7. Conclusion

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[31] To obtain the estimated increase in the number of violent offenses, we multiply the lower bound of $LNATE_{nt}^{Z}$ for the 1950-born white and nonwhite never-takers, 0.0012536, by the population of draft-eligible white and nonwhite never-takers born in 1950 (680,376). Similarly, the estimated increase in the number of nonviolent offenses is obtained by multiplying the lower bound of $LNATE_{nt}^{Z}$ for the nonwhite never-takers born in 1950, 0.0039425, by the population of nonwhite never-takers (92,779).

[32] To estimate the total number of offenses induced by military service, the total population of 1951- and 1952-born white always-takers is, respectively, 254,763 and 215,269, while the total population of 1952-born nonwhite always-takers is 33,372. We then multiply the lower bound estimates of military service effect $LATE_{at}$ of the respective birth cohort by the corresponding population of always-takers, obtaining the increased number of offenses by racial and birth year. Specifically, for 1951-born white always-takers, their estimated $LATE_{at}$ lower bound of military service on violent offenses is 0.0020024 times 254,763 = 510. For 1951-born white always-takers their estimated $LATE_{at}$ lower bound of military service on nonviolent offenses is 0.0011507 times 254,763 = 293. For 1952-born white always-takers, the corresponding estimated $LATE_{at}$ lower bound of military service on violent and nonviolent offenses are (0.0030653*215,269 = 660) and (0.002512*215,269 = 541), respectively. For 1952-born nonwhite always-takers, the corresponding estimated $LATE_{at}$ lower bound of military service on violent and nonviolent offenses are (0.0071961*33,372 = 240) and (0.0067336*33,372 = 225), respectively. Thus, the Vietnam War military service increased the violent and nonviolent offenses by always-takers by at least 1410 and 1059, respectively. Lastly, the induced total tangible costs are computed by multiplying the corresponding unit crime costs by the estimated draft-eligibility induced violent and nonviolent offenses, respectively. That is, (1,410*$380,192)+(1059*$144,341) = $688,927,839.

We examined the validity of the Vietnam Era lottery draft as an instrumental variable (IV) for military service in estimating causal effects of military service on incarceration and recidivism for violent and nonviolent crimes. Additionally, we reassessed the impacts of military service on the same outcomes using nonparametric bounds that allow for the IV to have a net impact—independent of military service—on the outcomes (i.e., allow for the invalidity of the IV). Finally, we provided a novel analysis of the effects of military service on incarceration and recidivism outcomes on a subpopulation that consists of military service volunteers, which may be informative about the current U.S. all-volunteer forces (AVF).

Our main findings are as follows. First, we show that the Vietnam Era lottery draft (net of the military service channel) increased the violent crime incarceration and recidivism among the 1950-born white and nonwhite males who were potentially draft avoiders (the "never-takers"). The estimated bounds' 95 percent confidence intervals on this effect exclude zero, and these results withstand the use of conservative multiple inference procedures. We interpret this evidence as a violation of the exclusion restriction (ER) in the current empirical context. Additionally, we found suggestive evidence that the net effect of the lottery draft on violent crime incarceration may be larger for two socially disadvantaged groups—nonwhites and those who had criminal justice contacts as juveniles.

Second, using estimated nonparametric bounds that allow for the IV to have a net impact on incarceration and recidivism rates, we are not able to rule out a zero effect of military service on those outcomes for the population of compliers. In other words, our estimated nonparametric bounds contain zero. Furthermore, we find that the conventional IV estimates, which assume the validity of the Vietnam lottery draft, may be biased. Specifically, we find that IV estimates may be upward biased for the effect of military service on violent crime incarceration and recidivism in the sample of 1950-born nonwhites and the pooled sample of 1950-born whites and nonwhites. We suspect that these biases can be attributed to the uncovered net effects of the lottery draft on the corresponding violent incarceration and recidivism outcomes.

Third, we analyze the military service effect on the always-takers subpopulation, which can potentially be used to draw implications for the current AVF veterans. For them, we find that there may be no effect of military service on violent or nonviolent incarceration outcomes for either whites or nonwhites born in 1948-1950, since the estimated bounds on those effects do not exclude zero. In contrast, the corresponding estimated bounds for the cohorts born in 1951-1952 suggest that the Vietnam military service increases the violent and nonviolent incarceration of the white always-takers by at least 0.20 p.p. and 0.12 p.p., respectively, with the 95% confidence intervals on the estimated bounds excluding zero. Also, for the nonwhite always-takers born in 1952, military service increases the violent and nonviolent incarceration rates by at least 0.72 p.p. and 0.67 p.p., respectively. A complementary analysis of average characteristics of always-takers of the different birth cohorts using our individual data on inmates suggests that the level of combat exposure may not be a significant contributor to the higher crime instigation effect of military service for the 1951-1952 birth cohort. Conversely, factors that appear to be significant contributors to the higher crime instigation effect of military service relate to drug use before, during, and after the periods of military service, pre-service criminal justice contacts, low socioeconomic status, and adverse family background. If the Vietnam Era military service indeed had potent crime instigation effects on military service volunteers who had drug abuse and criminal history records prior to the service, as our results suggest, then a potential implication is that current policies aimed at veterans' crime prevention could focus on pre-enlistment screening and treatment (particularly on criminal justice contacts and drug abuse history), in addition to post-service treatment. Lastly, taking the net effect of the Vietnam-era lottery draft and military service together, the results in this paper suggest that the tangible social cost of the violent and nonviolent offenses caused by these two effects was at least in the order of $1.1 billion in 2016 US dollars.

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Table 1. Relationship between latent principal strata and observed military service status (D) and eligibility to draft status (Z)

		Z	
		0	1
D	0	Never takers (*nt*) & Compliers (*c*)	Never takers (*nt*) & Defiers (*d*)
	1	Always takers (*at*) & Defiers (*d*)	Always takers (*at*) & Compliers (*c*)

Figure 1. Illustration of the $MATE^Z$ and $NATE^Z$ of the Vietnam War Lottery Draft

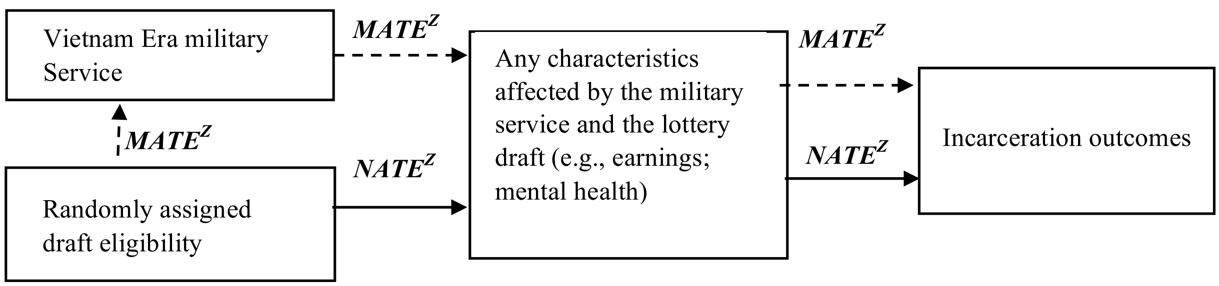

Table 2. Summary Statistics for the Inmates in SISFCF 1979-1991

Mean characteristics	White	Nonwhite	Difference
Total observations	2700	2619	
Vietnam veterans	0.2602***	0.1765***	0.0837***
	(0.0095)	(0.0081)	(0.0124)
Draft eligible	0.4365***	0.4378***	-0.0013
	(0.0107)	(0.0105)	(0.0151)
Never-takers	0.7298***	0.8068***	-0.0769***
	(0.0146)	(0.0129)	(0.0194)
Always-takers	0.2524***	0.1634***	0.0890***
	(0.0124)	(0.0103)	(0.0161)
Compliers	0.0178	0.0298*	-0.0121
	(0.0191)	(0.0165)	(0.0252)
Violent crime offenders	0.5322***	0.5927***	-0.0605***
	(0.0108)	(0.0105)	(0.0150)
Nonviolent crime offenders	0.5711***	0.5075***	0.0637***
	(0.0108)	(0.0106)	(0.0151)
Violent Recidivists	0.2589***	0.3222***	-0.0633***
	(0.0098)	(0.0101)	(0.0141)
Nonviolent Recidivists	0.2715***	0.2466***	0.0249*
	(0.0099)	(0.0094)	(0.0137)
Incarcerated before 1968	0.1545***	0.2038***	-0.0493***
	(0.0078)	(0.0085)	(0.0115)
Arrested before 18-year-old	0.3139***	0.3198***	-0.0059
	(0.0109)	(0.0105)	(0.0151)
On probation before 18-year-old	0.3029***	0.3065***	-0.0035
	(0.0100)	(0.0098)	(0.0140)

Note: Standard errors of the estimates are in brackets; *, ** and *** on the estimates indicate significance at 10%, 5% and 1% statistical significance levels, respectively.

Table 3. Estimated 1948-1952 Born Male Inmate Counts in SISFCF 1979, 1986, 1991

	Male Inmate	1948-1952 Born $Z=1, D=0$	1948-1952 Born $Z=1, D=1$	1948-1952 Born $Z=0, D=0$	1948-1952 Born $Z=0, D=1$
White Males					
1979 SISFCF	25915	7836	3293	11017	3770
1986 SISFCF	29990	9629	3254	12283	4824
1991 SISFCF State Facility	35321	11471	4326	14855	4669
1991 SISFCF Federal Facility	4613	1592	427	2223	370
Nonwhite Males					
1979 SISFCF	31673	11368	2407	15114	2785
1986 SISFCF	33043	10881	2652	16204	3306
1991 SISFCF State Facility	31470	11679	3026	14045	2721
1991 SISFCF Federal Facility	2257	842	243	941	232

Table 4. Summary Statistics of the U.S. Population-level Incarceration and Recidivism Rates by Draft Eligibility and Race

Characteristics	White			Nonwhite		
	Draft Eligible	Draft Ineligible	Difference	Draft Eligible	Draft Ineligible	Difference
All Surveys						
Violent Crime Incarceration	0.0022	0.0020	0.0002	0.0145	0.0140	0.0005
	(0.0001)	(0.0001)	(0.0002)	(0.0007)	(0.0006)	(0.0010)
Nonviolent Crime Incarceration	0.0021	0.0022	-0.0001	0.0129	0.0133	0.0003
	(0.0001)	(0.0001)	(0.0002)	(0.0007)	(0.0008)	(0.0010)
Violent Crime Recidivism	0.0010	0.0008	0.0001	0.0077	0.0069	0.0007
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0004)	(0.0007)
Nonviolent Crime Recidivism	0.0009	0.0010	-0.0001	0.0062	0.0057	0.0005
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0004)	(0.0007)
1979 Survey						
Violent Crime Incarceration	0.0019	0.0017	0.0002	0.0156	0.0172	-0.0016
	(0.0001)	(0.0001)	(0.0002)	(0.0010)	(0.0010)	(0.0014)
Nonviolent Crime Incarceration	0.0018	0.0018	-0.0000	0.0129	0.0110	0.0019
	(0.0001)	(0.0001)	(0.0002)	(0.0009)	(0.0007)	(0.0012)
Violent Crime Recidivism	0.0009	0.0008	0.0001	0.0075	0.0083	-0.0008
	(0.0001)	(0.0001)	(0.0001)	(0.0007)	(0.0007)	(0.0010)
Nonviolent Crime Recidivism	0.0008	0.0008	0.0000	0.0056	0.0051	0.0005
	(0.0001)	(0.0001)	(0.0001)	(0.0006)	(0.0005)	(0.0008)
1986 Survey						
Violent Crime Incarceration	0.0022	0.0020	0.0002	0.0157	0.0170	-0.0013
	(0.0002)	(0.0002)	(0.0002)	(0.0012)	(0.0010)	(0.0016)
Nonviolent Crime Incarceration	0.0018	0.0023	-0.0004	0.0124	0.0124	0.0000
	(0.0001)	(0.0001)	(0.0002)	(0.0011)	(0.0009)	(0.0014)
Violent Crime Recidivism	0.0011	0.0009	0.0002	0.0083	0.0093	-0.0010
	(0.0001)	(0.0001)	(0.0002)	(0.0008)	(0.0008)	(0.0012)
Nonviolent Crime Recidivism	0.0009	0.0012	-0.0002	0.0074	0.0064	0.0011
	(0.0001)	(0.0001)	(0.0002)	(0.0008)	(0.0007)	(0.0011)
1991 Survey						
Violent Crime Incarceration	0.0026	0.0022	0.0003	0.0152	0.0123	0.0029
	(0.0001)	(0.0001)	(0.0003)	(0.0014)	(0.0010)	(0.0017)
Nonviolent Crime Incarceration	0.0030	0.0029	0.0001	0.0161	0.0161	-0.0000
	(0.0002)	(0.0002)	(0.0003)	(0.0014)	(0.0014)	(0.0019)
Violent Crime Recidivism	0.0010	0.0009	0.0001	0.0084	0.0057	0.0027
	(0.0002)	(0.0001)	(0.0002)	(0.0009)	(0.0007)	(0.0012)
Nonviolent Crime Recidivism	0.0011	0.0011	0.0000	0.0065	0.0063	0.0002
	(0.0002)	(0.0001)	(0.0002)	(0.0009)	(0.0008)	(0.0012)

Note: Standard errors based on 2500 bootstrap replications and are parentheses.

Figure 2. Incarceration Rate for Violent and Nonviolent Crimes of 1948-1952 Whites by Lottery Interval

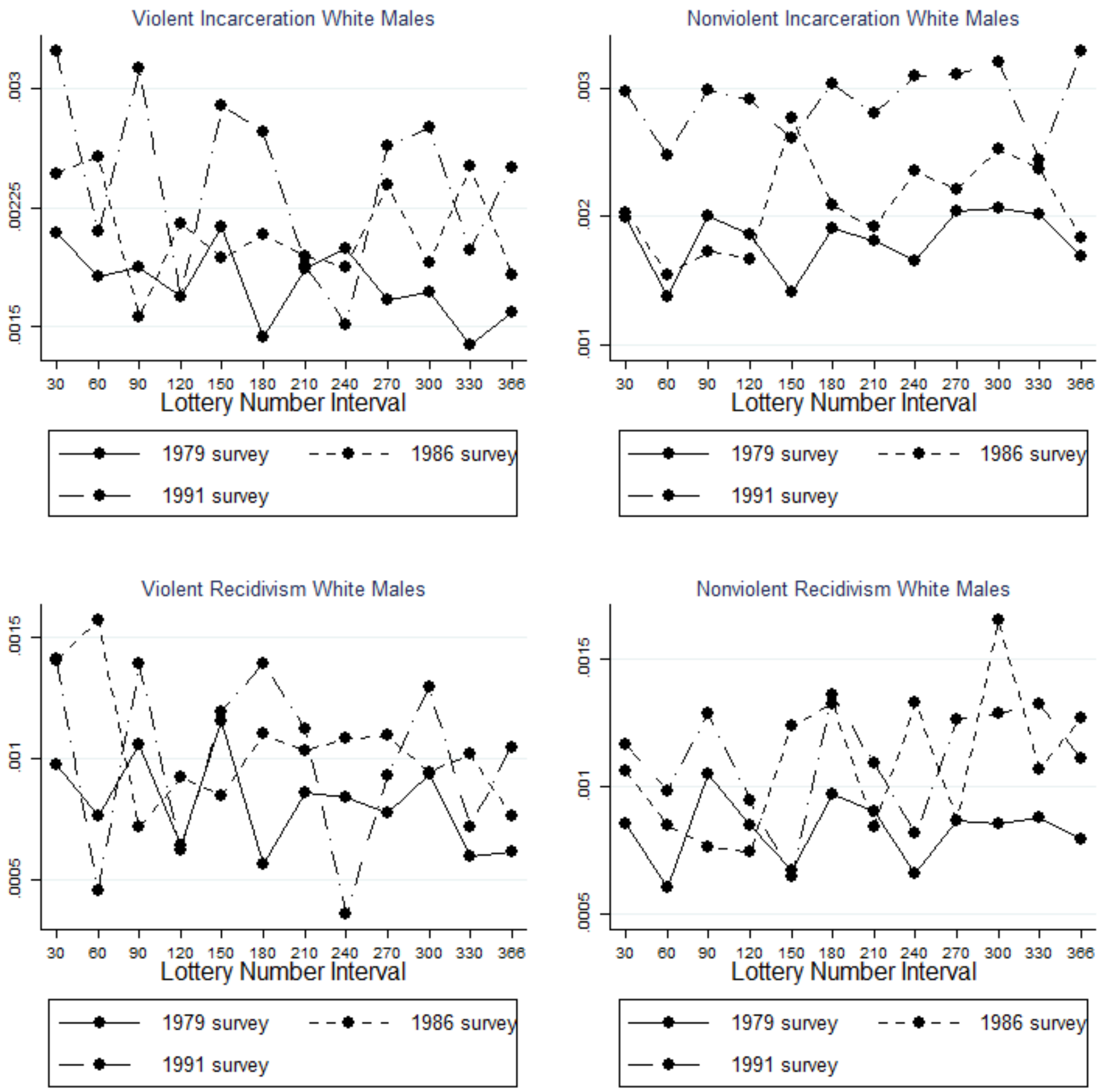

Figure 3. Incarceration Rate for Violent and Nonviolent Crimes of 1948-1952 Nonwhites by Lottery Interval

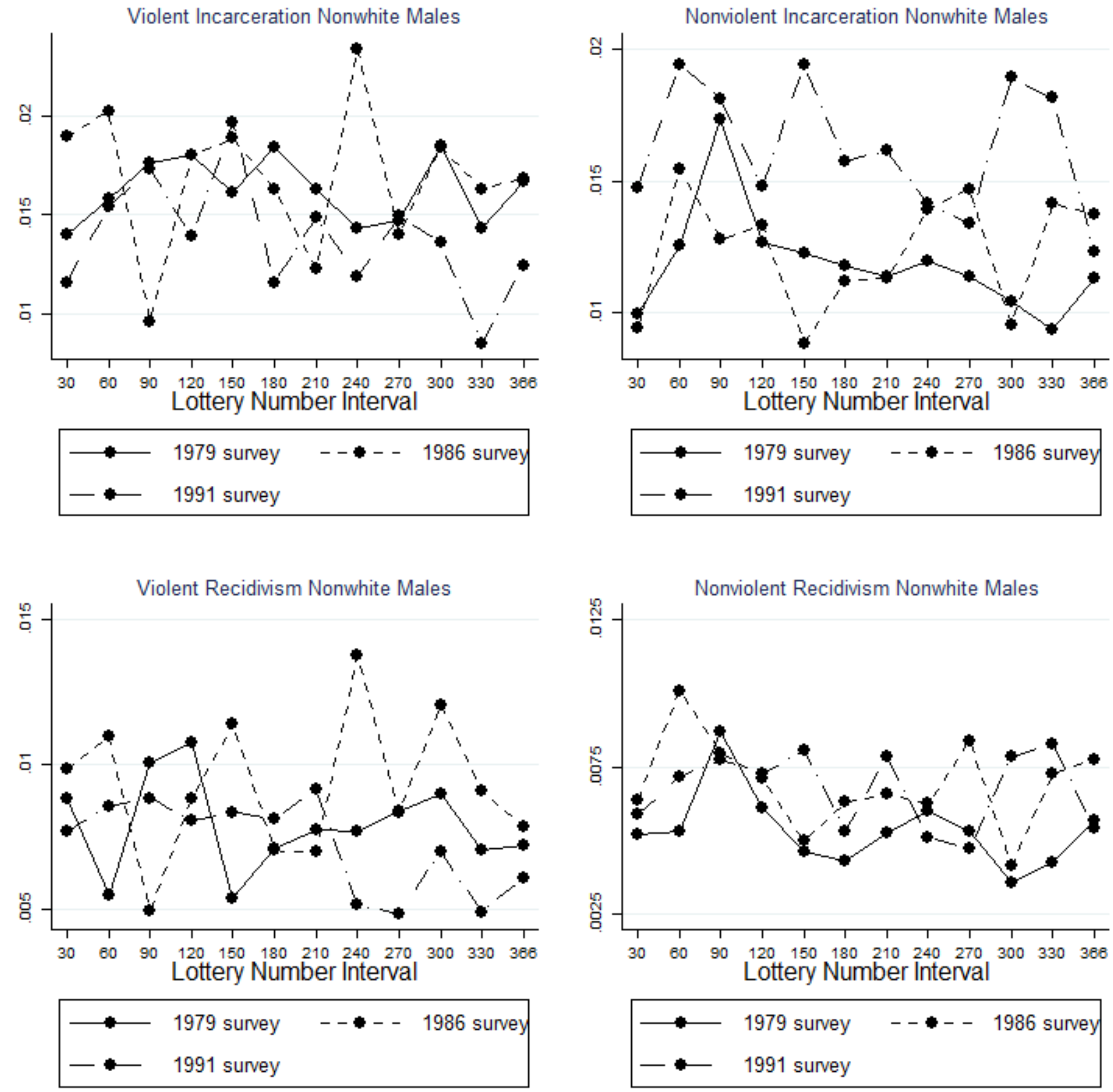

Figure 4. Net Effect of the Lottery Draft on Incarceration Outcomes of the Never-takers Stratum

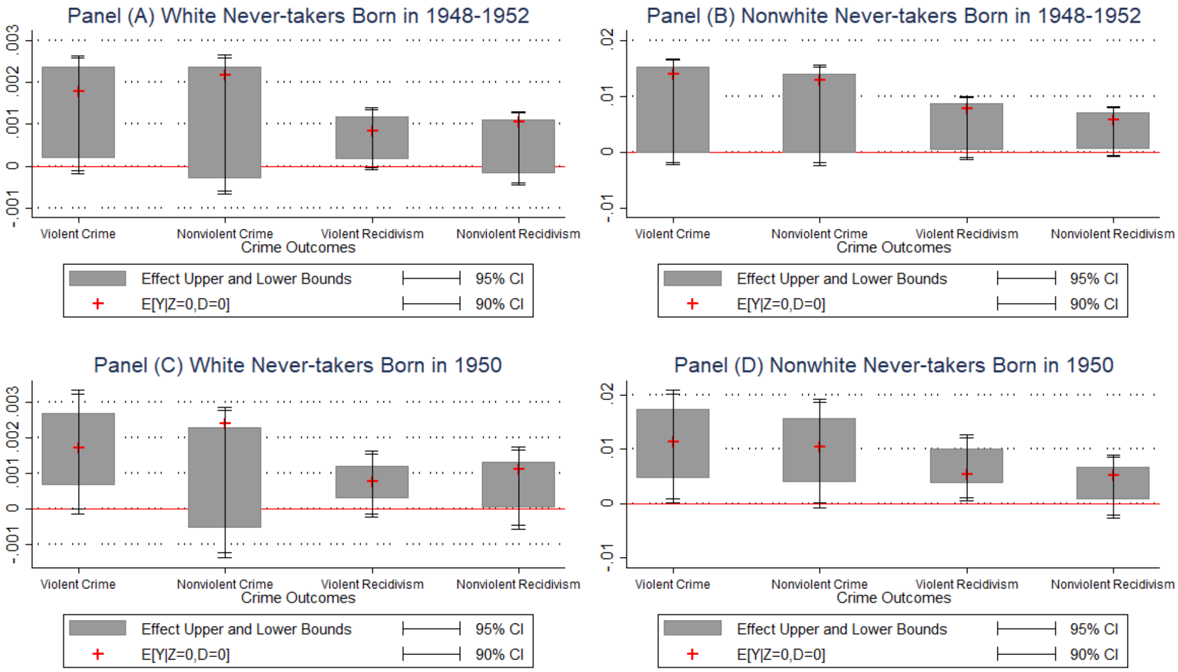

Figure 5. Net Effect of the Lottery Draft on Violent and Nonviolent Offending and Recidivism Outcomes of the Pooled 1950 Born White and Nonwhite Never-takers Stratum

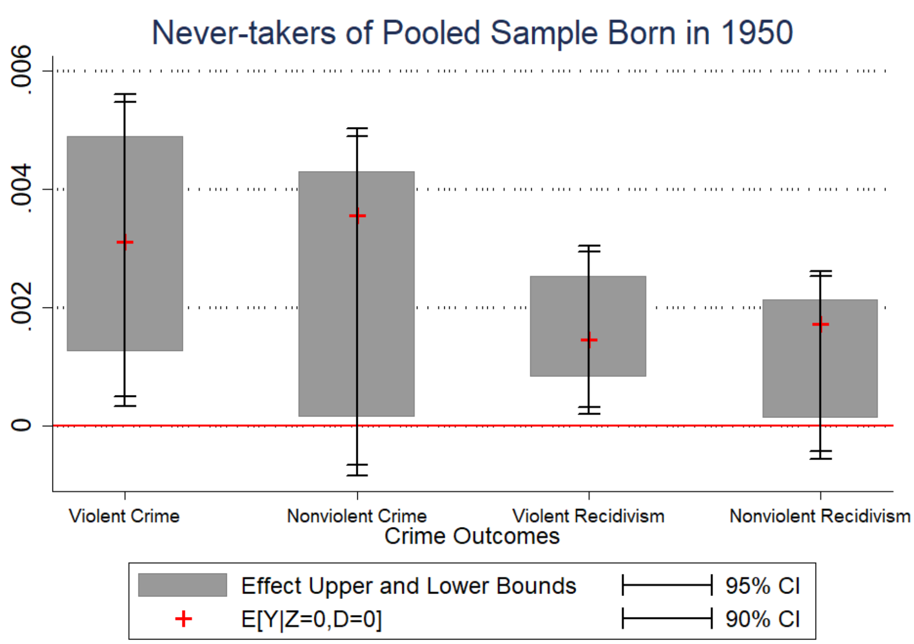

Table 5. Multiple Testing Results for the Net Effect of the Lottery Draft for the 1950 Born Never-takers (FWER Null Hypothesis H_0: $LNATE_{nt}$=0 in any of the cohort born in 1948, 1949, 1950, 1951 or 1952; FDR Null Hypothesis H_0: $LNATE_{nt}$=0 in all of the cohort born in 1948, 1949, 1950, 1951 or 1952)

	White Violent Offending	Nonwhite Violent Offending	White and Nonwhite Violent Offending	White Nonviolent Offending	Nonwhite Nonviolent Offending	White and Nonwhite Nonviolent Offending
Sequential FWER	**NR**	**NR**	**R****	**NR**	**NR**	**NR**
Sequential FDR	**NR**	**NR**	**R****	**NR**	**NR**	**NR**
Sharp Sequential FDR	**NR**	**NR**	**R***	**NR**	**NR**	**NR**
	White Violent Recidivism	Nonwhite Violent Recidivism	White and Nonwhite Violent Recidivism	White Nonviolent Recidivism	Nonwhite Nonviolent Recidivism	White and Nonwhite Nonviolent Recidivism
Sequential FWER	**NR**	**NR**	**R***	**NR**	**NR**	**NR**
Sequential FDR	**NR**	**NR**	**R***	**NR**	**NR**	**NR**
Sharp Sequential FDR	**NR**	**NR**	**R***	**NR**	**NR**	**NR**

Note: "R" stands for rejection and "NR" stands for nonrejection; *, ** and *** indicate significance at 10%, 5% and 1% statistical significance levels, respectively.

Table 6. Pre-draft Incarceration Outcomes for Different Groups of Inmates in SISFCF 1979-1991

	(1) Never-takers (nt)	(2) Always-takers (at)	(3) Always-takers and Compliers (*at* & *c*)	(4) Never-takers and Compliers (*nt* & *c*)	(5) nt v.s. at	(6) nt v.s. at & c	(7) at v.s. at & c	(8) nt v.s. nt & c
Panel A: Inmate Level								
Ever Arrested before 18-Year-Old (Obs: 5006)	0.3492 (0.0133)	0.2216 (0.0193)	0.2687 (0.0226)	0.3318 (0.0114)	**0.1276** **(0.0234)**	**0.0805** **(0.0262)**	-0.0471 (0.0297)	0.0174 (0.0175)
Ever on Probation before 18-Year-Old (Obs: 5354)	0.3251 (0.0125)	0.2192 (0.0181)	0.2325 (0.0197)	0.3237 (0.0104)	**0.1059** **(0.0220)**	**0.0926** **(0.0233)**	-0.0133 (0.0267)	0.0014 (0.0162)
Ever incarcerated before 1968 (Obs: 5126)	0.2236 (0.0108)	0.0841 (0.0119)	0.1016 (0.0142)	0.1861 (0.0087)	**0.1394** **(0.0160)**	**0.1220** **(0.0179)**	-0.0174 (0.0185)	**0.0375** **(0.0139)**
Panel B: Population Level								
Ever Arrested before 18-Year-Old	0.0030 (0.0002)	0.0015 (0.0002)	0.0013 (0.0002)	0.0023 (0.0001)	**0.0015** **(0.0003)**	**0.0017** **(0.0002)**	0.0002 (0.0002)	**0.0007** **(0.0002)**
Ever on Probation before 18-Year-Old	0.0025 (0.0001)	0.0013 (0.0002)	0.0009 (0.0001)	0.0020 (0.0001)	**0.0012** **(0.0002)**	**0.0015** **(0.0002)**	**0.0003** **(0.0002)**	**0.0005** **(0.0002)**
Ever incarcerated before 1968	0.0014 (0.0001)	0.0006 (0.0001)	0.0004 (0.0001)	0.0011 (0.0001)	**0.0008** **(0.0002)**	**0.0010** **(0.0001)**	0.0002 (0.0001)	**0.0003** **(0.0001)**

Note: Standard errors based on 1000 bootstrap replications are in parentheses; figures in bold indicate that they are statistical significant at 95 percent.

Table 7. Testable Implication of $E[Y(1)|nt] \geq E[Y(1)|at,c]$: $E[Y|Z=1,D=0]-E[Y|Z=1,D=1]$ (Males Born in 1948-1952)

Crime Outcomes	White	Nonwhite
Violent Crime Incarceration	0.0005* (0.0003)	0.0030* (0.0017)
Nonviolent Crime Incarceration	0.0008*** (0.0002)	0.0029* (0.0017)
Violent Crime Recidivism	0.0006*** (0.0002)	0.0044*** (0.0010)
Nonviolent Crime Recidivism	0.0005*** (0.0002)	0.0036*** (0.0010)

Note: Standard errors based on 2500 bootstrap replications are in parentheses; *, ** and *** indicate significance at 10%, 5% and 1% statistical significance levels, respectively.

Table 8. Estimated Military Service Effect using the Conventional IV Estimates

Survey Years	Violent Crime	Nonviolent Crime	Violent Recidivism	Nonviolent Recidivism
Panel A: Whites Born in 1948-1952				
Estimated effect of eligibility	.0002 [.0002]	-.0001 [.0002]	.0001 [.0001]	-.0002 [.0001]
IV estimated effect of service	.0017 [.0011]	-.0008 [.0012]	.0009 [.0007]	-.0005 [.0008]
Panel B: Nonwhites Born in 1948-1952				
Estimated effect of eligibility	.0005 [.0010]	.0003 [.0010]	.0007 [.0007]	.0005 [.0007]
IV estimated effect of service	.0067 [.0132]	.0043 [.01414]	.0101 [.0094]	.0067 [.0090]
Panel C: Whites Born in 1950				
Estimated effect of eligibility	.0005 [.0003]	-.0003 [.0003]	.0003 [.0002]	.0002 [.0002]
IV estimated effect of service	.0040 [.0028]	-.0023 [.0028]	.0022 [.0018]	.0014 [.0018]
Panel D: Nonwhites Born in 1950				
Estimated effect of eligibility	.0048** [.0021]	.0027 [.0023]	.0038*** [.0014]	.0004 [.0016]
IV estimated effect of service	.0497** [.0214]	.0276 [.0235]	.0392*** [.0148]	.0036 [.0166]
Panel E: Pooled Whites and Nonwhites Born in 1950				
Estimated effect of eligibility	.0011** [.0004]	.0001 [.0004]	.0007*** [.0002]	.0002 [.0003]
IV estimated effect of service	.0091*** [.0035]	.0010 [.0037]	.0064*** [.0022]	.0016 [.0025]

Note: Standard errors based on 2500 bootstrap replications are in parentheses; *, ** and *** indicate significance at 10%, 5% and 1% statistical significance levels, respectively.

Figure 6. Estimated Bounds for the Local Average Treatment Effect of Military Service on the Incarceration Rates for Compliers

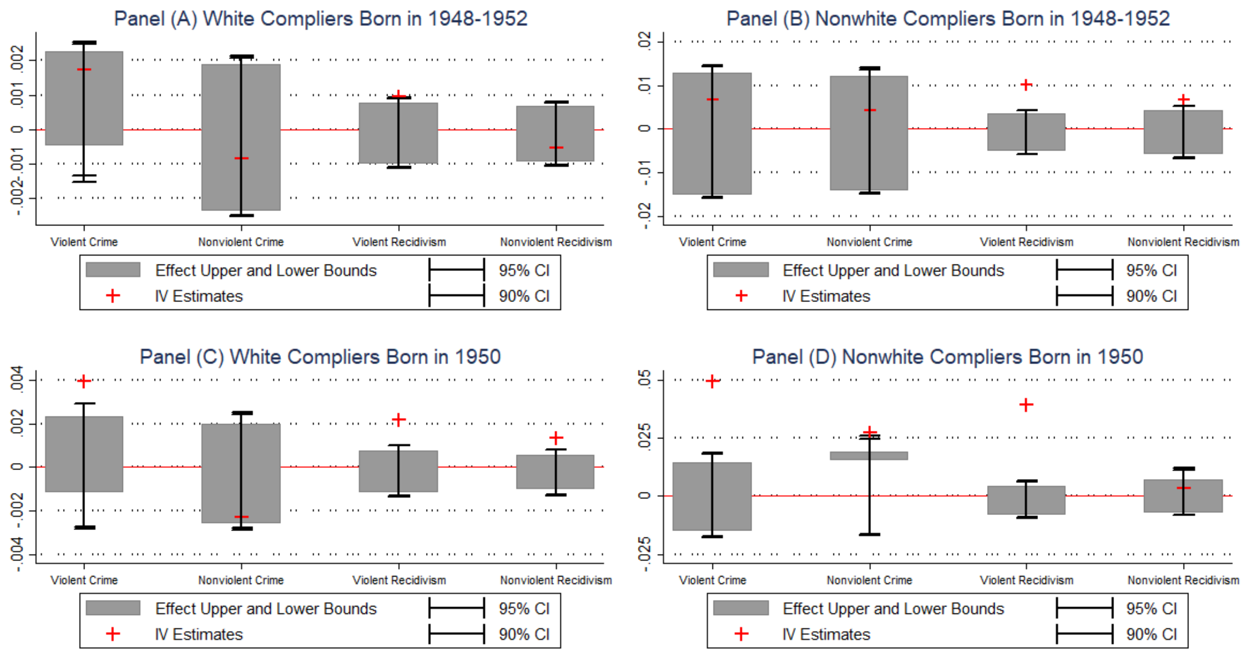

Figure 7. Estimated Bounds for the Local Average Treatment Effect of Military Service on the Incarceration Rates for Compliers in the Pooled 1950 Born White and Nonwhite Sample

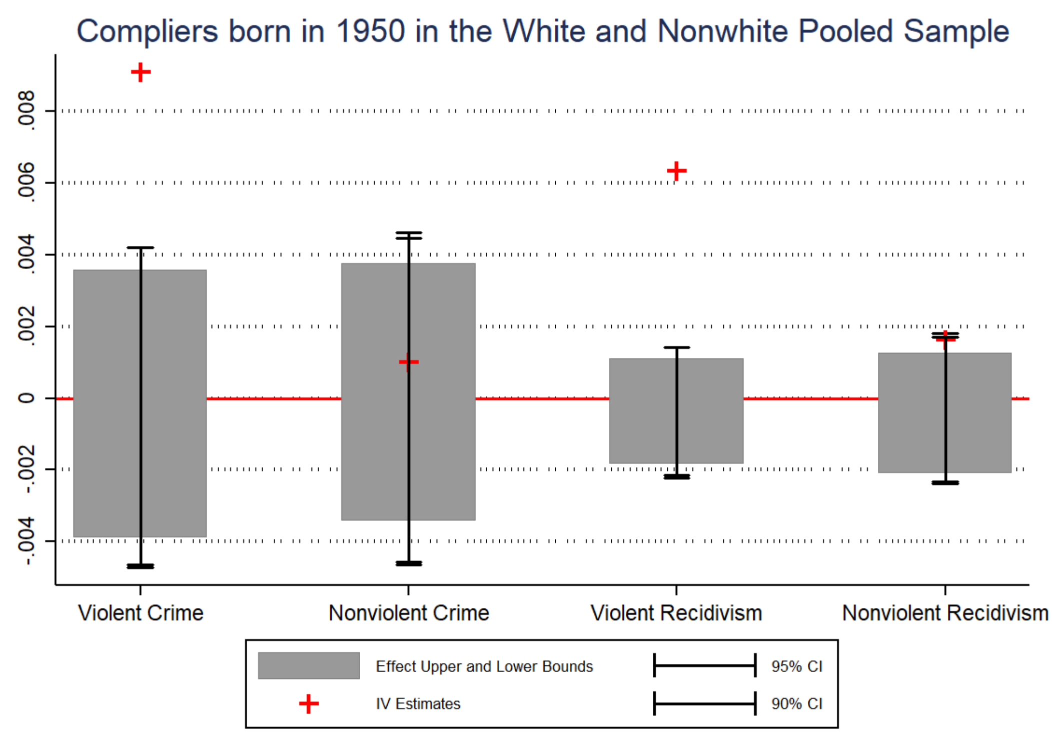

Figure 8. Estimated Bounds for the Local Average Treatment Effect of Military Service on the Incarceration Rates of White Always-takers

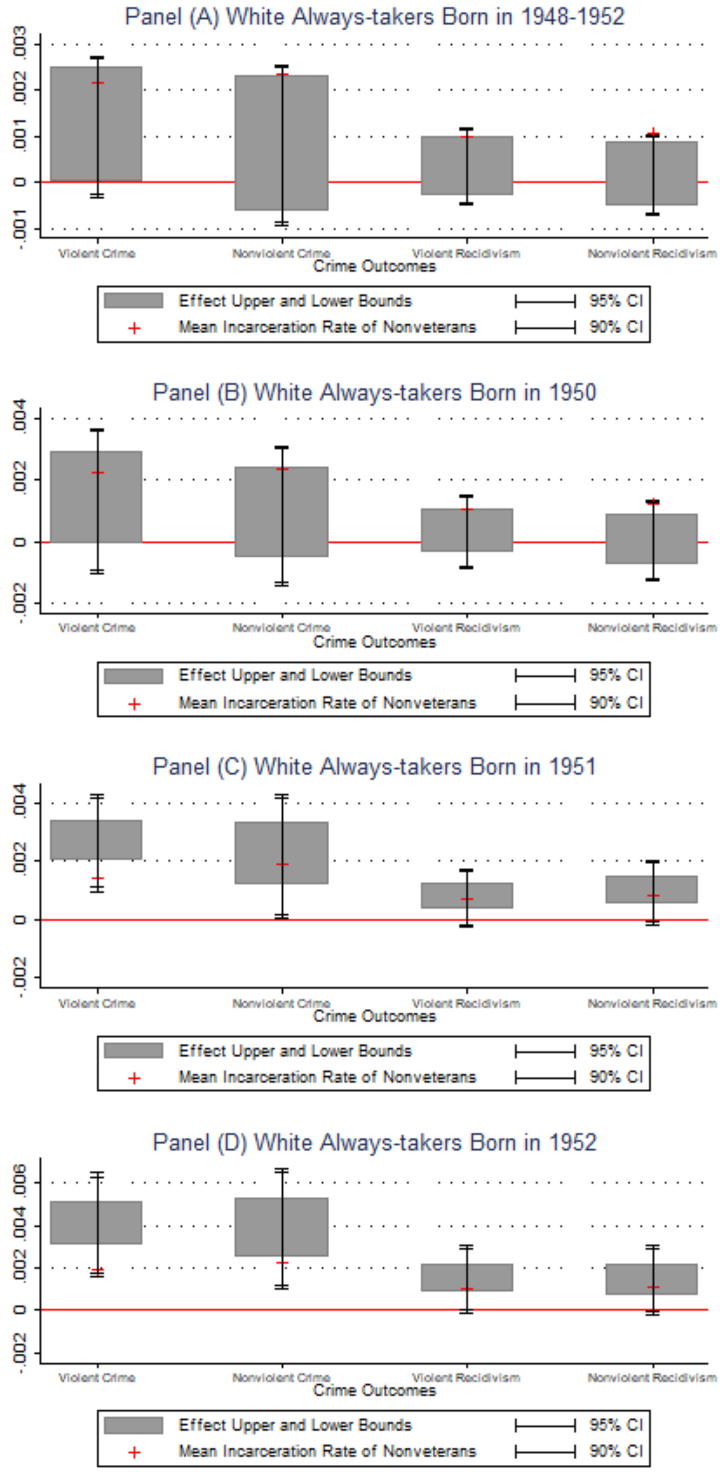

Figure 9. Estimated Bounds for the Local Average Treatment Effect of Military Service on the Incarceration Rates of Nonwhite Always-takers

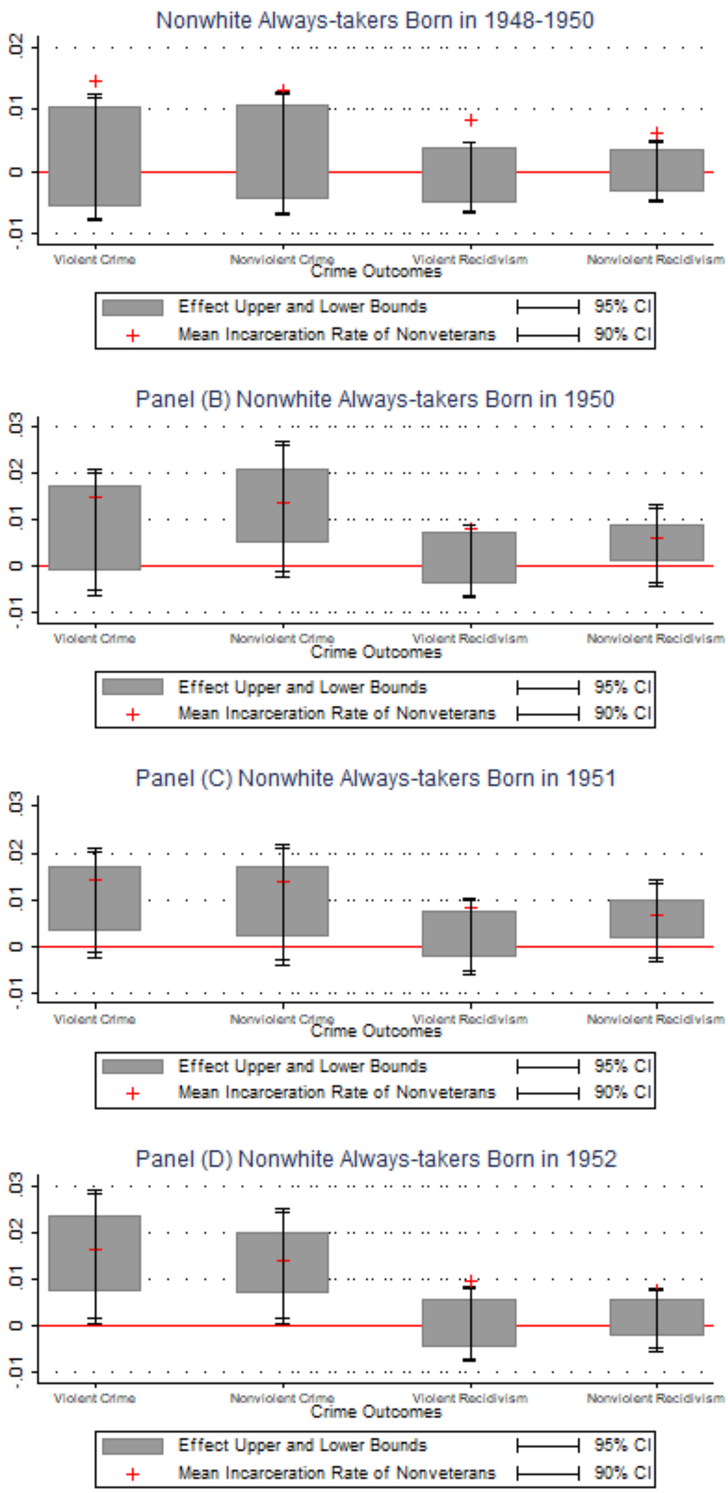

Table 9. Multiple Testing Results for the Military Service Effect of the Always-takers

(FWER Null Hypothesis H_0: $LATE_{at} = 0$ Estimates in any of the cohort born in 1948, 1949, 1950, 1951 or 1952; FDR Null Hypothesis H_0: $LATE_{at} = 0$ Estimates in all of the cohort born in 1948, 1949, 1950, 1951 or 1952)

	White Violent Offending Born 1951	White Violent Offending Born 1952	Nonwhite Violent Offending Born 1951	Nonwhite Violent Offending Born 1952
Sequential FWER	**R*** **	**R*** **	**NR**	**NR**
Sequential FDR	**R*** **	**R*** **	**NR**	**NR**
Sharp Sequential FDR	**R*** **	**R*** **	**NR**	**NR**
	White Nonviolent Offending Born 1951	White Nonviolent Offending Born 1952	Nonwhite Nonviolent Offending Born 1951	Nonwhite Nonviolent Offending Born 1952
Sequential FWER	**NR**	**R*** **	**NR**	**NR**
Sequential FDR	**NR**	**R*** **	**NR**	**NR**
Sharp Sequential FDR	**NR**	**R*** **	**NR**	**NR**
	White Nonviolent Recidivism Born 1951	White Nonviolent Recidivism Born 1952	Nonwhite Nonviolent Recidivism Born 1951	Nonwhite Nonviolent Recidivism Born 1952
Sequential FWER	**NR**	**NR**	**NR**	**NR**
Sequential FDR	**NR**	**NR**	**NR**	**NR**
Sharp Sequential FDR	**NR**	**NR**	**NR**	**NR**
	White Nonviolent Recidivism Born 1951	White Nonviolent Recidivism Born 1952	Nonwhite Nonviolent Recidivism Born 1951	Nonwhite Nonviolent Recidivism Born 1952
Sequential FWER	**NR**	**NR**	**NR**	**NR**
Sequential FDR	**NR**	**NR**	**NR**	**NR**
Sharp Sequential FDR	**NR**	**NR**	**NR**	**NR**

Note: "R" stands for rejection and "NR" stands for nonrejection; *, ** and *** indicate significance at 10%, 5% and 1% statistical significance levels, respectively.

Table 10. Characteristics of Always-takers Born in Different Years – Inmates Sample

	White birth cohorts			Nonwhite birth cohorts		
	1948-1950	**1951-1952**	**Difference**	**1948-1950**	**1951-1952**	**Difference**
Always-taker proportion	0.1973 (0.01679)	0.2984 (0.0177)	-0.1012*** (0.0244)	0.1467 (0.0152)	0.1757 (0.0139)	-0.0290 (0.0206)
Military service related characteristics (Obs: Whites 243; Nonwhites 159)						
Stationed in Vietnam	0.4221 (0.0639)	0.3294 (0.0404)	0.0927 (0.0756)	0.5321 (0.0705)	0.2399 (0.0493)	**0.2922*** (0.0860)**
Have Seen Combat during Military Service[33]	0.5018 (0.0910)	0.2228 (0.0533)	**0.2790*** (0.1055)**	0.5788 (0.1027)	0.1715 (0.0704)	**0.4073*** (0.1246)**
Served on or Before 1970	0.8950 (0.0281)	0.6975 (0.0331)	**0.1975*** (0.0435)**	0.9363 (0.0228)	0.4790 (0.0432)	**0.4573*** (0.0489)**
Drug use and juvenile criminal justice contact outcomes (Obs: Whites 299; Nonwhites 218)						
Ever Used Drug	0.7665 (0.0453)	0.9158 (0.0201)	**-0.1493*** (0.0495)**	0.8392 (0.0406)	0.8705 (0.0325)	-0.0313 (0.0521)
Age First Used Drug	17.6848 (0.4415)	17.2435 (0.2790)	0.4414 (0.5223)	18.8843 (0.6343)	16.2921 (0.4039)	**2.5922*** (0.7520)**
Using Drugs before the Current Offense	0.5755 (0.0513)	0.6085 (0.0391)	-0.0330 (0.0645)	0.4941 (0.0587)	0.6486 (0.0446)	**-0.1545** (0.0738)**
Juvenile Criminal Justice Contacts	0.3070 (0.0476)	0.4469 (0.0393)	**-0.1399** (0.0617)**	0.2915 (0.0528)	0.3330 (0.0446)	-0.0415 (0.0691)
Social economics characteristics (Obs: Whites 363; Nonwhites 244)[34]						
Abused Physically before 18-Year-Old	0.0509 (0.0226)	0.1218 (0.0509)	**-0.0708* (0.0374)**	0.0454 (0.0259)	0.0810 (0.0329)	-0.0355 (0.0419)
Highest grade father attended	12.6770 (0.7996)	10.4888 (0.4963)	**2.1882** (0.9411)**	10.6279 (0.8752)	10.1695 (0.7667)	0.4584 (1.1635)
Parent served in correctional facilities	0.0519 (0.0198)	0.0566 (0.0153)	-0.0047 (0.0250)	0.0401 (0.0200)	0.0952 (0.0259)	**-0.0551* (0.0328)**

Note: Standard errors based on 2500 bootstrap replications are in parentheses; *, ** and *** indicate significance at 10%, 5% and 1% statistical significance levels, respectively.

[33] The observations of the "have seen combat during military service" are 115 for the whites and 68 for the nonwhites.
[34] The observations of the "abused physically before 18-year-old" are 214 for the whites and 147 for the nonwhites.

Appendix 1

(NOT INTENDED FOR PUBLICATION)

Proofs of Propositions

This appendix section contains the proofs of the propositions in Section 3 of the paper. We present the proofs in a similar format in the online appendix of FF-L (2013).

Below we refer to the following equalities of which also appear in Section 3:

$$LMATE_k^z = E[Y(z, D_1) - Y(z, D_0)|k], \qquad \text{for } k = at, nt, c, d; \qquad \text{(A1.1)}$$

$$LNATE_k^Z = E[Y(1, D_z) - Y(0, D_z)|k], \qquad \text{for } k = at, nt, c, d; \qquad \text{(A1.2)}$$

$$E[Y|Z = 0, D = 0] = \frac{\pi_{nt}}{\pi_{nt}+\pi_c} \cdot E[Y(0)|nt] + \frac{\pi_c}{\pi_{nt}+\pi_c} \cdot E[Y(0)|c] \qquad \text{(A1.3)}$$

$$E[Y|Z = 1, D = 1] = \frac{\pi_{at}}{\pi_{at}+\pi_c} \cdot E[Y(1)|at] + \frac{\pi_c}{\pi_{at}+\pi_c} \cdot E[Y(1)|c] \qquad \text{(A1.4)}$$

By using Equation 6, the lower and upper bounds for $LATE_c^Z$ are derived by using the lower and upper bounds for $MATE^Z$. Following FF-L (2010), we use the point identified quantities and trimming bounds above as building blocks to construct bounds on $MATE^Z$ under $z = 1$ and $z = 0$ by writing it in different ways as a function of the local effects and average potential and counterfactual outcomes of the three strata. Similar in spirit of FF-L (2010), below we write $MATE^Z$ both under and not under exposure to the IV as

$$MATE^1 = \pi_c LMATE_c^1 \qquad \text{(A1.5)}$$

$$= \pi_{nt}E[Y(0)|nt] + \pi_{at}E[Y(0)|at] + \pi_c E[Y(1)|c] - \pi_c LNATE_c^0 - E[Y(0)] \qquad \text{(A1.6)}$$

$$= E[Y(1)] - \pi_{at}E[Y(1)|at] - \pi_{nt}E[Y(1)|nt] - \pi_c E[Y(1, D_0)|c] \qquad \text{(A1.7)}$$

$$= E[Y(1)] - E[Y(0)] - \pi_{at}LNATE_{at}^Z - \pi_{nt}LNATE_{nt}^Z - \pi_c LNATE_c^0 \qquad \text{(A1.8)}$$

$$MATE^0 = \pi_c LMATE_c^0 \qquad \text{(A1.9)}$$

$$= E[Y(1)] - \pi_{nt}E[Y(1)|nt] - \pi_{at}E[Y(1)|at] - \pi_c E[Y(0)|c] - \pi_c LNATE_c^1 \qquad \text{(A1.10)}$$

$$= \pi_{at}E[Y(0)|at] + \pi_{nt}E[Y(0)|nt] + \pi_c E[Y(0, D_1)|c] - E[Y(0)] \qquad \text{(A1.11)}$$

$$= E[Y(1)] - E[Y(0)] - \pi_{at}LNATE_{at}^Z - \pi_{nt}LNATE_{nt}^Z - \pi_c LNATE_c^1 \qquad \text{(A1.12)}$$

And $MATE^Z = \Pr(Z = 1) \cdot MATE^1 + \Pr(Z = 0) \cdot MATE^0$ (A1.13).

Under Assumption A1-A5, we partially identify $MATE^Z$ by plugging in the respective point estimates or bounds estimates of the components in (A1.5)-(A1.13). In later parts of this Appendix, we present the derivations and proofs for the bounds of $MATE^Z$.

To derive the lower and upper bounds for $LATE_{at}^{Z}$, we write $LATE_{at}^{Z}$ as $LATE_{at}^{Z} = \Pr(Z=1)\cdot(E[Y(1,1)|at]-E[Y(1,0)|at]) + \Pr(Z=0)\cdot(E[Y(0,1)|at]-E[Y(0,0)|at])$, then plug in the appropriate bounds derived into the terms that are not point identified or unobserved (i.e., $E[Y(1,1)|at]$, $E[Y(1,0)|at]$, and $E[Y(0,0)|at]$).

From Section 3 in the paper, the relevant point identified objects under Assumption A1-A3 in Section 3 are as follows: $\pi_{nt} = p_{0|1}$, $\pi_{at} = p_{1|0}$, $\pi_{c} = p_{1|1} - p_{1|0} = p_{0|0} - p_{0|1}$, $E[Y(1)] = E[Y|Z=1]$, $E[Y(0)] = E[Y|Z=0]$, $E[Y(1)|nt] = E[Y|Z=1, D=0]$, $E[Y(0)|at] = E[Y|Z=0, D=1]$, $\pi_{nt}E[Y(0)|nt] + \pi_{c}E[Y(0)|c] = p_{0|0}E[Y|Z=0, D=0]$ and $\pi_{at}E[Y(1)|at] + \pi_{c}E[Y(1)|c] = p_{1|1}E[Y|Z=1, D=1]$.

The trimming bounds on mean potential outcomes at the strata level under Assumptions A1-A3 are given by: $L^{0,nt} \leq E[Y(0)|nt] \leq U^{0,nt}$; $L^{1,at} \leq E[Y(1)|at] \leq U^{1,at}$; $L^{0,c} \leq E[Y(0)|c] \leq U^{0,c}$ and $L^{1,c} \leq E[Y(1)|c] \leq U^{1,c}$, where

$$L^{0,nt} = E\left[Y|Z=0, D=0, Y \leq y^{00}_{\frac{p_{0|1}}{p_{0|0}}}\right],\ U^{0,nt} = E\left[Y|Z=0, D=0, Y \geq y^{00}_{1-\frac{p_{0|1}}{p_{0|0}}}\right],$$

$$L^{1,at} = E\left[Y|Z=1, D=1, Y \leq y^{11}_{\frac{p_{1|0}}{p_{1|1}}}\right],\ U^{1,at} = E\left[Y|Z=1, D=1, Y \geq y^{11}_{1-\frac{p_{1|0}}{p_{1|1}}}\right],$$

$$L^{0,c} = E\left[Y|Z=0, D=0, Y \leq y^{00}_{1-\frac{p_{0|1}}{p_{0|0}}}\right],\ U^{0,c} = E\left[Y|Z=0, D=0, Y \geq y^{00}_{\frac{p_{0|1}}{p_{0|0}}}\right],$$

$$L^{1,c} = E\left[Y|Z=1, D=1, Y \leq y^{11}_{1-\frac{p_{1|0}}{p_{1|1}}}\right],\ U^{1,c} = E\left[Y|Z=1, D=1, Y \geq y^{11}_{\frac{p_{1|0}}{p_{1|1}}}\right],$$

The trimming bounds for $LNATE_{nt}^{Z}$ and $LNATE_{at}^{Z}$ in Proposition 1 can be derived by plugging in appropriate point estimates or trimming bounds of strata's mean potential outcomes from above.

Proof of Proposition 2. We start by deriving bounds for the non-point identified mean potential (and counterfactual) outcomes of the three strata (*at*, *c*, *nt*) and for all the local net and mechanism average treatment effects. To simply notations, we denote $E[Y|Z=z, D=d]$ as $\bar{Y}^{zd}$, where $z = 0,1$ and $d = 0,1$.

Bounds for $E[Y(0)|nt]$: Assumption A5(c), A5(d) and $\pi_{nt}\cdot E[Y(0)|nt] + \pi_{c}\cdot E[Y(0)|c] = p_{0|0}\,\bar{Y}^{00}$ imply that $E[Y(0)|nt] \geq \bar{Y}^{00}$ and $E[Y(0)|nt] \geq \bar{Y}^{01}$. Since $\bar{Y}^{00}$ can be bigger than, smaller than or equal to $\bar{Y}^{01}$ thus the lower bound for $E[Y(0)|nt]$ is $\max\{\bar{Y}^{00}, \bar{Y}^{01}\}$. A5 does not provide any additional information on the upper bound for $E[Y(0)|nt]$. Therefore, the lower bound and the upper bound under Assumption A1-A5 for $E[Y(0)|nt]$ is $\max\{\bar{Y}^{00}, \bar{Y}^{01}\} \leq E[Y(0)|nt] \leq U^{0,nt}$.

Bounds for $E[Y(1)|at]$: Assumption A5(c) and the equation $\pi_{at}\cdot E[Y(1)|at] + \pi_{c}\cdot E[Y(1)|c] = p_{1|1}\,\bar{Y}^{11}$ implies that the lower bound for $E[Y(1)|at]$ is $\bar{Y}^{11}$; Assumption A5(d) implies that the $E[Y(1)|at] \leq E[Y(1)|nt] = \bar{Y}^{10}$. Since $\bar{Y}^{10}$ can be bigger than, smaller than or equal to $U^{1,at}$, the upper for $E[Y(1)|at]$ is $\min\{\bar{Y}^{10}, U^{1,at}\}$. Therefore, the lower bound and the upper bound under Assumption A1-A5 for $E[Y(1)|at]$ is $\bar{Y}^{11} \leq E[Y(1)|at] \leq \min\{\bar{Y}^{10}, U^{1,at}\}$.

Bounds for $E[Y(0)|c]$: Assumption A5 does not provide any additional information on the lower bound for $E[Y(0)|c]$. Assumption A5(c) and A5(d) and the equation $\pi_{nt} \cdot E[Y(0)|nt] + \pi_c \cdot E[Y(0)|c] = p_{0|0}\, \bar{Y}^{00}$ imply that $E[Y(0)|c] \leq \bar{Y}^{00}$ and $E[Y(0)|c] \leq E[Y(0)|at] = \bar{Y}^{01}$. Since $\bar{Y}^{00}$ can be bigger than, smaller than or equal to $\bar{Y}^{01}$, the upper bound for $E[Y(0)|c]$ is $\min\{\bar{Y}^{00}, \bar{Y}^{01}\}$. To summarize, the lower and upper bounds for $E[Y(0)|c]$ is $L^{0,c} \leq E[Y(0)|c] \leq \min\{\bar{Y}^{00}, \bar{Y}^{01}\}$.

Bounds for $E[Y(1)|c]$: Assumption A5 does not provide any additional information on the lower bound for $E[Y(1)|c]$. Assumption A5(c), A5(d) and the equation $\pi_{nt} \cdot E[Y(1)|at] + \pi_c \cdot E[Y(1)|c] = p_{0|0}\, \bar{Y}^{11}$ imply that $E[Y(1)|c] \leq \bar{Y}^{11}$ and $E[Y(1)|c] \leq \bar{Y}^{10}$. And since $\bar{Y}^{11} \leq \bar{Y}^{10}$ by Assumption A5(c) and A5(d), the upper bound for $E[Y(1)|c]$ is $\bar{Y}^{11}$. To summarize, the lower and upper bounds for $E[Y(1)|c]$ is $L^{1,c} \leq E[Y(1)|c] \leq \bar{Y}^{11}$.

Bounds for $E[Y(1, D_0)|c]$: Assumption A4 implies that $E[Y(1, D_0)|c] \geq y^l$. Assumption A5 does not provide additional information to the lower bound for $E[Y(1, D_0)|c]$; Assumption A5(a) and A5(d) imply that $E[Y(1, D_0)|c] \leq E[Y(1)|at] \leq E[Y(1)|nt]$, and therefore $E[Y(1, D_0)|c] \leq U^{1,at}, and\ E[Y(1, D_0)|c] \leq \bar{Y}^{10}$. Since $U^{1,at}$ can be larger than, smaller than or equal to $\bar{Y}^{10}$, the upper bound for $E[Y(1, D_0)|c]$ is $\min\{U^{1,at}, \bar{Y}^{10}\}$. To summarize, the lower and upper bounds for $E[Y(1, D_0)|c]$ under Assumption A1-A5 is $y^l \leq E[Y(1, D_0)|c] \leq \min\{U^{1,at}, \bar{Y}^{10}\}$.

Bounds for $E[Y(0, D_1)|c]$: Assumption A4 implies that $E[Y(0, D_1)|c] \geq y^l$. Assumption A5 does not provide additional information to the lower bound for $E[Y(0, D_1)|c]$; Assumption A5(b) and A5(d) imply that $E[Y(0, D_1)|c] \leq E[Y(0)|at] \leq E[Y(0)|nt]$. Since $\bar{Y}^{01} \leq U^{0,nt}$, the upper bund for $E[Y(0, D_1)|c]$ is $\bar{Y}^{01}$. To summarize, the lower and upper bounds for $E[Y(0, D_1)|c]$ is $y^l \leq E[Y(0, D_1)|c] \leq \bar{Y}^{01}$.

Bounds for $LNATE_{nt}^{Z}$, for $z = 0, 1$: $E[Y(1)|nt]$ is point identified as $\bar{Y}^{10}$. Under the Assumptions A1-A5, the lower and upper bounds for $E[Y(0)|nt]$ are $\max\{\bar{Y}^{00}, \bar{Y}^{01}\} \leq E[Y(0)|nt] \leq U^{0,nt}$. By plugging in appropriate components, under Assumptions A1-A5, $\bar{Y}^{10} - U^{0,nt} \leq LNATE_{nt}^{Z} \leq \bar{Y}^{10} - \max\{\bar{Y}^{00}, \bar{Y}^{01}\}$.

Bounds for $LNATE_{at}^{Z}$, for $z = 0, 1$: $E[Y(0)|at]$ is point identified as $\bar{Y}^{01}$. By plugging in corresponding components, under Assumption A1-A5, $\bar{Y}^{11} - \bar{Y}^{01} \leq LNATE_{at}^{Z} \leq \min\{\bar{Y}^{10}, U^{1,at}\} - \bar{Y}^{01}$.

Bounds for $LNATE_{c}^{0}$: By plugging in appropriate components, under Assumptions A1-A5, $y^l - \min\{\bar{Y}^{00}, \bar{Y}^{01}\} \leq LNATE_{c}^{0} \leq \min\{\bar{Y}^{10}, \bar{U}^{1,at}\} - L^{0,c}$.

Bounds for $LNATE_{c}^{1}$: By plugging in appropriate components, under Assumptions A1-A5, $L^{1,c} - \bar{Y}^{01} \leq LNATE_{c}^{1} \leq \bar{Y}^{11} - y^l$.

Bounds for $LMATE_c^0$: By plugging in appropriate components, under Assumptions A1-A5, $y^l - \min\{\bar{Y}^{00}, \bar{Y}^{01}\} \leq LMATE_c^0 \leq \bar{Y}^{01} - L^{0,c}$.

Bounds for $LMATE_c^1$: By plugging in appropriate components, under Assumptions A1-A5, $L^{1,c} - \min\{\bar{Y}^{10}, U^{1,at}\} \leq LMATE_c^1 \leq \bar{Y}^{11} - y^l$.

Bounds for $E[Y(1{,}0)|at]$: Assumption A4 implies that $E[Y(1{,}0)|at] \geq y^l$. Assumption A5 does not contribute additional information to the lower bound of $E[Y(1{,}0)|at]$. Assumption A5(f) implies that $E[Y(1{,}0)|at] \leq E[Y(1)|nt]$ and therefore $E[Y(1{,}0)|at] \leq \bar{Y}^{10}$. To summarize, the lower and upper bounds for $E[Y(1{,}0)|at]$ is $y^l \leq E[Y(1{,}0)|at] \leq \bar{Y}^{10}$. And by plugging in appropriate components, $\bar{Y}^{11} - \bar{Y}^{10} \leq E[Y(1)|at] - E[Y(1{,}0)|at] \leq \min\{\bar{Y}^{10}, U^{1,at}\} - y^l$.

Bounds for $E[Y(0{,}0)|at]$: Assumption A4 implies that $E[Y(0{,}0)|at] \geq y^l$. Assumption A5(e) and A5(f) imply that $E[Y(0)|c] \leq E[Y(0{,}0)|at] \leq E[Y(0)|nt]$ and therefore $L^{0,c} \leq E[Y(0{,}0)|at] \leq U^{0,nt}$ (as $L^{0,c} \geq y^l$). And $\bar{Y}^{01} - U^{0,nt} \leq E[Y(0)|at] - E[Y(0{,}0)|at] \leq \bar{Y}^{01} - L^{0,c}$.

We now derive the bounds for *MATE*. We first use Equations A1.5-A1.8 to derive potential lower bounds for $MATE^l$ by plugging in the appropriate bounds derived above into the terms that are not point identified. The corresponding four lower bounds candidates are,

$$\Delta_1^1 = \pi_c \cdot (L^{1,c} - \min\{U^{1,at}, \bar{Y}^{10}\})$$

$$\Delta_2^1 = \pi_{nt} \cdot \max\{\bar{Y}^{00}, \bar{Y}^{01}\} + \pi_{at} \cdot \bar{Y}^{01} + \pi_c \cdot L^{1,c} - \pi_c \cdot [\min\{U^{1,at}, \bar{Y}^{10}\} - L^{0,c}] - E[Y|Z=0]$$

$$\Delta_3^1 = E[Y|Z=1] - \pi_{at} \cdot \min\{U^{1,at}, \bar{Y}^{10}\} - \pi_{nt} \cdot \bar{Y}^{10} - \pi_c \cdot \min\{U^{1,at}, \bar{Y}^{10}\}$$

$$\Delta_4^1 = E[Y|Z=1] - E[Y|Z=0] - \pi_{at} \cdot \min\{U^{1,at}, \bar{Y}^{10}\} + \pi_{at} \cdot \bar{Y}^{01} - \pi_{nt} \cdot [\bar{Y}^{10} - \max\{\bar{Y}^{00}, \bar{Y}^{01}\}] - \pi_c \cdot [\min\{\bar{Y}^{10}, U^{1,at}\} - L^{0,c}]$$

After some algebra, we have $\Delta_1^1 - \Delta_2^1 = \pi_{nt} \cdot U^{0,nt} - \pi_{nt} \cdot \max\{\bar{Y}^{00}, \bar{Y}^{01}\} \geq 0$ and therefore $\Delta_1^1 \geq \Delta_2^1$; $\Delta_1^1 - \Delta_3^1 = \pi_c \cdot \min\{U^{1,at}, \bar{Y}^{10}\} - \pi_c \cdot U^{1,at} \leq 0$ and therefore $\Delta_3^1 \geq \Delta_1^1$; $\Delta_1^1 - \Delta_4^1 = \pi_{at} \cdot \min\{U^{1,at}, \bar{Y}^{10}\} - \pi_{at} \cdot \bar{Y}^{11} + \pi_{nt} \cdot \bar{Y}^{00} - \pi_{nt} \cdot \max\{\bar{Y}^{00}, \bar{Y}^{01}\} + \pi_c \cdot \bar{Y}^{00} - \pi_c \cdot L^{0,c} \geq 0$ as $\pi_{at} \cdot \min\{U^{1,at}, \bar{Y}^{10}\} - \pi_{at} \cdot \bar{Y}^{11} \geq 0$, $\pi_{nt} \cdot \bar{Y}^{00} - \pi_{nt} \cdot \max\{\bar{Y}^{00}, \bar{Y}^{01}\} \geq 0$, and $\pi_c \cdot \bar{Y}^{00} - \pi_c \cdot L^{0,c} \geq 0$, and therefore $\Delta_1^1 \geq \Delta_4^1$. To summarize, the lower bound for $MATE^l$ is Δ_3^1.

Second, for the upper bounds for $MATE^l$, using Equations A1.5-A1.8 we write down the four candidate upper bounds as follows.

$$\Upsilon_1^1 = \pi_c \cdot (\bar{Y}^{11} - y^l)$$

$$\Upsilon_2^1 = \pi_{nt} \cdot U^{0,nt} + \pi_{at} \cdot \bar{Y}^{01} + \pi_c \cdot \bar{Y}^{11} - \pi_c \cdot [0 - \min\{\bar{Y}^{00}, \bar{Y}^{01}\}] - E[Y|Z=0]$$

$$\Upsilon_3^1 = E[Y|Z=1] - \pi_{at} \cdot \bar{Y}^{11} - \pi_{nt} \cdot \bar{Y}^{10} - 0$$

$$\Upsilon_4^1 = E[Y|Z=1] - E[Y|Z=0] - \pi_{at} \cdot [\bar{Y}^{11} - \bar{Y}^{01}] - \pi_{nt} \cdot [\bar{Y}^{10} - \mathrm{U}^{0,\mathrm{nt}}] - \pi_c \cdot (0 - \min\{\bar{Y}^{00}, \bar{Y}^{01}\})$$

After some algebra, we have $\Upsilon_1^1 - \Upsilon_2^1 = \pi_c \cdot L^{0,c} - \pi_c \cdot \min\{\bar{Y}^{00}, \bar{Y}^{01}\} \leq 0$ and therefore $\Upsilon_1^1 \leq \Upsilon_2^1$; $\Upsilon_1^1 - \Upsilon_3^1 = \pi_c \cdot \bar{Y}^{11} - E[Y|Z=1] + \pi_{at} \cdot \bar{Y}^{11} + \pi_{nt} \cdot \bar{Y}^{10} = 0$ and therefore $\Upsilon_1^1 = \Upsilon_3^1$; $\Upsilon_1^1 - \Upsilon_4^1 = \pi_c \cdot L^{0,c} - \pi_c \cdot \min\{\bar{Y}^{00}, \bar{Y}^{01}\} \leq 0$ and therefore $\Upsilon_1^1 \leq \Upsilon_4^1$. Therefore the upper bound for *MATE^1* is Υ_1^1 or Υ_3^1.

We then move on to *MATE^0*, using Equations A1.9-A1.12 and by plugging in the appropriate bounds derived into the terms that are not point identified, we have the following candidate for the lower bounds.

$\Delta_1^0 = \pi_c \cdot (y^l - \min\{\bar{Y}^{00}, \bar{Y}^{01}\})$

$\Delta_2^0 = E[Y|Z=1] - \pi_{nt} \cdot \bar{Y}^{10} - \pi_{at} \cdot \min\{\bar{Y}^{10}, U^{1,at}\} - \pi_c \cdot \min\{\bar{Y}^{00}, \bar{Y}^{01}\} - \pi_c \cdot (\bar{Y}^{11} - y^l)$

$\Delta_3^0 = \pi_{at} \cdot \bar{Y}^{01} + \pi_{nt} \cdot \max\{\bar{Y}^{00}, \bar{Y}^{01}\} + \pi_c \cdot y^l - E[Y|Z=0]$

$\Delta_4^0 = E[Y|Z=1] - E[Y|Z=0] - \pi_{at} \cdot (\min\{U^{1,at}, \bar{Y}^{10}\} - \bar{Y}^{01}) - \pi_{nt} \cdot (\bar{Y}^{10} - \max\{\bar{Y}^{00}, \bar{Y}^{01}\}) - \pi_c \cdot [\bar{Y}^{11} - y^l]$

After some algebra, we have $\Delta_1^0 - \Delta_2^0 = \pi_{at} \cdot \min\{\bar{Y}^{10}, U^{1,at}\} - \pi_{at} \cdot \bar{Y}^{11} \geq 0$, and therefore $\Delta_1^0 \geq \Delta_2^0$; $\Delta_3^0 - \Delta_4^0 = \pi_{at} \cdot \min\{\bar{Y}^{10}, U^{1,at}\} - \pi_{at} \cdot \bar{Y}^{11} \geq 0$, and therefore $\Delta_3^0 \geq \Delta_4^0$; $\Delta_1^0 - \Delta_3^0 = \pi_c \cdot \bar{Y}^{00} - \pi_c \cdot \min\{\bar{Y}^{00}, \bar{Y}^{01}\} + \pi_{nt} \cdot \bar{Y}^{00} - \pi_{nt} \cdot \max\{\bar{Y}^{00}, \bar{Y}^{01}\}$. Since $\pi_c \cdot \bar{Y}^{00} - \pi_c \cdot \min\{\bar{Y}^{00}, \bar{Y}^{01}\} \geq 0$ and $\pi_{nt} \cdot \bar{Y}^{00} - \pi_{nt} \cdot \max\{\bar{Y}^{00}, \bar{Y}^{01}\} \leq 0$, therefore, Δ_1^0 can be larger than, equal to and smaller than Δ_3^0. To summarize, the lower bound for *MATE^0* under Assumptions A1-A5 is $\max\{\Delta_1^0, \Delta_3^0\}$.

Last, we derive the upper bounds candidates for *MATE^0* using Equations A1.9-A1.12. They are.

$\Upsilon_1^0 = \pi_c \cdot (\bar{Y}^{01} - L^{0,c})$

$\Upsilon_2^0 = E[Y|Z=1] - \pi_{nt} \cdot \bar{Y}^{10} - \pi_{at} \cdot \bar{Y}^{11} - \pi_c \cdot \mathrm{L}^{0,c} - \pi_c \cdot (L^{1,c} - \bar{Y}^{01})$

$\Upsilon_3^0 = \pi_{at} \cdot \bar{Y}^{01} + \pi_{nt} \cdot \mathrm{U}^{0,nt} + \pi_c \cdot \bar{Y}^{01} - E[Y|Z=0]$

$\Upsilon_4^0 = E[Y|Z=1] - E[Y|Z=0] - \pi_{at} \cdot [\bar{Y}^{11} - \bar{Y}^{01}] - \pi_{nt} \cdot [\bar{Y}^{10} - \mathrm{U}^{0,nt}] - \pi_c \cdot (L^{1,c} - \bar{Y}^{01})$

After some algebra, we have $\Upsilon_1^0 - \Upsilon_2^0 = \pi_c \cdot L^{1,c} - \pi_c \cdot \bar{Y}^{11} \leq 0$, and therefore $\Upsilon_1^0 \leq \Upsilon_2^0$; $\Upsilon_1^0 - \Upsilon_3^0 = 0$, and therefore $\Upsilon_1^0 = \Upsilon_3^0$; $\Upsilon_1^0 - \Upsilon_4^0 = \pi_c \cdot L^{1,c} - \pi_c \cdot \bar{Y}^{11} \leq 0$, and therefore $\Upsilon_1^0 \leq \Upsilon_4^0$. To summarize, the upper bound for *MATE^0* under Assumptions A1-A5 is Υ_1^0 or Υ_3^0.

In summary from above, the lower bound for $MATE^Z$ under Assumptions A1-A5 is $\Pr(Z=1) \cdot \Delta_3^1 + \Pr(Z=0) \cdot \min\{\Delta_1^0, \Delta_3^0\}$; the upper bound for $MATE^Z$ under Assumptions A1-A5 is $\Pr(Z=1) \cdot \Upsilon_1^1 + \Pr(Z=0) \cdot \Upsilon_1^0$ (Υ_1^1 and Υ_1^0 can be replaced by Υ_3^1 and Υ_3^0 respectively).

Based on that $E[Y(1) - Y(0)] = NATE^z + MATE^z$, the bounds for $NATE^z$ under Assumptions A1-A5 is $E[Y|Z=1] - E[Y|Z=0] - \Pr(Z=0) \cdot \Upsilon_1^1 - \Pr(Z=1) \cdot \Upsilon_1^0 \leq NATE^z \leq E[Y|Z=1] - E[Y|Z=0] - \Pr(Z=0) \cdot \Delta_3^1 - \Pr(Z=1) \cdot \min\{\Delta_1^0, \Delta_3^0\}$.

Following Equation 9 in the main text, the lower bound for $LATE_c^Z$ is $\frac{\Pr(Z=1)\cdot\Delta_3^1+\Pr(Z=0)\cdot\min\{\Delta_1^0,\Delta_3^0\}}{E[D|Z=1]-E[D|Z=0]} \leq LATE^Z \leq \frac{\Pr(Z=1)\cdot\Upsilon_1^1+\Pr(Z=0)\cdot\Upsilon_1^0}{E[D|Z=1]-E[D|Z=0]}$.

Finally, we derive bounds for $LATE_{at}^Z$ in Proposition 2 by plugging in appropriate bounds and point estimates that we derived earlier. The bounds for $LATE_{at}^Z$ is $\Pr(Z=1)\cdot(\bar{Y}^{11}-\bar{Y}^{10}) + \Pr(Z=0)\cdot(\bar{Y}^{01}-U^{0,nt}) \leq LATE_{at}^Z \leq \Pr(Z=1)\cdot(\min\{\bar{Y}^{10},U^{1,at}\}-y^l)+\Pr(Z=0)\cdot(\bar{Y}^{01}-L^{0,c})$.

Appendix 2. Estimation Biases in the Pre-Draft Characteristics Analysis
(NOT INTENDED FOR PUBLICATION)

Ideally, the pre-draft characteristics are computed using individual level data of the total male population, which includes both the incarcerated and non-incarcerated population. For example, the mean characteristics of the racial variable white for strata k_1 can be expressed as follows.

$$\begin{aligned}
E[white|k_1] &= 1 \cdot \Pr[incarceration = 1|k_1] \cdot \Pr[white = 1|incarceration = 1, k_1] \\
&+1 \cdot \Pr[incarceration = 0|k_1] \cdot \Pr[white = 1|incarceration = 0, k_1] \\
&+0 \cdot \Pr[incarceration = 1|k_1] \cdot \Pr[white = 0|incarceration = 1, k_1] \\
&+0 \cdot \Pr[incarceration = 0|k_1] \cdot \Pr[white = 0|incarceration = 0, k_1] \\
&= \Pr[incarceration = 1|k_1] \cdot \Pr[white = 1|incarceration = 1, k_1] \\
&+ \Pr[incarceration = 0|k_1] \cdot \Pr[white = 1|incarceration = 0, k_1]
\end{aligned}$$

(Equation A2.1)

And the difference between two strata k_1 and k_2 can be expressed as,

$$\begin{aligned}
E[white|k_1] - E[white|k_2] &= \Pr[incarceration = 1|k_1] \cdot \Pr[white = 1|incarceration = 1, k_1] \\
&+ \Pr[incarceration = 0|k_1] \cdot \Pr[white = 1|incarceration = 0, k_1] \\
&- \Pr[incarceration = 1|k_2] \cdot \Pr[white = 1|incarceration = 1, k_2] \\
&- \Pr[incarceration = 0|k_2] \cdot \Pr[white = 1|incarceration = 0, k_2]
\end{aligned}$$

(Equation A2.2)

In lack of individual level data, the first method to compute these pre-draft characteristics is by using the inmate individual level data only; in another word, we use $\Pr[white = 1|incarceration = 1, k_i]$ in replace of $\Pr[white = 1|incarceration = 0, k_i]$ $i = 1,2$, then the estimated mean difference is,

$$\begin{aligned}
E[\widehat{white}|k_1] - E[\widehat{white}|k_2] &= \Pr[incarceration = 1|k_1] \cdot \Pr[white = 1|incarceration = 1, k_1] \\
&+ \Pr[incarceration = 0|k_1] \cdot \Pr[white = 1|incarceration = 1, k_1] \\
&- \Pr[incarceration = 1|k_2] \cdot \Pr[white = 1|incarceration = 1, k_2] \\
&- \Pr[incarceration = 0|k_2] \cdot \Pr[white = 1|incarceration = 1, k_2] \\
&= \Pr[white = 1|incarceration = 1, k_1] - \Pr[white = 1|incarceration = 1, k_2]
\end{aligned}$$

(Equation A2.3)

The bias of the estimated mean difference using the first method is

$$\alpha_1 = \left(E[\widehat{white}|k_1] - E[\widehat{white}|k_2]\right) - (E[white|k_1] - E[white|k_2])$$

$$\begin{aligned} = \Pr[incarceration = 0|k_1] \\ \cdot (\Pr[white = 1|incarceration = 1, k_1] \\ - \Pr[white = 1|incarceration = 0, k_1]) \\ - \Pr[incarceration = 0|k_2] \\ \cdot (\Pr[white = 1|incarceration = 1, k_2] \\ - \Pr[white = 1|incarceration = 0, k_2]) \end{aligned}$$

(Equation A2.4)

Intuitively, the bias consists of the difference in the difference of the characteristics' means between the incarcerated sample and the non-incarcerated sample of stratum k_1 and k_2.

The second method to compute the mean characteristics for strata k_1 and k_2 is by estimating the first product term of the last equation in Equation A2.1. The estimated mean difference is,

$$\begin{aligned} E[\widehat{white}|k_1] - E[\widehat{white}|k_2] = \Pr[incarceration = 1|k_1] \\ \cdot \Pr[white = 1|incarceration = 1, k_1] - \Pr[incarceration = 1|k_2] \\ \cdot \Pr[white = 1|incarceration = 1, k_2] \end{aligned}$$

$$= \Pr[white = 1 \,\&\, incarceration = 1|k_1] - \Pr[white = 1 \,\&\, incarceration = 1|k_2]$$

(Equation A2.5)

And the potential bias under the second method is,

$$\alpha_2 = \left(E[\widehat{white}|k_1] - E[\widehat{white}|k_2]\right) - (E[white|k_1] - E[white|k_2])$$

$$= \Pr[incarceration = 0|k_2] \cdot \Pr[white = 1|incarceration = 0, k_2] - \Pr[incarceration = 0|k_1] \cdot \Pr[white = 1|incarceration = 0, k_1]$$

(Equation A2.6)

which can be interpreted as the differences in mean of the characteristics between strata k_1 and k_2 in the non-incarcerated population.