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ABSTRACT

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

Employing nonparametric bounds, we examine the effect of military service on incarceration outcomes using the Vietnam draft lotteries as a possibly invalid instrumental variable for military service. The draft is allowed to have a direct effect on the outcomes independently of military service, disposing of the exclusion restriction. We find: (i) suggestive but not strong statistical evidence that the direct effect of the draft increases the incarceration rate for violent offenses for a particular cohort of draft avoiders, and (ii) military service increases the incarceration rate for violent and nonviolent crimes of white volunteers and veterans in certain birth cohorts.

JEL Classification:	K4, C31, C36
Keywords:	conscription, military service, incarceration, crime, nonparametric bounds

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I. Introduction

Recent estimates suggest that over 181,500 veterans are in the U.S. jails and prisons, and that veterans are 16% more likely to be sentenced for violent crimes than non-veterans (Bureau of Justice Statistics, 2015). The U.S. Department of Veteran Affairs (VA) has funded programs such as the Veteran Justice Outreach and the Veteran Treatment Courts to support the veterans' reintegration to society and prevent them from landing in prisons and jails. This paper seeks to contribute to our understanding of the relationship between conscription, military service, incarceration, and recidivism, with the aim of informing VA and U.S. policies to reduce veterans' incarceration and recidivism. We employ recently developed nonparametric bounds that allow examining the causal effects of the Vietnam war military service under relatively mild conditions. They also allow conducting inference on all veterans, including those that volunteered for service, a group that should be informative about the current U.S. all-volunteer forces (AVF).

Empirical studies that estimate the causal effects of military service on a myriad of post-service outcomes often use the Vietnam draft lotteries as a source of exogenous variation—that is, as an instrumental variable (IV)—for the Vietnam era military service (for instance, Angrist, 1990; Dobkin and Shabani, 2009; Angrist et al., 2010; Heerwig and Conley, 2013). This approach overcomes the missing counterfactual outcome problem (for instance, 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 a decision typically made by the individual. For instance, 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 draft lotteries generate exogenous variation in military service because military induction was based on a Random Sequence Number (RSN) that was assigned to potential draftees based solely on birthdays, making it independent of individuals' 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., with no noticeable effects for nonwhites.

This conventional IV approach has limitations. One limitation relates to the requirement that the draft lotteries eligibility (the IV) not affect the incarceration outcomes in any way except

through the indirect mechanism of military service. This requirement is known as the exclusion restriction assumption (ER). Unfortunately, it is plausible that the ER is not satisfied in the context of the Vietnam war draft lotteries. The leading factors for the potential direct (or net-of-military-service) effect of the draft-eligibility are draft avoidance behaviors. As an example, Kuziemko (2010) suggested the notion of "dodging down" as an avoidance behavior consisting of delinquencies and criminal activities, because having a criminal record was a way to avoid being drafted into military service by failing the military induction's "moral standards" (Suttler, 1970; Shapiro and Striker, 1970).[4] Besides, being drafted and refusing to serve also lead to convictions and prison sentences for draft offenders following the draft law (Baskir and Strauss, 1978). Peterson (1998) documents that almost half of the 570,000 traceable draft avoiders during the U.S. Vietnam war became accused draft offenders, and around 22,000 of them were convicted after being brought to trial. 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). Therefore, if those behaviors relate to future incarceration, they result in a direct effect of the draft lotteries (net of military service) on future incarceration. Under violations of the ER, the IV estimate of the effect of military service will have a bias of the same sign as the direct effect of the draft lotteries, independently of the sign of the effect of military service on incarceration.[5]

Another limitation of the conventional IV approach is that it solely provides estimates of the military service effect for individuals who would serve only when draft-eligible (that is, the "compliers"). Thus, the IV approach fails to provide estimates of the effect of military service for the volunteers (those who will serve regardless of draft-eligibility, or "always-takers") and consequently for all of those who serve (the treated population) that consists of both of these groups. The group of volunteers is important for at least three reasons. First, they represent a high proportion of the veteran population—between 78% and 84% of all servicemen from the cohorts (1948 to 1952) affected by the draft. Second, the group of volunteers may be of particular interest

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[4] Other studies have documented the effects on education of another type of draft avoidance behavior–using the education exemption and enrolling in universities to avoid the draft (Card and Lemieux, 2001; Deuchert and Huber, 2017).

[5] Lindo and Stoecker (2014) indeed recognize that the violation of the ER is one potential concern for the validity of their estimates using the draft lotteries as the IV for military service, and perform an indirect assessment of this possibility.

given that the U.S. military works under an AVF system since 1973. Insights about volunteers rather than about compliers are likely to be more relevant to the current U.S. military. Lastly, learning about the military effect of volunteers allows learning about the overall population of veterans, which encompasses both compliers and volunteers. Effects on treated individuals are of first-order importance for policy (see, for instance, Heckman et al., 1999).

To address the above limitations of the conventional IV approach, we employ recently developed nonparametric bounds in Flores and Flores-Lagunes (2013; hereafter FF-L) and Chen, Flores, and Flores-Lagunes et al. (2019; hereafter CFF-L). These bounds allow conducting inference on the effect of military service on incarceration and recidivism outcomes for the draft volunteers and the veteran population. They also allow for potential violations of the ER by the IV since they permit the draft lotteries to have a direct effect on the incarceration outcomes through channels net of their impact on military service. Intuitively, the methodology separates the total effect of the draft lotteries on the outcomes into an indirect (mechanism) effect that works through military service, and a direct (net) effect that does not work through military service. They provide bounds on each of those two effects, and the bounds on the indirect effect of the lottery draft—the only effect assumed to exist under the ER—are used to bound the military effect for volunteers, compliers, and veterans. For some of the bounds we employ, the ER is replaced with a mean weak monotonicity assumption that we describe and assess in detail later.

A challenge in estimating those bounds on effects on incarceration and recidivism outcomes is that individual-level data that is representative of the U.S. male population, and that contains the necessary information to determine lottery draft eligibility, is not publicly available. For this reason, we construct population-level incarceration rates by adapting 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 birth statistics from the Vital Statistics of the United States (VSUS) 1948-1952, and nationally representative estimates from Wang et al. (2020) of the population proportions of volunteers, compliers and draft avoiders.

We have three main sets of results. The first concerns the effects of conscription (the lottery drafts), net of military service. The estimated bounds on the direct effect of the draft lotteries for the draft avoiders born in 1948-1952 are suggestive of positive effects on the incarceration and recidivism outcomes considered, although their 90%-level confidence intervals do not statistically

rule out a zero effect. Considering that the behavioral responses by draft avoiders born in different years are plausibly different since the conscription circumstances changed (as discussed in section V.A.), we explored the heterogeneity of the direct effect by birth-year cohorts. Consistent with the relatively short reaction time to the draft that males born in 1950 had, likely limiting their behavioral avoidance responses (for instance, using educational deferments), their estimated bounds strongly suggest positive direct effects of military service on violent incarceration and recidivism. However, while the estimated bounds' confidence intervals exclude zero, confidence intervals that account for multiple testing do not. [6] We choose to interpret this evidence as suggestive of non-zero direct effects because the bounds employed to learn about the direct effect for draft avoiders dispose of the ER and do not add other assumptions. Under this interpretation, the estimates suggest that the draft lotteries may be an invalid IV for military service in the context of at least some incarceration and recidivism outcomes. Nevertheless, one can also interpret these results as simply not providing statistical evidence that the direct effects are non-zero, in which case the ER would not be rejected.

The second set of results concerns the effects of military service on the incarceration outcomes of volunteers—individuals who serve in the military regardless of their draft-eligibility. This group represents the majority of the Vietnam veterans and it may be informative about the current U.S. AVF. The estimated bounds on the effects for this group indicate that military service statistically increases the violent and nonviolent incarceration rates of white volunteers in certain birth cohorts (1951 and 1952). A simple analysis of the average characteristics of volunteers from different birth cohorts suggests that potential mediating factors of this effect are drug use, pre-service criminal justice contacts, low socioeconomic status, and disadvantaged family background. An implication is that an AVF system should be attentive to criminal justice contacts and drug use history during induction screenings.

The last set of results pertains to the incarceration and recidivism effects of military service for the population of veterans. Since veterans include compliers with the draft lotteries and volunteers, with the latter group representing 78-84% of veterans, it is not entirely surprising that

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[6] We apply multiple testing adjustment procedures (the Family-wise Error Rate or FWER, and the False Discovery Rate or FDR) across the five male birth cohorts under consideration (1948, 1949, 1950, 1951 and 1952) in the construction of the confidence intervals for the estimated bounds. Details of the procedures are in section IV.C.

the estimated bounds yield results similar to those found for volunteers. Military service statistically increases violent and nonviolent incarceration rates of white veterans from the 1951 and 1952 birth cohorts. The difference in results between volunteers and veterans is that the estimated bounds are somewhat wider and less precisely estimated for the group of veterans. The documented effects of military service on violent and nonviolent incarceration rates for volunteers and veterans appear substantial, as they represent at least between 47 and 160 percent of the corresponding incarceration rates of non-veterans.

This paper contributes to several strands of literature. First, it complements the literature on the consequences of the draft lotteries 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 draft lotteries on the subject cohorts soon after the draft, that is, when the individuals were in their early twenties. We show evidence that the draft lotteries may have increased the long-term incarceration for violent crimes for some males, 8 to 22 years after the draft, and that this effect may be separate from actual military service. Second, the paper contributes to the analysis of the crime and incarceration effects of the Vietnam era military service (for instance, Bouffard, 2014; Lindo and Stoecker, 2014; Teachman and Tedrow, 2014b) and 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 (for instance, Galiani et al. 2011, Albaek et al. 2017, Siminski et al. 2016, Hjalmarsson and Lindquist, 2019). Third, it contributes to the growing literature employing nonparametric bounds in IV models without the ER assumption (for instance, FF-L, 2013; Amin et al., 2016; CFF-L, 2019; Wang et al., 2020), by illustrating how this approach can be applied to situations where individual-level data is not available and auxiliary data is employed to undertake statistical inference under a bounded outcome. Lastly, we contribute to the growing literature on statistical methodologies to test implications of the assumptions underlying the conventional IV model with heterogeneous effects (Kitagawa, 2015; Huber and Mellace, 2015; FF-L, 2013; Mourifié and Wan, 2017) by empirically assessing the validity of the ER in the current empirical context, while adjusting for multiple testing.

II. The Vietnam Draft Lotteries

The Vietnam draft lotteries were a method adopted during the Vietnam war to fairly allocate military services in the U.S. They 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 draft lotteries (for instance, Angrist et al., 2010; Lindo and Stoecker, 2014) and focus on the 1948-1952 birth cohorts.[7]

Draft eligibility status based on the RSNs did not equate subsequent military induction. 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 military induction in the Vietnam era (Suttler, 1970; Shapiro and Striker, 1970; Baskir and Strauss, 1978).

An issue related to the randomization mechanism of the RSNs in the 1969 draft lotteries has been documented: men with birthdays in later months tended to receive lower lottery numbers and be drafted relative to men with birthdays in earlier months (Fienberg, 1971).[8] For this reason, the previous literature that employs the Vietnam draft lotteries as an IV for military service use birth month-by-year indicators to account for this issue (for instance, Angrist et al., 2010; Lindo and Stoecker, 2014). We will also account for this aspect within our bounding methodology.

III. Econometric Methods

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[7] 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 (for instance, Angrist et al., 2010). 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.

[8] 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).

We are interested in estimating the effects of military service on incarceration and recidivism outcomes. To avoid selection bias, we will employ the Vietnam era draft lotteries as an IV. However, we are particularly concerned with the validity of the ER due to factors that can result in direct effects of the IV on the outcomes through channels other than military service, rendering traditional IV estimators biased. We also have particular interest in learning about military service effects for individuals other than those who comply with their draft-eligibility status. For this reason, we adopt the nonparametric bounding techniques in FF-L (2013) and CFF-L (2019). These techniques allow the draft-eligibility IV to have a direct effect on the outcomes of interest, thereby disposing of the ER assumption. They also allow bounding the effect for the subpopulations of volunteers and compliers, thus allowing to place bounds on the effect for all veterans (that is, the average treatment effect on the treated). These bounding techniques rely on two main ideas. The first is to separate the total, reduced-form effect of eligibility to draft (the IV) on the outcomes of interest into an indirect effect that works through the channel of military service (that is, the military service induced by the draft), and a direct effect that does not work through the military service. This separation allows for violations of the ER assumption. The second idea is to define the direct and indirect effects at the level of the subpopulations of compliers, volunteers, and draft avoiders, and construct bounds on these "local" effects that can then be aggregated up to the level of other populations, such as the effect on all veterans. The exposition of the nonparametric bounds in the rest of this section focuses largely on the intuition behind those methods, while the interested reader is referred to FF-L (2013) and CFF-L (2019) for the details.

III.A Basic Setup

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 non-veterans) 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 follow Imbens and Angrist (1994) and Angrist et al. (1996) and partition the total population into four latent principal strata based on the values of the vector $\{D_{1i}, D_{0i}\}$. 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. One stratum is the draft avoiders or never-takers (nt): individuals who are non-veterans either when eligible or ineligible to draft ($D_{1i}=0$, $D_{0i} =0$). If draft avoiders receive a high lottery number that was not

called, they will not volunteer to serve; but if they receive a low lottery number that is probable to be called for induction, they likely undertake strategic actions to avoid the draft.[9] The second stratum is military service volunteers or always-takers (*at*): individuals who, regardless of whether they are eligible to draft, will serve in the military ($D_{1i}=1$, $D_{0i}=1$). This stratum is potentially relevant to the current AVF system since they do not need conscription to serve. The third stratum is the compliers (*c*): individuals who will serve in the military only if their lottery number is called to enlist ($D_{1i}=1$, $D_{0i}=0$). The literature using the draft lotteries as an IV estimates effects only for this stratum. The last group is the defiers (*d*): individuals who will enlist when their lottery numbers are not called for induction and will avoid enlistment if their lottery numbers are called ($D_{1i}=0$, $D_{0i}=1$). This stratum will be ruled out by assumption below.

Define the 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, which are standard in the literature (for instance, Imbens and Angrist, 1994). In contrast, the last two potential outcomes are counterfactual incarceration outcomes that are never observed in the data, but that are key to relax the ER assumption. The third potential outcome is 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 is 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. These last two potential outcomes will be employed to decompose the draft lotteries' total effect into an indirect (mechanism) and a direct (net) effect. 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.

The nonparametric bounding techniques we employ rely on the following assumptions that are also used in the conventional IV method (Imbens and Angrist, 1994; Angrist et al., 1996). The

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

first assumption, A1, is the random assignment of the instrument Z (the draft lotteries eligibility). The Vietnam draft lotteries satisfy 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 (for instance, 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 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 conventional IV method uses an additional assumption to point-identify the effect of military service on the outcome for the stratum of compliers ($LATE_c$):

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

This additional assumption, referred to as the exclusion restriction (ER), states that the draft lotteries do not affect incarceration outcomes in any way except through military service: $Y_i(0, d) = Y_i(1, d)$ for all i. This assumption can be interpreted as ruling out a non-zero direct effect of the eligibility to draft on incarceration outcomes. Given concerns that the ER may not be satisfied in this setting, our methodology will not employ this assumption.

III.B Three Parameters of Focus

The main insight to allow for possible violations of the ER is to decompose the total effect of the draft lotteries on a given incarceration outcome, $E[Y(1) - Y(0)]$, into two parts (see FF-L, 2010, 2013; CFF-L, 2019, and references therein). The first part is the direct (net) average treatment effect or $NATE^Z$. It is the effect of the draft lotteries on incarceration that is not related to (or is net of the effect that works through) the military service:[10]

(2) $NATE^z = E[Y(1, D_z) - Y(0, D_z)]$, for $z = 0, 1$.

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[10] 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 behaviors, as long as these channels are different from the actual military service.

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

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

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 $NATE^Z$. 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. Thus, we omit the superscript Z in what follows and focus on estimating parameters averaged over Z. Also, note that the ER shuts down the $NATE$ by assumption, that is, it rules out the relevance of any other channel that results in direct effects of the draft lotteries on the crime outcomes. Thus, an interpretation of $MATE$ is that it contains the "good" part of the effect of the lottery draft on the outcome that works through the military service, which can be used to (partially) identify the effect of military service within the IV framework (FF-L, 2013; CFF-L, 2019).

The effects in (2) and (3) can be defined for each one of the three principal strata, corresponding to "local" direct and indirect effects for draft avoiders, volunteers, and compliers. Note that, by definition of the strata, the indirect effect of the draft lotteries for draft avoiders and volunteers is zero since the draft lotteries do not change their potential military service status. Consequently, the direct effect of the draft lotteries for draft avoiders and volunteers coincides with their total effect. In contrast, for compliers, both their direct and indirect effects of the draft lotteries can in principle be non-zero. CFF-L (2019) provides nonparametric bounds for each of the direct and indirect local effects of the draft lotteries, local effects of the military service, as well as the corresponding effects for populations that are unions of principal strata (for instance, the group of veterans).

We focus on bounding three of those effects in this paper. First, since the direct effect of the draft lotteries is likely to work through the channel of draft avoidance behaviors, we focus on the direct effect of the draft lotteries on the draft avoiders' incarceration outcomes:

$$(4) \qquad LNATE_{nt} = E[Y(1, D_z) - Y(0, D_z)|nt] \quad \text{for } z = 0,1.$$

A nonzero direct effect of the draft lotteries on the draft avoiders is consistent with a violation of the ER. The second effect we focus on is the effect of the Vietnam war military service on incarceration outcomes for the volunteers, which can be expressed as:

(5) $LATE_{at} \equiv E[Y(z,1) - Y(z,0)|at]$, for $z = 0,1$.

The third effect of interest is the effect of military service on incarceration outcomes for the population of veterans, or the average treatment effect on the treated (ATT):

(6) $ATT \equiv LATE_{\{c,at\}} \equiv E[Y(z,1) - Y(z,0)|D = 1]$, for $z = 0,1$.

As previously mentioned, the population of veterans consists of both compliers and volunteers, and it is a population of first order importance. Even though prior literature (for instance, Lindo and Stoecker, 2014) estimates the military effects for compliers, we do not focus on this subpopulation because our bounds are wide for this group, likely due to the relatively small size of this stratum that does not allow more precise statistical inference.

III.C Nonparametric Bounds on the Three Parameters of Focus

In this subsection, we provide intuition for the partial identification results in FF-L (2013) and CFF-L (2019) that underlie the estimated nonparametric bounds on the three effects of focus.

Under assumptions A1-A3, the direct effect of the draft lotteries on the draft avoiders in (4) can be partially identified. Specifically, note that the first term in (4) is point identified as $E[Y(1)|nt] = E[Y|Z = 1, D = 0]$, which follows from Table 1 once the d stratum is eliminated. Additionally, letting $p_{d|z} \equiv \Pr(D_i = d|Z_i = z)$ for $d, z = \{0,1\}$, the population proportions of the three strata, denoted as π_{at}, π_c and π_{nt}, are point identified as $\pi_{at} = p_{1|0}$, $\pi_c = (p_{1|1} - p_{1|0})$, and $\pi_{nt} = p_{0|1}$. Then, while we cannot distinguish the never-takers from compliers when they are both ineligible-to-draft and did not serve in the military (the upper left cell in Table 1), bounds can be provided for the second term in (4) using "trimming bounds" (Lee, 2009; Zhang et al., 2008). To illustrate, note that the average outcome for the observed group with $\{Z = 0, D = 0\}$ can be written as a weighted average of outcomes of the nt and c strata (Imbens and Rubin, 1997):

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

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. With all the components in (4) either point or partially identified, bounds on this local effect are obtained.

It is important to note that the bounds on $LNATE_{nt}$ in (4) rely on the same assumptions as the conventional IV estimates, sans the ER assumption. Thus, maintaining A1-A3, if the estimated

bounds on $LNATE_{nt}$ statistically exclude zero, this provides statistical evidence of the existence of a direct effect of the draft lotteries on the incarceration outcomes for (at least some) draft avoiders. In turn, given that the ER assumption is imposed on every unit in the population, this implies the invalidity of the draft lotteries as an IV for military service in this context.[11]

In contrast to $LNATE_{nt}$, additional assumptions are needed to construct bounds on the other two effects, $LATE_{at}$ and ATT. The reason is that some objects in (*5*) and (*6*) are never observed in the data. These are the counterfactual outcome of volunteers had they not served in the military, $Y(z, 0)$, and the counterfactual outcomes $Y(1, D_0)$ and $Y(0, D_1)$ for compliers, the latter two needed to bound ATT. Bounds for the corresponding expectations of these counterfactual outcomes can be constructed under the following two assumptions that have been employed in prior literature (for instance, FF-L 2010, 2013; Huber et al., 2017; CFF-L, 2018, 2019). The first assumption is that the outcome is bounded, which provides natural bounds for the required expectations. The second assumption imposes weakly monotonic relationships of average potential outcomes across strata that share the same draft eligibility.[12] Formally,

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

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 $z=\{0, 1\}$.

A4 states that the potential incarceration outcomes have a bounded support, which is satisfied in our setting since the outcomes considered are binary. Thus, by providing bounds on the expectations in (*5*) and (*6*) of counterfactual outcomes that are never observed in the data, A4 permits the construction of bounds on $LATE_{at}$ and ATT. In contrast, A5 is substantive. It formalizes the notion that particular strata likely have characteristics that make them less likely to be imprisoned than other strata. Specifically, in our empirical setting, A5 states that, conditional

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[11] Related work that proposes statistical tests for implications of assumptions A1 to A4 are Huber and Mellace (2015), Kitagawa (2015), and Mourifié and Wan (2017).

[12] The same prior literature has also considered an assumption that restricts the local average effects to be either non-positive or non-negative. We do not consider this assumption here since the previous literature on the effects of the draft lotteries and the military service on future incarcerations is inconclusive about the effect's sign (for instance, Kuziemko, 2010; Lindo and Stoecker, 2014; Albaek et al., 2017).

on the same eligibility to draft and potential veteran status, the compliers are (weakly) less likely to be incarcerated than the volunteers, who in turn are (weakly) less likely to be incarcerated than the draft avoiders. A5 improves on the natural bounds provided by A4 since now point- or partially-identified mean potential outcomes for certain strata serve as tighter bounds for the expectations in (5) and (6) of counterfactual outcomes that are never observed in the data. Given that A5 is a crucial assumption, we extensively discuss and assess it in section V.B.1. Online Appendix A provides expressions for the nonparametric bounds we employ, their derivation, and proofs, while we present details on their estimation and statistical inference in section IV.C below.

IV. Data and Empirical Strategy

IV.A Data Sources and Key Variables

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 current offenses in his record involve violent offenses, and we classify inmates as incarcerated for a nonviolent crime otherwise.[13] This classification is different from the measure in Lindo and Stoecker (2014) that focuses on original offenses in the inmate's record. The conclusions from the analysis conducted below are very similar with any of the two measures, and also using a measure that

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[13] 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.

classifies inmates as incarcerated for a violent crime if any of his records show a violent offense (see Online Appendix G). Besides incarceration outcomes related to current offenses, we construct a measure of recidivism in order to analyze the military service effect on this important aspect. For each of the violent and nonviolent recidivism outcomes, an indicator variable is set to one if the inmate is currently incarcerated for violent or nonviolent crimes, respectively, and also had juvenile criminal justice contacts before they became eligible for military induction, 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.[14] The use of age 18 is because age 18½ was the lowest induction age during the Vietnam war. The use of the year 1968 for prior incarcerations is because the exact age of prior incarceration is not available in SISFCF, but the year of admission to an incarceration facility is; 1968 is chosen since the first draft lottery took place in December 1969. 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, which consists of 2700 white and 2619 nonwhite inmates. The table shows summary statistics on veteran status, draft-eligibility, crime outcomes, and estimated strata proportions (under A1 and A3), for white and nonwhite inmates. We see that a higher proportion of white inmates served in the Vietnam era war, and while nonwhite inmates have a higher estimated proportion of draft avoiders than whites, white inmates have a higher estimated proportion of volunteers. Note that the estimated proportion of compliers is small for white and nonwhite inmates, and it is not significantly different from zero

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[14] 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 [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)?".

for white inmates. These estimated proportions of compliers using the inmate sample are smaller than those for the corresponding U.S. population, which are between 7-14% (reported in Online Appendix C). The estimated proportions of compliers from the inmate sample are not employed by our methodology. Regarding the criminal offending status, white inmates are less likely to be violent crime offenders and to have been incarcerated before 1968 regardless of their current criminal offending status, relative to nonwhite inmates. Lastly, white and nonwhite inmates show similar proportions on arrests and probation before 18 years of age.

To construct the outcomes, 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 from the VSUS by race to calculate incarceration rates for males born between 1948 and 1952.[15] The outcomes are constructed from mean incarceration or recidivism rates for a certain crime type in survey year s, of males born in birth year b and birth month m, with eligibility to draft z, and veteran status d:

$$(8) \qquad Incarceration\ outcome_{sbm}(z, d) = \frac{\#of\ inmates_{sbm}(z,d)}{\#of\ Births_{(z, d|\ b, m)}}.$$

The numerator of the constructed incarceration rate outcome in (8) is the inmate counts by characteristics s, b, m, z, and d 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.[16] For reference, Table 3 summarizes the estimated counts of male inmates born in 1948-1952, broken down by draft-eligibility and veteran status. The denominator in (8) is the male population in the U.S. defined by characteristics b, m, z, and d. To construct it, we employ the VSUS.

We note that the recidivism outcomes constructed following Equation (8) are not comparable to the conventional measure of recidivism that expresses the number of recidivists

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[15] 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).

[16] 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).

divided by the count of previously incarcerated individuals. To create a comparable measure, 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 draft lotteries, which is not available to us. Instead, the measure of recidivism following Equation (8) is constructed by dividing the inmate count of recidivists by the U.S. male population count (for each subgroup defined by characteristics *b*, *m*, *z*, and *d*). 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 offenses 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.[17] These estimates largely suggest no statistically significant "intention-to-treat" effects of the draft-eligibility on the outcomes of whites or nonwhites.

To estimate our bounds, we need estimates of the population strata proportions (under A1-A3) from a dataset that is representative of the U.S. population. Wang et al. (2020) used a special version of the 1982-1996 National Health Interview Survey (NHIS) to estimate the k strata proportions in the U.S. population, which we borrow here. Specifically, they constructed draft-eligibility and Vietnam era military service variables, which allows estimating the strata proportions under assumptions A1-A3. Online Appendix C presents these estimated U.S. male population strata proportions.

IV.B Estimation Strategy

The bounds described in section III.C can, in principle, be estimated with analog estimators. For example, the mean potential outcomes of the eligible-to-draft draft avoiders ($E[Y(1)|nt]$) and of the ineligible-to-draft draft volunteers ($E[Y(0)|at]$) can be estimated as $\sum_{i=1}^{N}\{Y_i \cdot I(Z_i = 1, D_i = 0)/I(Z_i = 1, D_i = 0)\}$ and $\sum_{i=1}^{N}\{Y_i \cdot I(Z_i = 0, D_i = 1)/I(Z_i = 0, D_i = 1)\}$, respectively. The first four columns of Table 5 present these estimates and the mean incarceration rates of the observed groups $E[Y|Z = 0, D = 0]$ and $E[Y|Z = 1, D = 1]$, which consist of a mixture of strata. From section III.C., the population strata proportions can be

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[17] 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 can be found in Online Appendix F.

estimated using the NHIS data as $\pi_{at} = p_{1|0}$, $\pi_c = (p_{1|1} - p_{1|0})$, and $\pi_{nt} = p_{0|1}$. For the trimmed means related to the trimming bounds motivated in section III.C. (Equation 7), they could be easily estimated with access to individual-level outcome data. All of these analog estimates would then be plugged into the expressions for the nonparametric bounds presented in Online Appendix A.

As detailed in the previous subsection, however, we lack access to individual-level outcome data. Instead, we innovate by estimating the trimming bounds exploiting the binary nature of the outcomes.[18] The informativeness of the estimated bounds is aided by the relative magnitudes of the incarceration rates and the trimming proportions. To illustrate, consider computing trimming bounds on $E[Y(0)|nt]$ for the violent crime incarceration outcome of white draft avoiders. From (7), the trimming bounds for this object use the observed group of ineligible-to-draft non-veterans ($\{Z = 0, D = 0\}$) and the trimming proportion $\pi_{nt}/(\pi_c + \pi_{nt})$. The average incarceration outcome for the observed group, $E[Y|Z = 0, D = 0] = 0.0018$, is smaller than the trimming proportion $\pi_{nt}/(\pi_c + \pi_{nt}) = 0.82$. Therefore, as shown in Figure 2, the $(1 - 0.82)$-th percentile of Y in that observed group ($y^{00}_{1-0.82}$), which corresponds to the left end of the bracket in the top panel, and the 0.82-th quantile of Y in the same observed group ($y^{00}_{0.82}$), which corresponds to the right end of the bracket in the bottom panel, must both equal zero. Hence, the upper bound of $E[Y(0)|nt]$ is computed by dividing the estimated number of inmates in the group with $\{Z = 0, D = 0\}$ by the estimated total male population that belongs to the draft avoiders stratum with $\{Z = 0, D = 0\}$; and the lower bound of $E[Y(0)|nt]$ is zero following the argument above. Lastly, the upper and lower bounds on the direct effect of the draft lotteries on draft avoiders, $LNATE_{nt}$, are estimated by subtracting the lower and upper bounds of $E[Y(0)|nt]$, respectively, from the point estimated $E[Y(1)|nt]$.

To illustrate the construction of the bounds, Table 5 presents the intermediate steps in the estimation of the upper and lower bounds on $LNATE_{nt}$ for the 1948-1952 born draft avoiders: the point estimates of $E[Y(1)|nt]$ and the upper and lower bound estimates on $E[Y(0)|nt]$. Similar steps are employed in the estimation of the bounds on the other parameters of focus, although sometimes the expressions of the bounds involve taking the max or min over objects that are

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[18] We thank Carlos A. Flores for pointing out to us that the bounds in FF-L (2010, 2013) and CFF-L (2019) can still be computed without individual-level data when the outcome is binary.

candidates to be bounds (see Online Appendix A). Note also that the width of the nonparametric bounds partially depends on the relative strata proportions. For example, for the upper bound of $E[Y(0)|c]$, the $\pi_c/(\pi_c + \pi_{nt})$ largest values of Y are used to construct the trimmed means. Since $\pi_c/(\pi_c + \pi_{nt}) < \pi_{nt}/(\pi_c + \pi_{nt})$ in all birth cohorts, the estimated upper bounds on $E[Y(0)|c]$ (obtained using trimming) will be wider than the similarly estimated upper bounds on $E[Y(0)|nt]$.

Lastly, to account in our estimation procedure for the issue related to the randomization mechanism of the RSNs in the 1969 draft (Fienberg, 1971) described in section II, we first conduct estimation of all relevant objects and effects for each month of birth and then construct a weighted average of those estimates across all the birth months, weighted by the male population born in each month. This procedure, which is applied to all draft years, effectively controls for month of birth in the estimation of the bounds.

IV.C Statistical Inference

For the nonparametric bounds that do not involve max and min operators (see Online Appendix A), statistical inference is based on confidence regions for the true parameter of interest following Imbens and Manski (2004). Other nonparametric bounds we use involve those operators, which break down standard statistical inference (Hirano and Porter, 2012). To conduct valid inference on those bounds, we rely on the methodology proposed by Chernozhukov, Lee, and Rosen (2013), described in Online Appendix B. 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.

Furthermore, since we analyze the direct effects of the draft lotteries and military service effects in several subsamples defined by birth cohorts, we are mindful of the potential problem of performing multiple testing of null hypotheses. It is well known that the situation of multiple testing increases the risk of falsely rejecting a true null hypothesis of a zero effect. To control for this, we employ three different sequential multiple testing procedures. The first is the sequential Family-wise Error Rate (FWER) testing procedure (Holm, 1979). The second and third procedures are the sequential False Discovery Rate (FDR) by Benjamini and Hochberg (1995) and the sharp

sequential FDR in Benjamini, Krieger, and Yekutieli (2006). To implement the multiple testing procedures to our estimated bounds, we follow Mourifie and Wan (2017).[19]

V. Results

In this section, we present the results employing the estimated nonparametric bounds on the three parameters of focus. We also discuss implications of the results, conduct additional analyses to increase our understanding of the results obtained, and provide estimates of the social costs implied by our estimates.

V.A Direct Effects of the Draft Lotteries on the Outcomes of Draft Avoiders

We begin by analyzing the direct effects of the draft lotteries on the incarceration and recidivism outcomes of draft avoiders. While we start by analyzing all the relevant cohorts (1948-1952) combined, we subsequently explore the heterogeneity of the direct effects by cohorts whose behavior may had been affected differently by the draft. Throughout the results in this subsection, only the conventional IV assumptions are employed sans the ER assumption (A1 to A3).

Figure 3 presents estimated bounds for the direct effect of the draft lotteries on the draft avoiders (nt) stratum. The shaded bar and the capped intervals represent the estimated bounds and confidence intervals (90 and 95 percent), respectively; the dark dots show, for reference, the mean incarceration rates of ineligible-to-draft non-veterans. The top two panels in Figure 3 present estimated bounds and confidence intervals on the direct effect of the draft lotteries for the 1948-1952 born white (Panel A) and nonwhite (Panel B) draft avoiders. We present results by race since prior literature has found that the effects of conscription and of military service vary over this dimension (for instance, Kuziemko, 2010; Lindo and Stoecker, 2014). The estimated lower bounds for whites suggest that the direct effect of the draft lotteries on their violent incarceration and recidivism (first and third bars) outcomes 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 non-veterans). For nonwhites, the estimated lower bounds are consistent with a direct 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. (13.6%; the fourth bar). However, the previously discussed bounds are not precisely

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[19] In the case in which the bounds involve max and min 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 hypotheses tested for each effect across the birth cohorts under analysis.

estimated, as their 90% confidence intervals do not exclude a zero direct effect. The estimated lower bounds on the other outcomes presented in Panels A and B of Figure 3 are negative, thus not excluding a zero direct effect. In sum, for the combined 1948-1952 birth cohorts, we do not find strong evidence of non-zero direct effects of the draft lotteries on the incarceration outcomes.

We next explore the heterogeneity in the direct effect of the draft lotteries over the cohorts exposed to the draft. In principle, there could be differences in the avoidance behaviors over cohorts, caused by the different timing and circumstances of the draft lotteries conducted in 1969, 1970, and 1971. Males born in 1948 to 1950 were subjected to the first national lottery draft conducted in December 1969, where drafted men were called for physical examinations and inductions starting in January 1970. This timeline stands in stark contrast with the 1951- and 1952-born males who were subjected to the draft lotteries of July 1970 and August 1971, respectively, and who had more time to strategically react to their lottery numbers since the call for inductions begun at the start of the following year (and they also had the benefit of witnessing the earlier draft). Moreover, in the 1970 and 1971 drafts, the lottery numbers called for induction were (ex-post) 36% and 51% lower relative to those in the 1969 draft. These circumstances suggest that men exposed to the 1969 draft may have had to resort to immediate avoidance behaviors (for instance, delinquency or draft evasion) instead of other avoidance behaviors (such as educational deferments) given the limited reaction time they had. There is another aspect that distinguishes one of the three cohorts subjected to the 1969 draft: while men born in 1948 and 1949 could have been subjected to earlier years' local drafts—and therefore already "picked over" and/or prepared to behave strategically to dodge conscription—men born in 1950 were subjected for the first time to conscription through the national 1969 draft. In sum, we expect that cohorts who had a shorter time to react to the draft lotteries and were not previously subjected to local drafts might had been more likely to engage in immediate avoidance behaviors that could have impacted negatively their incarceration outcomes.

The results on the direct effect of the draft lotteries on draft avoiders by cohort suggestively reflect the previous arguments regarding the drafts they were subjected to: the cohorts subjected to the 1969 draft tend to have estimated bounds that are more consistent with positive direct effects of the draft lotteries on their incarceration outcomes. The results for all birth cohorts are available in Online Appendix D, while Panels C and D in Figure 3 present the estimated bounds and confidence intervals for the direct effect of the draft lotteries on the 1950 cohort that likely had the

least reaction time. The estimated bounds for this cohort are estimated more precisely relative to other cohorts, although the estimated bounds across cohorts do exhibit a large degree of overlap.

Panel C of Figure 3 presents estimated bounds and confidence intervals for the direct effect of the draft lotteries for the 1950-born white draft avoiders. Their estimated lower bounds suggest that the direct effect of the draft lotteries is to increase the incarceration rates for violent crimes by at least 0.07 p.p. (38.4%; the first bar), violent recidivism by at least 0.03 p.p. (40%; the third bar), and nonviolent recidivism by at least 0.02 p.p. (21.9%; the fourth bar). Nevertheless, the 90 percent confidence intervals for these effects marginally include zero. For nonviolent crime incarceration, the estimated bounds include a zero direct effect. Panel D of Figure 3 presents estimated bounds and confidence intervals for the direct effect of the draft lotteries for the 1950-born nonwhite draft avoiders. Their estimated lower bounds imply a direct effect of the draft lotteries on violent crime incarceration of at least a 0.46 p.p. (40.8%; the first bar) increase, and on violent crime recidivism an increase of at least 0.38 p.p. (69.0%; the third bar). For these two effects, the 95 percent confidence intervals exclude zero. The estimated bounds also suggest that the draft lotteries directly increase nonviolent crime incarceration by at least 0.33 p.p. (38.5%; the second bar) and nonviolent crime recidivism by at least 0.04 p.p. (10.3%), but the corresponding 90 percent confidence intervals do not exclude a zero direct effect. A valid concern in our exploration of heterogeneity in the direct effects of the draft lotteries on draft avoiders is that we have conducted tests of hypotheses in several subsamples, and thus rejection of the null of no direct effects could occur by chance. For this reason, we implement the three conservative multiple testing procedures described in Section IV.C. that allow statistically controlling for a valid significance level when simultaneously testing the null hypothesis of a zero direct effect over all birth cohorts. Applying these conservative inference procedures, we do not reject the null hypotheses that the direct effect of the draft lotteries is zero for any of the outcomes in Panel D.[20]

Overall, our interpretation of the results is that there is some evidence suggesting that the direct effect of the draft lotteries (unrelated to military service) increases future violent crime

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[20] The estimated bounds on the direct effect of the draft lotteries on the outcomes are more precisely estimated in the pooled sample of white and nonwhite 1950-born draft avoiders. In this case, draft-eligibility directly 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 statistical significance of these results withstand the multiple testing conservative adjustments.

incarceration and recidivism for draft avoiders, particularly for those in the 1950-born cohort. Despite the confidence intervals including a zero-effect, we choose this interpretation because the bounds employed to learn about the direct effect dispose of the ER while not adding other assumptions. Given these mild assumptions, it is remarkable that most of the bounds exclude zero effects, although the corresponding confidence intervals include zero effects at conventional statistical levels, especially once multiple testing procedures are employed; that is, the bounds seem to be imprecisely estimated. Of course, one can also interpret these results as simply not providing statistical evidence that the direct effects are non-zero. Under the former interpretation, the estimates suggest that the draft lotteries may be an invalid IV for military service in the context of at least some incarceration and recidivism outcomes, since the ER assumption must be satisfied by every unit in the sample. As a consequence, point estimates of the effect of military service on violent incarceration and recidivism outcomes based on conventional IV methods using the draft lotteries IV may have to be considered with caution. Lastly, note that the estimated direct effects of the draft lotteries presented in this section are for the draft avoiders, and thus cannot be generalized to the groups of draft volunteers and compliers.[21]

What channels might explain the presence of a direct effect on the violent crime incarceration and recidivism rates of draft avoiders? One plausible channel 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. Another possible channel is simply draft evasion, which in several instances resulted in prosecution and conviction (see, for instance, Peterson, 1998).[22] In both cases, early delinquency and incarceration can increase later years' incarceration and recidivism. The notion of increased adult criminal behavior after contacts with the judicial system as a youth has been documented in Bayer et al. (2009) and Aizer and

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[21] The groups of volunteers and compliers may experience direct effects on their incarceration outcomes. However, we do not estimate bounds for their effects since we are not aware of theoretical or anecdotal support for them to experience these effects. Still, the methods we employ next do not assume that those effects are zero.
[22] Another possibility, suggested to us by an anonymous referee, is labor market discrimination of potential employers against draft avoiders, since the RSNs were published and potential employers could have identified draft avoiders by their date of birth. This would make formal work less rewarding for draft avoiders relative to illegal work.

Doyle (2015). Both papers document that earlier year incarcerations significantly increase recidivism both for violent and nonviolent crime types.[23]

V.B The Effects of Military Service on the Outcomes of Volunteers and Veterans

We now turn to statistical inference on the effects of military service on incarceration and recidivism outcomes for volunteers and the population of veterans. We start with a detailed assessment of the key assumption we employ to construct bounds on this effect while disposing of the ER assumption. Subsequently we present results for volunteers, a group of singular importance, followed by secondary analyses aimed to understand some of the potential channels behind their effects. Lastly, we present results for the population of veterans.

V.B.1 Discussion and Assessment of Assumption A5

The key assumption we employ to derive bounds on the effects of military service on veterans is A5. This assumption implies that, conditional on the same eligibility to draft status and potential veteran status, compliers are (weakly) less likely to be incarcerated than the volunteers, who in turn are (weakly) less likely to be incarcerated than the draft avoiders. We offer three indirect assessments. One is based on the idea that estimated pre-draft incarceration outcomes by strata can inform the proposed ranking in A5. A second argument is based on the relative high school completion rate of the three strata, combined with the documented relationship between schooling and crime in the literature. The last argument is based on a testable implication of the bounds to be employed, which can be used to "falsify" the set of assumptions A1 to A3 plus A5.

The average pre-draft outcomes for each stratum can be estimated under assumptions A1 to A3 using individual-level data (FF-L, 2010, 2013; CFF-L, 2018, 2019).[24] However, the

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[23] An alternative channel through which the draft lotteries may have a direct effect on incarceration and recidivism outcomes is the "dodging-up" avoidance behavior, such as obtaining admissions into college to avoid the draft (for instance, Card and Lemieux, 2001). This type of avoidance behavior, resulting in higher educational attainment, is predicted to reduce incidence of criminal activities given the negative 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. Indeed, the counteracting effects of these avoidance behaviors may be a reason why the estimated bounds do not exclude zero more often and are relatively imprecisely estimated.

[24] Intuitively, for the *nt* stratum, the average pre-draft outcomes correspond to the mean pre-draft outcomes of eligible-to-draft non-veterans, 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 non-veterans, and both the strata proportions and the average pre-draft outcomes of *at* and *nt* are identified.

individual-level data available to us is only for inmates. Thus, we undertake two different suggestive exercises, each of which is imperfect since the resulting estimated average pre-draft outcomes are likely biased. The first is to employ data exclusively on inmates from the SISFCF. A problem is that the resulting estimates are likely not representative of the U.S. male population due to self-selection into incarceration. In the second exercise, we compute average pre-draft outcomes by strata by counting the number of inmates belonging to a specific stratum and who experience the pre-draft outcome, and dividing this number by the estimated U.S. male population who belong to that same stratum, using data from the VSUS and the estimated population strata proportions from Wang et al. (2020). In this case, bias in the estimates may arise because the average pre-draft outcomes in the non-incarcerated population may differ over strata. Even though the biases in the two methods are generally different from each other, they both lend indirect support to the weak ranking of the strata in A5.[25]

The estimated average pre-draft outcomes using the two methods explained above are presented in Panel A and Panel B of Table 6, respectively.[26] We focus on three pre-draft outcomes: arrests, probation, and incarceration before being subjected to the draft (turning 18 years old or before the year 1968). The estimated pre-draft averages under both methods indicate that draft avoiders were more likely to have been arrested, on probation, and incarcerated before they were subjected to the draft, relative to compliers and volunteers combined (Columns 5 and 6). Since 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 (see, for instance, Bayer et al., 2009; Aizer and Doyle, 2015), these estimates offer indirect support to the weak ranking of draft avoiders relative to the other two strata in A5. Turning to the weak monotonicity relationship in A5 between volunteers and compliers, Table 6 suggests that, on the population-scaled level (Panel B), volunteers have higher average rates of probation before the draft relative to a group that combines volunteers and compliers (bold figure in Column 7). Although on the inmate level (Panel A) the

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[25] Online Appendix E presents the formal mathematical expressions of the biases in these two ways of computing average pre-draft outcomes by strata using the inmates sample.

[26] Note that, in contrast to other papers using nonparametric bounds (FF-L, 2010, 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, as previously mentioned, the proportion of compliers in the inmate sample is quite low. These small proportions do not allow the estimation of the complier's average pre-draft outcomes with precision.

estimates suggest that volunteers have lower criminal justice contacts before the draft than the observational group of volunteers and compliers combined (Column 7), none of these differences between the two groups are statistically significant. Thus, overall, we find indirect evidence supporting the weak ranking of strata postulated in A5 and we do not find statistically significant evidence contradicting such ranking.

The second source of indirect support for A5 is based on the relative high school completion rate of the three strata which, in light of the literature on the relationship between schooling and crime outcomes, supports the weak ranking of strata in A5. More specifically, using the restricted-use representative data from the NHIS, Wang et al. (2020) report that, for whites, compliers have higher high school completion rate (0.93) relative to volunteers (0.88), whom in turn have higher high school completion rate relative to draft avoiders (0.85), with their differences being statistically significant. For nonwhites, compliers have higher high school completion rate (0.99) relative to volunteers (0.87), whom in turn have higher high school completion rate relative to draft avoiders (0.73), with their differences being statistically significant with the exception of the difference between compliers and volunteers. Given the negative relationship between education and incarcerations (see, for instance, Lochner and Moretti, 2004), this evidence indirectly supports the weak ranking of the potential incarceration outcomes in A5.

The final evidence we present in support of A5 relies on one testable implication that follows from the derivation of the bounds using assumptions A1 to A3 plus A5, which can be used to "falsify" those assumptions (CFF-L, 2019).[27] Recall that A5 indicates that the potential incarceration rate outcome of draft avoiders should not be lower than those for the compliers and the draft volunteers. 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 draft avoiders, $E[Y(1)|nt]$, must not be smaller than the conditional mean $E[Y|Z = 1, D = 1]$, which is the mean of the draft-eligible volunteers and compliers, $E[Y(1)|at, c]$. Table 7 presents 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. Thus, the testable implication is not statistically rejected for any of the outcomes or

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[27] 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.

analysis groups. The same conclusion is reached when these groups are broken down by birth cohorts (not shown).

V.B.2 The Effects of Military Service on the Outcomes of Volunteers

We now move on to the subpopulation of Vietnam era volunteers—an important group since it can be informative about the current AVF in the U.S. The results we present for them are novel in the context of IV methods since the subpopulation of focus there is the compliers. We have discovered heterogeneous effects of military service on incarceration and recidivism outcomes for volunteers born in different years. In Figures 4 and 5, the dark dots represent the mean incarceration rates of non-veterans, reported for reference. Results for white and nonwhite volunteers born in 1948-1952 and 1950 are presented in Figure 4. All the corresponding estimated bounds include zero, with the exception of the bounds on the nonviolent crime incarceration and recidivism for the 1950-born nonwhites (Panel D, Figure 4). However, the 90 percent confidence intervals on those bounds do not rule out a zero military effect. Thus, the results for the 1948-1952 and 1950 born whites and nonwhites in Figure 4 suggest that the military service crime effect may not be different from zero.

In contrast, turning to the 1951 and 1952 birth cohorts in Figure 5, most of the estimated bounds on the effect for volunteers—and some of their estimated confidence intervals—exclude zero. Specifically, for white volunteers 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. (Panels A and B of Figure 5), respectively. These are potentially large effects as they represent at least 144 and 160 percent of the mean outcome of non-veterans, respectively. For the outcome of nonviolent crime incarceration for the white volunteers born in 1951 and 1952, the estimated bounds indicate that military service increases the incarceration rates by at least 0.08 p.p. (49%) and 0.15 p.p. (86%), respectively. Furthermore, the 90% confidence intervals on the four estimated bounds just mentioned exclude zero. As for the recidivism outcomes, the estimated bounds for white volunteers born in 1951 and 1952 indicate that the effect of military service is to increase violent recidivism by at least 0.03 p.p. (44%) and 0.08 p.p. (80%) respectively, and nonviolent recidivism by at least 0.04 p.p. (56%) and 0.02 p.p. (23%), 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 violent recidivism for the 1952-born white volunteers.

The results for nonwhite volunteers born in 1951 and 1952 are presented in Panels C and D of Figure 5, respectively. The bounds for the 1951-born volunteers indicate an increase in violent incarceration rates of at least 0.32 p.p. (23%), and an increase in nonviolent recidivism rates of at least 0.15 p.p. (31%). The estimated bounds' 90% confidence intervals, however, do not exclude zero. For the 1952 birth cohort, military service increases violent crime incarceration of nonwhite volunteers by at least 0.72 p.p. (44%) and their corresponding 95 percent confidence intervals exclude zero. Military service also increases their nonviolent incarceration rate by 0.4 p.p. (34%), although the confidence intervals do not rule out zero. The rest of the estimated bounds for nonwhite volunteers do not exclude a zero effect. Thus, the evidence of military service effects on the incarceration and recidivism outcomes of nonwhite volunteers is more tenuous than for whites.

We perform statistical inference robust to multiple testing for the estimated military service effect for volunteers for the same reason we used those methods for the estimated direct effect for the draft avoiders in section V.A. After employing the three multiple testing methods across the five birth cohorts, we reject the null hypothesis of zero military service effects for white volunteers on their violent crime incarceration for the 1951- and 1952-born and for the nonviolent crime incarceration of the 1952-born. In contrast, we do not reject (at conventional significance levels) the null hypothesis of a zero military service effect on the 1952-born nonwhite volunteers' crime outcomes or the 1951-born white volunteers' nonviolent crime outcomes.

V.B.3 Additional Analysis on the Effect of Military Service on Volunteers

The previous results indicate that the estimated bounds for the 1948-1952 and the 1950 cohorts do not generally exclude zero while those for the 1951 and 1952 cohorts on the violent and nonviolent incarceration rates are predominantly positive and often exclude zero. In an effort to understand the factors that may lie behind the difference in these results, we use the inmate's data to compare several of the cohort's average characteristics in Table 8. 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 V.B.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 volunteers in the sample under A1-A3. The choice of characteristics to be compared is guided by the literature documenting channels through which military service affects crime outcomes: combat exposure (Rohlfs, 2010), drug use (Robins, 1973),

pre-service arrests and offending (Albæk et al., 2017), childhood physical abuse victimization and maltreatment (Khawand, 2009), and family background (Hjalmarsson and Lindquist, 2019).

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).[28] For each of these three violence exposure measures, the volunteers in the 1948-1950 cohort have higher averages relative to the volunteers 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 (for instance, Killgore et al. 2008; Rholfs, 2010; Sreenivasan et al. 2013). Nevertheless, this evidence is consistent with 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 (such as affirmation to patriotic beliefs, self-improvement, and solidarity with others) that are positively associated with a myriad of traumatic exposures (such as 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 (for instance, 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[29] effect of wartime combats 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 indeed possible that combat exposure can be related to lower incarceration rates for volunteers.

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[28] 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?".

[29] Posttraumatic growth is defined as positive psychological changes in response to trauma (Tedeschi and Calhoun, 1996).

The second set of characteristics compared in Table 8 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.[30] Table 8 shows that inmate volunteers 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 (for instance, Robins et al., 1975; Stanton, 1976), and the documented positive relationship between drug use and criminal offenses (for instance, Ellinswood, 1971; Tinklenberg, 1973), this evidence is consistent with the documented differential effects of military service on the incarceration rates of different cohorts of volunteers. 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 volunteers who had been using drugs before their military service. Furthermore, recent 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 8 is the involvement with the criminal justice system as a juvenile. The estimates indicate that inmate volunteers born in 1951-1952 were more likely to have had criminal justice contacts (arrests, probation, and incarceration) as juveniles relative to the earlier cohorts of 1948-1950, a difference only statistically significant for whites. One may conjecture that a criminal history prior to military service contributes to a larger crime instigation effect of the Vietnam era military service, which is consistent with the differential

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[30] 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?"

effects found for volunteers in different cohorts. This is also consistent with similar evidence reported in Hjalmarsson and Lindquist (2019) in the context of the mandatory military service in Sweden.

The last set of characteristics analyzed in Table 8 are three indicators of family background and socioeconomic status. The estimates suggest that the white inmate volunteers in the 1951-1952 cohorts 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 cohorts. For nonwhite inmates, volunteers 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 cohorts.[31] Therefore, inmate volunteers in the 1951-1952 birth cohorts, on average, tend to have worse family background and lower socioeconomic status relative to the 1948-1950 birth cohorts. This may be another reason why military service significantly increased the former cohorts' incarceration rates, given the literature documenting a strong negative correlation between socioeconomic status and incarcerations (for instance, Kearney et al., 2014) and the evidence in Hjalmarsson and Lindquist (2019) that military service has the most potent crime instigation effect among men with low socioeconomic status.

To summarize, the disadvantaged characteristics of volunteers from the 1951-1952 cohorts—with the possible exception of combat exposure, as previously discussed—appear consistent with the finding that the effect of military service on incarceration and recidivism outcomes is stronger for them 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.

V.B.4 The Effects of Military Service on the Outcomes of Veterans

We now present results on the crime instigation effects of military service on the population of Vietnam era veterans, that is, the treated population by military service—a population of first-order importance for policy. The veteran population consists of volunteers and compliers. To

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[31] 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)?".

provide context, based on the NHIS estimates of the proportions of volunteers and compliers, the volunteers account for about 78-84% of the population of veterans. As it was the case with volunteers, the results for the population of veterans are novel in the context of IV methods.

The results for white and nonwhite veterans are presented in Figure 6. Perhaps not surprisingly, since volunteers account for the majority of the Vietnam era veterans, the results mirror closely those already documented for the group of volunteers in section V.B.2. Namely, while for white and nonwhite veterans born in 1948-1952 and 1950 (Panel A to Panel D) the estimated bounds include zero (except in one instance), the majority of the estimated bounds for the 1951 and 1952 birth cohorts (Panel E to Panel H) exclude zero. Although the estimated bounds on the effects of the volunteers and veterans tend to be similar, the estimated bounds for the veterans tend to be wider and less precisely estimated. This stems from the fact that compliers consistently represent a smaller proportion of veterans relative to volunteers, and that the corresponding estimated bounds on the effects for compliers tend to be wide and centered around zero.[32] The estimated bounds for the 1951- and 1952-born veterans that statistically exclude zero and withstand multiple testing adjustments are for the military service effect on the violent incarceration of whites. First, the estimated lower bound on the military service effect on violent crime for white veterans born in 1951 indicates that it is at least 0.17 p.p. (Panel E), which represents 124% of the mean outcome of non-veterans. Second, the same effect for the 1952-born white veterans (Panel G) is estimated to be at least 0.25 p.p., a 128% increase relative to the mean outcome of non-veterans.

V.C Monetary Social Costs of Crime from the Draft Lotteries and Military Service

We provide simple estimates of the crime and incarceration consequences of the Vietnam military service in terms of societal monetary costs based on the previous results. To do this, we estimate 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 $403,915 (converted into 2019 U.S. dollars). For nonviolent crimes, we combine the estimates in McCollister et al. (2010) and the costs for drug violations in Delisi and

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[32] The estimated bounds for compliers are available in Online Appendix D. As previously mentioned, we do not discuss those results herein given that the estimated bounds are wide and imprecisely estimated, likely due to the relatively small size of this stratum.

Gatling (2003) to estimate an average per unit cost of $153,347 (in 2019 U.S. dollars). These costs include victim costs (such as medical expenses, cash losses, property theft or damage), criminal justice system costs, and crime career costs (that is, 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.[33] 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 and withstand multiple testing adjustments. Thus, the figures represent a conservatively estimated lower bound of societal monetary costs.

Our results show that Vietnam era military service significantly increased violent crime incarceration for white veterans born in 1951 and 1952, and significantly increased nonviolent crime incarceration for white volunteers. 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 1,274 violent offenses and 331 nonviolent offenses. The induced total tangible cost is at least about $565 million 10-20 years after the Vietnam era military service took place.[34] According to the Congressional Research Service (2010), the cost of the Vietnam war amounted to $111 billion ($843 billion in 2019 U.S. dollars). Our results suggest that this figure may be an underestimate given that the crime instigation effects of military service are not accounted for. Also, based on the current amount of resources that policymakers devote for corrective purposes, addressing early the crime instigation consequences of military service suggests cost-savings

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[33] Other studies (for instance, 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.

[34] To estimate the total number of offenses induced by military service, the total population of 1951- and 1952-born white veterans is, respectively, 326,933 and 288,935, while the total population of 1951-born white volunteers is 215,335. We then multiply the lower bound estimates of military service effect of the respective birth cohort by the corresponding population of volunteers or veterans, obtaining the increased number of offenses by race and birth year. Specifically, for 1951-born white veterans, their estimated ATT lower bound of military service on violent offenses is 0.0017203 times 326,933 = 562. For 1952-born white veterans their estimated ATT lower bound of military service on violent offenses is 0.0024635 times 288,935 = 712. For 1951-born white volunteers, the corresponding estimated $LATE_{at}$ lower bound of military service on nonviolent offenses is (0.0015379*215,335 = 331). Thus, the Vietnam War military service increased the violent and nonviolent offenses by at least 1274 and 331, 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, (1,274*$403,915)+(331*$153,347) = $565,345,567.

potential. For instance, in 2019, Congress allocated $23 million in funding for the 2020 fiscal year for Veterans Treatment Court programs (U.S. 116th Congress, 2019).

VI. Conclusion

We examined the effect of conscription and military service on incarceration and recidivism outcomes using the Vietnam era draft lotteries as a possibly invalid instrumental variable (IV) for military service. To do this, we employed recently developed nonparametric bounds that relax the so-called exclusion restriction assumption (ER) that requires the IV to have an impact on the outcome only through the treatment. The draft lotteries may violate the ER since some males subjected to them had an incentive to engage in adverse behaviors to avoid conscription (for instance, commit delinquencies or simply not comply) that can have an effect on future incarceration. The bounds we employ also allow conducting statistical inference on two important groups that conventional IV methods are silent about: the military service volunteers and the overall group of veterans (that is, the treated group). The overall group of veterans is of first-order importance for policy, while the group of volunteers may be informative about the current U.S. all-volunteer forces (AVF).

Our main findings are as follows. First, we find evidence that could be interpreted as suggestive of positive direct effects of the Vietnam era draft lotteries (net of the military service channel) on the violent crime incarceration and recidivism outcomes of draft avoiders, particularly among the 1950-born cohort. That the 1950-born cohort exhibits these effects is consistent with their short reaction time to the draft documented in the literature, which likely limited the range of other behavioral responses they had available (for instance, educational deferment). The estimated bounds, which rely on the same conventional assumptions of the conventional IV estimates, sans the ER, are positive and often exclude zero. However, the estimated bounds' confidence intervals on this effect—particularly those employing conservative multiple testing adjustments—do not exclude zero. For this reason, the evidence can also be interpreted as simply not providing statistical evidence that the direct effects of the draft lotteries are non-zero. Under the first interpretation, the evidence would point towards a violation of the ER in the current empirical context since the ER assumption is required to be satisfied by everyone in the sample.

Second, we find heterogeneous effects of the military service on the incarceration and recidivism rates of the different cohorts of volunteers exposed to the draft lotteries. We find no statistical evidence of effects of military service on violent or nonviolent incarceration outcomes

for males 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 era military service increased the violent incarceration rate of whites by at least 0.20 p.p. and 0.31 p.p., respectively, with the 95% confidence intervals on the estimated bounds excluding zero. Also, for the white volunteers born in 1952, military service increased the nonviolent incarceration rates by at least 0.15 p.p. A complementary analysis of average characteristics of volunteers from the different birth cohorts using our individual-level data on inmates suggests that the factors that appear to be significant contributors to the crime instigation effect of military service relate to drug use, pre-draft criminal justice contacts, low socioeconomic status, and adverse family background. If the Vietnam era military service had crime instigation effects on military service volunteers who had drug abuse and criminal history records prior to the service, as our results suggest, then an 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 the current post-service efforts.

The last set of results pertain to the effect of military service on the incarceration outcomes of the overall group of veterans (that is, the average treatment effect on the treated). The group of veterans consists of compliers and volunteers, with the latter group accounting for about 78-84 percent. Thus, it is not entirely surprising that the results for all veterans mirror those for volunteers, although the estimated bounds for veterans are wider and less precisely estimated. Particularly, the 1951- and 1952-born white veterans experience an effect of military service on their violent incarceration rate of at least 0.17 p.p. and 0.25 p.p., respectively. The results for volunteers and veterans are large, as they represent an increase of at least between 83 and 160 percent relative to the incarceration rates of non-veterans. Lastly, the results in this paper suggest that the long-term tangible social cost of the violent and nonviolent offenses caused by Vietnam era military service is at least in the order of $565 million in 2019 US dollars. Importantly, if the crime-instigation effect of military service is more potent among volunteers, the AVF system instituted after the Vietnam war could have heightened it, which could explain the level of funding for programs aimed at ameliorating the involvement of veterans with the judicial and incarceration systems.

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I. Introduction

Recent estimates suggest that over 181,500 veterans are in the U.S. jails and prisons, and that veterans are 16% more likely to be sentenced for violent crimes than non-veterans (Bureau of Justice Statistics, 2015). The U.S. Department of Veteran Affairs (VA) has funded programs such as the Veteran Justice Outreach and the Veteran Treatment Courts to support the veterans' reintegration to society and prevent them from landing in prisons and jails. This paper seeks to contribute to our understanding of the relationship between conscription, military service, incarceration, and recidivism, with the aim of informing VA and U.S. policies to reduce veterans' incarceration and recidivism. We employ recently developed nonparametric bounds that allow examining the causal effects of the Vietnam war military service under relatively mild conditions. They also allow conducting inference on all veterans, including those that volunteered for service, a group that should be informative about the current U.S. all-volunteer forces (AVF).

Empirical studies that estimate the causal effects of military service on a myriad of post-service outcomes often use the Vietnam draft lotteries as a source of exogenous variation—that is, as an instrumental variable (IV)—for the Vietnam era military service (for instance, Angrist, 1990; Dobkin and Shabani, 2009; Angrist et al., 2010; Heerwig and Conley, 2013). This approach overcomes the missing counterfactual outcome problem (for instance, 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 a decision typically made by the individual. For instance, 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 draft lotteries generate exogenous variation in military service because military induction was based on a Random Sequence Number (RSN) that was assigned to potential draftees based solely on birthdays, making it independent of individuals' 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., with no noticeable effects for nonwhites.

This conventional IV approach has limitations. One limitation relates to the requirement that the draft lotteries eligibility (the IV) not affect the incarceration outcomes in any way except

through the indirect mechanism of military service. This requirement is known as the exclusion restriction assumption (ER). Unfortunately, it is plausible that the ER is not satisfied in the context of the Vietnam war draft lotteries. The leading factors for the potential direct (or net-of-military-service) effect of the draft-eligibility are draft avoidance behaviors. As an example, Kuziemko (2010) suggested the notion of "dodging down" as an avoidance behavior consisting of delinquencies and criminal activities, because having a criminal record was a way to avoid being drafted into military service by failing the military induction's "moral standards" (Suttler, 1970; Shapiro and Striker, 1970).[4] Besides, being drafted and refusing to serve also lead to convictions and prison sentences for draft offenders following the draft law (Baskir and Strauss, 1978). Peterson (1998) documents that almost half of the 570,000 traceable draft avoiders during the U.S. Vietnam war became accused draft offenders, and around 22,000 of them were convicted after being brought to trial. 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). Therefore, if those behaviors relate to future incarceration, they result in a direct effect of the draft lotteries (net of military service) on future incarceration. Under violations of the ER, the IV estimate of the effect of military service will have a bias of the same sign as the direct effect of the draft lotteries, independently of the sign of the effect of military service on incarceration.[5]

Another limitation of the conventional IV approach is that it solely provides estimates of the military service effect for individuals who would serve only when draft-eligible (that is, the "compliers"). Thus, the IV approach fails to provide estimates of the effect of military service for the volunteers (those who will serve regardless of draft-eligibility, or "always-takers") and consequently for all of those who serve (the treated population) that consists of both of these groups. The group of volunteers is important for at least three reasons. First, they represent a high proportion of the veteran population—between 78% and 84% of all servicemen from the cohorts (1948 to 1952) affected by the draft. Second, the group of volunteers may be of particular interest

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[4] Other studies have documented the effects on education of another type of draft avoidance behavior–using the education exemption and enrolling in universities to avoid the draft (Card and Lemieux, 2001; Deuchert and Huber, 2017).

[5] Lindo and Stoecker (2014) indeed recognize that the violation of the ER is one potential concern for the validity of their estimates using the draft lotteries as the IV for military service, and perform an indirect assessment of this possibility.

given that the U.S. military works under an AVF system since 1973. Insights about volunteers rather than about compliers are likely to be more relevant to the current U.S. military. Lastly, learning about the military effect of volunteers allows learning about the overall population of veterans, which encompasses both compliers and volunteers. Effects on treated individuals are of first-order importance for policy (see, for instance, Heckman et al., 1999).

To address the above limitations of the conventional IV approach, we employ recently developed nonparametric bounds in Flores and Flores-Lagunes (2013; hereafter FF-L) and Chen, Flores, and Flores-Lagunes et al. (2019; hereafter CFF-L). These bounds allow conducting inference on the effect of military service on incarceration and recidivism outcomes for the draft volunteers and the veteran population. They also allow for potential violations of the ER by the IV since they permit the draft lotteries to have a direct effect on the incarceration outcomes through channels net of their impact on military service. Intuitively, the methodology separates the total effect of the draft lotteries on the outcomes into an indirect (mechanism) effect that works through military service, and a direct (net) effect that does not work through military service. They provide bounds on each of those two effects, and the bounds on the indirect effect of the lottery draft—the only effect assumed to exist under the ER—are used to bound the military effect for volunteers, compliers, and veterans. For some of the bounds we employ, the ER is replaced with a mean weak monotonicity assumption that we describe and assess in detail later.

A challenge in estimating those bounds on effects on incarceration and recidivism outcomes is that individual-level data that is representative of the U.S. male population, and that contains the necessary information to determine lottery draft eligibility, is not publicly available. For this reason, we construct population-level incarceration rates by adapting 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 birth statistics from the Vital Statistics of the United States (VSUS) 1948-1952, and nationally representative estimates from Wang et al. (2020) of the population proportions of volunteers, compliers and draft avoiders.

We have three main sets of results. The first concerns the effects of conscription (the lottery drafts), net of military service. The estimated bounds on the direct effect of the draft lotteries for the draft avoiders born in 1948-1952 are suggestive of positive effects on the incarceration and recidivism outcomes considered, although their 90%-level confidence intervals do not statistically

rule out a zero effect. Considering that the behavioral responses by draft avoiders born in different years are plausibly different since the conscription circumstances changed (as discussed in section V.A.), we explored the heterogeneity of the direct effect by birth-year cohorts. Consistent with the relatively short reaction time to the draft that males born in 1950 had, likely limiting their behavioral avoidance responses (for instance, using educational deferments), their estimated bounds strongly suggest positive direct effects of military service on violent incarceration and recidivism. However, while the estimated bounds' confidence intervals exclude zero, confidence intervals that account for multiple testing do not.[6] We choose to interpret this evidence as suggestive of non-zero direct effects because the bounds employed to learn about the direct effect for draft avoiders dispose of the ER and do not add other assumptions. Under this interpretation, the estimates suggest that the draft lotteries may be an invalid IV for military service in the context of at least some incarceration and recidivism outcomes. Nevertheless, one can also interpret these results as simply not providing statistical evidence that the direct effects are non-zero, in which case the ER would not be rejected.

The second set of results concerns the effects of military service on the incarceration outcomes of volunteers—individuals who serve in the military regardless of their draft-eligibility. This group represents the majority of the Vietnam veterans and it may be informative about the current U.S. AVF. The estimated bounds on the effects for this group indicate that military service statistically increases the violent and nonviolent incarceration rates of white volunteers in certain birth cohorts (1951 and 1952). A simple analysis of the average characteristics of volunteers from different birth cohorts suggests that potential mediating factors of this effect are drug use, pre-service criminal justice contacts, low socioeconomic status, and disadvantaged family background. An implication is that an AVF system should be attentive to criminal justice contacts and drug use history during induction screenings.

The last set of results pertains to the incarceration and recidivism effects of military service for the population of veterans. Since veterans include compliers with the draft lotteries and volunteers, with the latter group representing 78-84% of veterans, it is not entirely surprising that

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[6] We apply multiple testing adjustment procedures (the Family-wise Error Rate or FWER, and the False Discovery Rate or FDR) across the five male birth cohorts under consideration (1948, 1949, 1950, 1951 and 1952) in the construction of the confidence intervals for the estimated bounds. Details of the procedures are in section IV.C.

the estimated bounds yield results similar to those found for volunteers. Military service statistically increases violent and nonviolent incarceration rates of white veterans from the 1951 and 1952 birth cohorts. The difference in results between volunteers and veterans is that the estimated bounds are somewhat wider and less precisely estimated for the group of veterans. The documented effects of military service on violent and nonviolent incarceration rates for volunteers and veterans appear substantial, as they represent at least between 47 and 160 percent of the corresponding incarceration rates of non-veterans.

This paper contributes to several strands of literature. First, it complements the literature on the consequences of the draft lotteries 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 draft lotteries on the subject cohorts soon after the draft, that is, when the individuals were in their early twenties. We show evidence that the draft lotteries may have increased the long-term incarceration for violent crimes for some males, 8 to 22 years after the draft, and that this effect may be separate from actual military service. Second, the paper contributes to the analysis of the crime and incarceration effects of the Vietnam era military service (for instance, Bouffard, 2014; Lindo and Stoecker, 2014; Teachman and Tedrow, 2014b) and 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 (for instance, Galiani et al. 2011, Albaek et al. 2017, Siminski et al. 2016, Hjalmarsson and Lindquist, 2019). Third, it contributes to the growing literature employing nonparametric bounds in IV models without the ER assumption (for instance, FF-L, 2013; Amin et al., 2016; CFF-L, 2019; Wang et al., 2020), by illustrating how this approach can be applied to situations where individual-level data is not available and auxiliary data is employed to undertake statistical inference under a bounded outcome. Lastly, we contribute to the growing literature on statistical methodologies to test implications of the assumptions underlying the conventional IV model with heterogeneous effects (Kitagawa, 2015; Huber and Mellace, 2015; FF-L, 2013; Mourifié and Wan, 2017) by empirically assessing the validity of the ER in the current empirical context, while adjusting for multiple testing.

II. The Vietnam Draft Lotteries

The Vietnam draft lotteries were a method adopted during the Vietnam war to fairly allocate military services in the U.S. They 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 draft lotteries (for instance, Angrist et al., 2010; Lindo and Stoecker, 2014) and focus on the 1948-1952 birth cohorts.[7]

Draft eligibility status based on the RSNs did not equate subsequent military induction. 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 military induction in the Vietnam era (Suttler, 1970; Shapiro and Striker, 1970; Baskir and Strauss, 1978).

An issue related to the randomization mechanism of the RSNs in the 1969 draft lotteries has been documented: men with birthdays in later months tended to receive lower lottery numbers and be drafted relative to men with birthdays in earlier months (Fienberg, 1971).[8] For this reason, the previous literature that employs the Vietnam draft lotteries as an IV for military service use birth month-by-year indicators to account for this issue (for instance, Angrist et al., 2010; Lindo and Stoecker, 2014). We will also account for this aspect within our bounding methodology.

III. Econometric Methods

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[7] 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 (for instance, Angrist et al., 2010). 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.

[8] 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).

We are interested in estimating the effects of military service on incarceration and recidivism outcomes. To avoid selection bias, we will employ the Vietnam era draft lotteries as an IV. However, we are particularly concerned with the validity of the ER due to factors that can result in direct effects of the IV on the outcomes through channels other than military service, rendering traditional IV estimators biased. We also have particular interest in learning about military service effects for individuals other than those who comply with their draft-eligibility status. For this reason, we adopt the nonparametric bounding techniques in FF-L (2013) and CFF-L (2019). These techniques allow the draft-eligibility IV to have a direct effect on the outcomes of interest, thereby disposing of the ER assumption. They also allow bounding the effect for the subpopulations of volunteers and compliers, thus allowing to place bounds on the effect for all veterans (that is, the average treatment effect on the treated). These bounding techniques rely on two main ideas. The first is to separate the total, reduced-form effect of eligibility to draft (the IV) on the outcomes of interest into an indirect effect that works through the channel of military service (that is, the military service induced by the draft), and a direct effect that does not work through the military service. This separation allows for violations of the ER assumption. The second idea is to define the direct and indirect effects at the level of the subpopulations of compliers, volunteers, and draft avoiders, and construct bounds on these "local" effects that can then be aggregated up to the level of other populations, such as the effect on all veterans. The exposition of the nonparametric bounds in the rest of this section focuses largely on the intuition behind those methods, while the interested reader is referred to FF-L (2013) and CFF-L (2019) for the details.

III.A Basic Setup

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 non-veterans) 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 follow Imbens and Angrist (1994) and Angrist et al. (1996) and partition the total population into four latent principal strata based on the values of the vector $\{D_{1i}, D_{0i}\}$. 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. One stratum is the draft avoiders or never-takers (nt): individuals who are non-veterans either when eligible or ineligible to draft ($D_{1i} = 0$, $D_{0i} = 0$). If draft avoiders receive a high lottery number that was not

called, they will not volunteer to serve; but if they receive a low lottery number that is probable to be called for induction, they likely undertake strategic actions to avoid the draft.[9] The second stratum is military service volunteers or always-takers (*at*): individuals who, regardless of whether they are eligible to draft, will serve in the military (D_{1i}=1, D_{0i} =1). This stratum is potentially relevant to the current AVF system since they do not need conscription to serve. The third stratum is the compliers (*c*): individuals who will serve in the military only if their lottery number is called to enlist (D_{1i}=1, D_{0i} =0). The literature using the draft lotteries as an IV estimates effects only for this stratum. The last group is the defiers (*d*): individuals who 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). This stratum will be ruled out by assumption below.

Define the 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, which are standard in the literature (for instance, Imbens and Angrist, 1994). In contrast, the last two potential outcomes are counterfactual incarceration outcomes that are never observed in the data, but that are key to relax the ER assumption. The third potential outcome is 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 is 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. These last two potential outcomes will be employed to decompose the draft lotteries' total effect into an indirect (mechanism) and a direct (net) effect. 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.

The nonparametric bounding techniques we employ rely on the following assumptions that are also used in the conventional IV method (Imbens and Angrist, 1994; Angrist et al., 1996). The

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

first assumption, A1, is the random assignment of the instrument Z (the draft lotteries eligibility). The Vietnam draft lotteries satisfy 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 (for instance, 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 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 conventional IV method uses an additional assumption to point-identify the effect of military service on the outcome for the stratum of compliers ($LATE_c$):

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

This additional assumption, referred to as the exclusion restriction (ER), states that the draft lotteries do not affect incarceration outcomes in any way except through military service: $Y_i(0, d) = Y_i(1, d)$ for all i. This assumption can be interpreted as ruling out a non-zero direct effect of the eligibility to draft on incarceration outcomes. Given concerns that the ER may not be satisfied in this setting, our methodology will not employ this assumption.

III.B Three Parameters of Focus

The main insight to allow for possible violations of the ER is to decompose the total effect of the draft lotteries on a given incarceration outcome, $E[Y(1) - Y(0)]$, into two parts (see FF-L, 2010, 2013; CFF-L, 2019, and references therein). The first part is the direct (net) average treatment effect or $NATE^Z$. It is the effect of the draft lotteries on incarceration that is not related to (or is net of the effect that works through) the military service:[10]

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

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[10] 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 behaviors, as long as these channels are different from the actual military service.

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

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

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 $NATE^Z$. 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. Thus, we omit the superscript Z in what follows and focus on estimating parameters averaged over Z. Also, note that the ER shuts down the $NATE$ by assumption, that is, it rules out the relevance of any other channel that results in direct effects of the draft lotteries on the crime outcomes. Thus, an interpretation of $MATE$ is that it contains the "good" part of the effect of the lottery draft on the outcome that works through the military service, which can be used to (partially) identify the effect of military service within the IV framework (FF-L, 2013; CFF-L, 2019).

The effects in (2) and (3) can be defined for each one of the three principal strata, corresponding to "local" direct and indirect effects for draft avoiders, volunteers, and compliers. Note that, by definition of the strata, the indirect effect of the draft lotteries for draft avoiders and volunteers is zero since the draft lotteries do not change their potential military service status. Consequently, the direct effect of the draft lotteries for draft avoiders and volunteers coincides with their total effect. In contrast, for compliers, both their direct and indirect effects of the draft lotteries can in principle be non-zero. CFF-L (2019) provides nonparametric bounds for each of the direct and indirect local effects of the draft lotteries, local effects of the military service, as well as the corresponding effects for populations that are unions of principal strata (for instance, the group of veterans).

We focus on bounding three of those effects in this paper. First, since the direct effect of the draft lotteries is likely to work through the channel of draft avoidance behaviors, we focus on the direct effect of the draft lotteries on the draft avoiders' incarceration outcomes:

(4) $LNATE_{nt} = E[Y(1, D_z) - Y(0, D_z)|nt]$ for $z = 0, 1$.

A nonzero direct effect of the draft lotteries on the draft avoiders is consistent with a violation of the ER. The second effect we focus on is the effect of the Vietnam war military service on incarceration outcomes for the volunteers, which can be expressed as:

(5) $LATE_{at} \equiv E[Y(z,1) - Y(z,0)|at]$,   for $z = 0,1$.

The third effect of interest is the effect of military service on incarceration outcomes for the population of veterans, or the average treatment effect on the treated (ATT):

(6) $ATT \equiv LATE_{\{c,at\}} \equiv E[Y(z,1) - Y(z,0)|D = 1]$,   for $z = 0,1$.

As previously mentioned, the population of veterans consists of both compliers and volunteers, and it is a population of first order importance. Even though prior literature (for instance, Lindo and Stoecker, 2014) estimates the military effects for compliers, we do not focus on this subpopulation because our bounds are wide for this group, likely due to the relatively small size of this stratum that does not allow more precise statistical inference.

III.C Nonparametric Bounds on the Three Parameters of Focus

In this subsection, we provide intuition for the partial identification results in FF-L (2013) and CFF-L (2019) that underlie the estimated nonparametric bounds on the three effects of focus.

Under assumptions A1-A3, the direct effect of the draft lotteries on the draft avoiders in (4) can be partially identified. Specifically, note that the first term in (4) is point identified as $E[Y(1)|nt] = E[Y|Z = 1, D = 0]$, which follows from Table 1 once the d stratum is eliminated. Additionally, letting $p_{d|z} \equiv \Pr(D_i = d|Z_i = z)$ for $d, z = \{0,1\}$, the population proportions of the three strata, denoted as π_{at}, π_c and π_{nt}, are point identified as $\pi_{at} = p_{1|0}, \pi_c = (p_{1|1} - p_{1|0})$, and $\pi_{nt} = p_{0|1}$. Then, while we cannot distinguish the never-takers from compliers when they are both ineligible-to-draft and did not serve in the military (the upper left cell in Table 1), bounds can be provided for the second term in (4) using "trimming bounds" (Lee, 2009; Zhang et al., 2008). To illustrate, note that the average outcome for the observed group with $\{Z = 0, D = 0\}$ can be written as a weighted average of outcomes of the nt and c strata (Imbens and Rubin, 1997):

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

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. With all the components in (4) either point or partially identified, bounds on this local effect are obtained.

It is important to note that the bounds on $LNATE_{nt}$ in (4) rely on the same assumptions as the conventional IV estimates, sans the ER assumption. Thus, maintaining A1-A3, if the estimated

bounds on $LNATE_{nt}$ statistically exclude zero, this provides statistical evidence of the existence of a direct effect of the draft lotteries on the incarceration outcomes for (at least some) draft avoiders. In turn, given that the ER assumption is imposed on every unit in the population, this implies the invalidity of the draft lotteries as an IV for military service in this context.[11]

In contrast to $LNATE_{nt}$, additional assumptions are needed to construct bounds on the other two effects, $LATE_{at}$ and ATT. The reason is that some objects in (5) and (6) are never observed in the data. These are the counterfactual outcome of volunteers had they not served in the military, $Y(z, 0)$, and the counterfactual outcomes $Y(1, D_0)$ and $Y(0, D_1)$ for compliers, the latter two needed to bound ATT. Bounds for the corresponding expectations of these counterfactual outcomes can be constructed under the following two assumptions that have been employed in prior literature (for instance, FF-L 2010, 2013; Huber et al., 2017; CFF-L, 2018, 2019). The first assumption is that the outcome is bounded, which provides natural bounds for the required expectations. The second assumption imposes weakly monotonic relationships of average potential outcomes across strata that share the same draft eligibility.[12] Formally,

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

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}.

A4 states that the potential incarceration outcomes have a bounded support, which is satisfied in our setting since the outcomes considered are binary. Thus, by providing bounds on the expectations in (5) and (6) of counterfactual outcomes that are never observed in the data, A4 permits the construction of bounds on $LATE_{at}$ and ATT. In contrast, A5 is substantive. It formalizes the notion that particular strata likely have characteristics that make them less likely to be imprisoned than other strata. Specifically, in our empirical setting, A5 states that, conditional

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[11] Related work that proposes statistical tests for implications of assumptions A1 to A4 are Huber and Mellace (2015), Kitagawa (2015), and Mourifié and Wan (2017).

[12] The same prior literature has also considered an assumption that restricts the local average effects to be either non-positive or non-negative. We do not consider this assumption here since the previous literature on the effects of the draft lotteries and the military service on future incarcerations is inconclusive about the effect's sign (for instance, Kuziemko, 2010; Lindo and Stoecker, 2014; Albaek et al., 2017).

on the same eligibility to draft and potential veteran status, the compliers are (weakly) less likely to be incarcerated than the volunteers, who in turn are (weakly) less likely to be incarcerated than the draft avoiders. A5 improves on the natural bounds provided by A4 since now point- or partially-identified mean potential outcomes for certain strata serve as tighter bounds for the expectations in (5) and (6) of counterfactual outcomes that are never observed in the data. Given that A5 is a crucial assumption, we extensively discuss and assess it in section V.B.1. Online Appendix A provides expressions for the nonparametric bounds we employ, their derivation, and proofs, while we present details on their estimation and statistical inference in section IV.C below.

IV. Data and Empirical Strategy

IV.A Data Sources and Key Variables

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 current offenses in his record involve violent offenses, and we classify inmates as incarcerated for a nonviolent crime otherwise.[13] This classification is different from the measure in Lindo and Stoecker (2014) that focuses on original offenses in the inmate's record. The conclusions from the analysis conducted below are very similar with any of the two measures, and also using a measure that

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[13] 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.

classifies inmates as incarcerated for a violent crime if any of his records show a violent offense (see Online Appendix G). Besides incarceration outcomes related to current offenses, we construct a measure of recidivism in order to analyze the military service effect on this important aspect. For each of the violent and nonviolent recidivism outcomes, an indicator variable is set to one if the inmate is currently incarcerated for violent or nonviolent crimes, respectively, and also had juvenile criminal justice contacts before they became eligible for military induction, 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.[14] The use of age 18 is because age 18½ was the lowest induction age during the Vietnam war. The use of the year 1968 for prior incarcerations is because the exact age of prior incarceration is not available in SISFCF, but the year of admission to an incarceration facility is; 1968 is chosen since the first draft lottery took place in December 1969. 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, which consists of 2700 white and 2619 nonwhite inmates. The table shows summary statistics on veteran status, draft-eligibility, crime outcomes, and estimated strata proportions (under A1 and A3), for white and nonwhite inmates. We see that a higher proportion of white inmates served in the Vietnam era war, and while nonwhite inmates have a higher estimated proportion of draft avoiders than whites, white inmates have a higher estimated proportion of volunteers. Note that the estimated proportion of compliers is small for white and nonwhite inmates, and it is not significantly different from zero

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[14] 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 [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)?".

for white inmates. These estimated proportions of compliers using the inmate sample are smaller than those for the corresponding U.S. population, which are between 7-14% (reported in Online Appendix C). The estimated proportions of compliers from the inmate sample are not employed by our methodology. Regarding the criminal offending status, white inmates are less likely to be violent crime offenders and to have been incarcerated before 1968 regardless of their current criminal offending status, relative to nonwhite inmates. Lastly, white and nonwhite inmates show similar proportions on arrests and probation before 18 years of age.

To construct the outcomes, 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 from the VSUS by race to calculate incarceration rates for males born between 1948 and 1952.[15] The outcomes are constructed from mean incarceration or recidivism rates for a certain crime type in survey year s, of males born in birth year b and birth month m, with eligibility to draft z, and veteran status d:

$$(8) \qquad Incarceration\ outcome_{sbm}(z,d) = \frac{\#of\ inmates_{sbm}(z,d)}{\#of\ Births_{(z,d\,|\,b,m)}}.$$

The numerator of the constructed incarceration rate outcome in (8) is the inmate counts by characteristics s, b, m, z, and d 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.[16] For reference, Table 3 summarizes the estimated counts of male inmates born in 1948-1952, broken down by draft-eligibility and veteran status. The denominator in (8) is the male population in the U.S. defined by characteristics b, m, z, and d. To construct it, we employ the VSUS.

We note that the recidivism outcomes constructed following Equation (8) are not comparable to the conventional measure of recidivism that expresses the number of recidivists

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[15] 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).

[16] 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).

divided by the count of previously incarcerated individuals. To create a comparable measure, 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 draft lotteries, which is not available to us. Instead, the measure of recidivism following Equation (8) is constructed by dividing the inmate count of recidivists by the U.S. male population count (for each subgroup defined by characteristics b, m, z, and d). 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 offenses 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.[17] These estimates largely suggest no statistically significant "intention-to-treat" effects of the draft-eligibility on the outcomes of whites or nonwhites.

To estimate our bounds, we need estimates of the population strata proportions (under A1-A3) from a dataset that is representative of the U.S. population. Wang et al. (2020) used a special version of the 1982-1996 National Health Interview Survey (NHIS) to estimate the k strata proportions in the U.S. population, which we borrow here. Specifically, they constructed draft-eligibility and Vietnam era military service variables, which allows estimating the strata proportions under assumptions A1-A3. Online Appendix C presents these estimated U.S. male population strata proportions.

IV.B Estimation Strategy

The bounds described in section III.C can, in principle, be estimated with analog estimators. For example, the mean potential outcomes of the eligible-to-draft draft avoiders $(E[Y(1)|nt])$ and of the ineligible-to-draft draft volunteers $(E[Y(0)|at])$ can be estimated as $\sum_{i=1}^{N}\{Y_i \cdot I(Z_i = 1, D_i = 0)/I(Z_i = 1, D_i = 0)\}$ and $\sum_{i=1}^{N}\{Y_i \cdot I(Z_i = 0, D_i = 1)/I(Z_i = 0, D_i = 1)\}$, respectively. The first four columns of Table 5 present these estimates and the mean incarceration rates of the observed groups $E[Y|Z = 0, D = 0]$ and $E[Y|Z = 1, D = 1]$, which consist of a mixture of strata. From section III.C., the population strata proportions can be

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[17] 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 can be found in Online Appendix F.

estimated using the NHIS data as $\pi_{at} = p_{1|0}$, $\pi_c = (p_{1|1} - p_{1|0})$, and $\pi_{nt} = p_{0|1}$. For the trimmed means related to the trimming bounds motivated in section III.C. (Equation 7), they could be easily estimated with access to individual-level outcome data. All of these analog estimates would then be plugged into the expressions for the nonparametric bounds presented in Online Appendix A.

As detailed in the previous subsection, however, we lack access to individual-level outcome data. Instead, we innovate by estimating the trimming bounds exploiting the binary nature of the outcomes.[18] The informativeness of the estimated bounds is aided by the relative magnitudes of the incarceration rates and the trimming proportions. To illustrate, consider computing trimming bounds on $E[Y(0)|nt]$ for the violent crime incarceration outcome of white draft avoiders. From (7), the trimming bounds for this object use the observed group of ineligible-to-draft non-veterans ($\{Z = 0, D = 0\}$) and the trimming proportion $\pi_{nt}/(\pi_c + \pi_{nt})$. The average incarceration outcome for the observed group, $E[Y|Z = 0, D = 0] = 0.0018$, is smaller than the trimming proportion $\pi_{nt}/(\pi_c + \pi_{nt}) = 0.82$. Therefore, as shown in Figure 2, the $(1 - 0.82)$-th percentile of Y in that observed group ($y_{1-0.82}^{00}$), which corresponds to the left end of the bracket in the top panel, and the 0.82-th quantile of Y in the same observed group ($y_{0.82}^{00}$), which corresponds to the right end of the bracket in the bottom panel, must both equal zero. Hence, the upper bound of $E[Y(0)|nt]$ is computed by dividing the estimated number of inmates in the group with $\{Z = 0, D = 0\}$ by the estimated total male population that belongs to the draft avoiders stratum with $\{Z = 0, D = 0\}$; and the lower bound of $E[Y(0)|nt]$ is zero following the argument above. Lastly, the upper and lower bounds on the direct effect of the draft lotteries on draft avoiders, $LNATE_{nt}$, are estimated by subtracting the lower and upper bounds of $E[Y(0)|nt]$, respectively, from the point estimated $E[Y(1)|nt]$.

To illustrate the construction of the bounds, Table 5 presents the intermediate steps in the estimation of the upper and lower bounds on $LNATE_{nt}$ for the 1948-1952 born draft avoiders: the point estimates of $E[Y(1)|nt]$ and the upper and lower bound estimates on $E[Y(0)|nt]$. Similar steps are employed in the estimation of the bounds on the other parameters of focus, although sometimes the expressions of the bounds involve taking the max or min over objects that are

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[18] We thank Carlos A. Flores for pointing out to us that the bounds in FF-L (2010, 2013) and CFF-L (2019) can still be computed without individual-level data when the outcome is binary.

candidates to be bounds (see Online Appendix A). Note also that the width of the nonparametric bounds partially depends on the relative strata proportions. For example, for the upper bound of $E[Y(0)|c]$, the $\pi_c/(\pi_c + \pi_{nt})$ largest values of Y are used to construct the trimmed means. Since $\pi_c/(\pi_c + \pi_{nt}) < \pi_{nt}/(\pi_c + \pi_{nt})$ in all birth cohorts, the estimated upper bounds on $E[Y(0)|c]$ (obtained using trimming) will be wider than the similarly estimated upper bounds on $E[Y(0)|nt]$.

Lastly, to account in our estimation procedure for the issue related to the randomization mechanism of the RSNs in the 1969 draft (Fienberg, 1971) described in section II, we first conduct estimation of all relevant objects and effects for each month of birth and then construct a weighted average of those estimates across all the birth months, weighted by the male population born in each month. This procedure, which is applied to all draft years, effectively controls for month of birth in the estimation of the bounds.

IV.C Statistical Inference

For the nonparametric bounds that do not involve max and min operators (see Online Appendix A), statistical inference is based on confidence regions for the true parameter of interest following Imbens and Manski (2004). Other nonparametric bounds we use involve those operators, which break down standard statistical inference (Hirano and Porter, 2012). To conduct valid inference on those bounds, we rely on the methodology proposed by Chernozhukov, Lee, and Rosen (2013), described in Online Appendix B. 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.

Furthermore, since we analyze the direct effects of the draft lotteries and military service effects in several subsamples defined by birth cohorts, we are mindful of the potential problem of performing multiple testing of null hypotheses. It is well known that the situation of multiple testing increases the risk of falsely rejecting a true null hypothesis of a zero effect. To control for this, we employ three different sequential multiple testing procedures. The first is the sequential Family-wise Error Rate (FWER) testing procedure (Holm, 1979). The second and third procedures are the sequential False Discovery Rate (FDR) by Benjamini and Hochberg (1995) and the sharp

sequential FDR in Benjamini, Krieger, and Yekutieli (2006). To implement the multiple testing procedures to our estimated bounds, we follow Mourifie and Wan (2017).[19]

V. Results

In this section, we present the results employing the estimated nonparametric bounds on the three parameters of focus. We also discuss implications of the results, conduct additional analyses to increase our understanding of the results obtained, and provide estimates of the social costs implied by our estimates.

V.A Direct Effects of the Draft Lotteries on the Outcomes of Draft Avoiders

We begin by analyzing the direct effects of the draft lotteries on the incarceration and recidivism outcomes of draft avoiders. While we start by analyzing all the relevant cohorts (1948-1952) combined, we subsequently explore the heterogeneity of the direct effects by cohorts whose behavior may had been affected differently by the draft. Throughout the results in this subsection, only the conventional IV assumptions are employed sans the ER assumption (A1 to A3).

Figure 3 presents estimated bounds for the direct effect of the draft lotteries on the draft avoiders (nt) stratum. The shaded bar and the capped intervals represent the estimated bounds and confidence intervals (90 and 95 percent), respectively; the dark dots show, for reference, the mean incarceration rates of ineligible-to-draft non-veterans. The top two panels in Figure 3 present estimated bounds and confidence intervals on the direct effect of the draft lotteries for the 1948-1952 born white (Panel A) and nonwhite (Panel B) draft avoiders. We present results by race since prior literature has found that the effects of conscription and of military service vary over this dimension (for instance, Kuziemko, 2010; Lindo and Stoecker, 2014). The estimated lower bounds for whites suggest that the direct effect of the draft lotteries on their violent incarceration and recidivism (first and third bars) outcomes 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 non-veterans). For nonwhites, the estimated lower bounds are consistent with a direct 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. (13.6%; the fourth bar). However, the previously discussed bounds are not precisely

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[19] In the case in which the bounds involve max and min 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 hypotheses tested for each effect across the birth cohorts under analysis.

estimated, as their 90% confidence intervals do not exclude a zero direct effect. The estimated lower bounds on the other outcomes presented in Panels A and B of Figure 3 are negative, thus not excluding a zero direct effect. In sum, for the combined 1948-1952 birth cohorts, we do not find strong evidence of non-zero direct effects of the draft lotteries on the incarceration outcomes.

We next explore the heterogeneity in the direct effect of the draft lotteries over the cohorts exposed to the draft. In principle, there could be differences in the avoidance behaviors over cohorts, caused by the different timing and circumstances of the draft lotteries conducted in 1969, 1970, and 1971. Males born in 1948 to 1950 were subjected to the first national lottery draft conducted in December 1969, where drafted men were called for physical examinations and inductions starting in January 1970. This timeline stands in stark contrast with the 1951- and 1952-born males who were subjected to the draft lotteries of July 1970 and August 1971, respectively, and who had more time to strategically react to their lottery numbers since the call for inductions begun at the start of the following year (and they also had the benefit of witnessing the earlier draft). Moreover, in the 1970 and 1971 drafts, the lottery numbers called for induction were (ex-post) 36% and 51% lower relative to those in the 1969 draft. These circumstances suggest that men exposed to the 1969 draft may have had to resort to immediate avoidance behaviors (for instance, delinquency or draft evasion) instead of other avoidance behaviors (such as educational deferments) given the limited reaction time they had. There is another aspect that distinguishes one of the three cohorts subjected to the 1969 draft: while men born in 1948 and 1949 could have been subjected to earlier years' local drafts—and therefore already "picked over" and/or prepared to behave strategically to dodge conscription—men born in 1950 were subjected for the first time to conscription through the national 1969 draft. In sum, we expect that cohorts who had a shorter time to react to the draft lotteries and were not previously subjected to local drafts might had been more likely to engage in immediate avoidance behaviors that could have impacted negatively their incarceration outcomes.

The results on the direct effect of the draft lotteries on draft avoiders by cohort suggestively reflect the previous arguments regarding the drafts they were subjected to: the cohorts subjected to the 1969 draft tend to have estimated bounds that are more consistent with positive direct effects of the draft lotteries on their incarceration outcomes. The results for all birth cohorts are available in Online Appendix D, while Panels C and D in Figure 3 present the estimated bounds and confidence intervals for the direct effect of the draft lotteries on the 1950 cohort that likely had the

least reaction time. The estimated bounds for this cohort are estimated more precisely relative to other cohorts, although the estimated bounds across cohorts do exhibit a large degree of overlap.

Panel C of Figure 3 presents estimated bounds and confidence intervals for the direct effect of the draft lotteries for the 1950-born white draft avoiders. Their estimated lower bounds suggest that the direct effect of the draft lotteries is to increase the incarceration rates for violent crimes by at least 0.07 p.p. (38.4%; the first bar), violent recidivism by at least 0.03 p.p. (40%; the third bar), and nonviolent recidivism by at least 0.02 p.p. (21.9%; the fourth bar). Nevertheless, the 90 percent confidence intervals for these effects marginally include zero. For nonviolent crime incarceration, the estimated bounds include a zero direct effect. Panel D of Figure 3 presents estimated bounds and confidence intervals for the direct effect of the draft lotteries for the 1950-born nonwhite draft avoiders. Their estimated lower bounds imply a direct effect of the draft lotteries on violent crime incarceration of at least a 0.46 p.p. (40.8%; the first bar) increase, and on violent crime recidivism an increase of at least 0.38 p.p. (69.0%; the third bar). For these two effects, the 95 percent confidence intervals exclude zero. The estimated bounds also suggest that the draft lotteries directly increase nonviolent crime incarceration by at least 0.33 p.p. (38.5%; the second bar) and nonviolent crime recidivism by at least 0.04 p.p. (10.3%), but the corresponding 90 percent confidence intervals do not exclude a zero direct effect. A valid concern in our exploration of heterogeneity in the direct effects of the draft lotteries on draft avoiders is that we have conducted tests of hypotheses in several subsamples, and thus rejection of the null of no direct effects could occur by chance. For this reason, we implement the three conservative multiple testing procedures described in Section IV.C. that allow statistically controlling for a valid significance level when simultaneously testing the null hypothesis of a zero direct effect over all birth cohorts. Applying these conservative inference procedures, we do not reject the null hypotheses that the direct effect of the draft lotteries is zero for any of the outcomes in Panel D.[20]

Overall, our interpretation of the results is that there is some evidence suggesting that the direct effect of the draft lotteries (unrelated to military service) increases future violent crime

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[20] The estimated bounds on the direct effect of the draft lotteries on the outcomes are more precisely estimated in the pooled sample of white and nonwhite 1950-born draft avoiders. In this case, draft-eligibility directly 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 statistical significance of these results withstand the multiple testing conservative adjustments.

incarceration and recidivism for draft avoiders, particularly for those in the 1950-born cohort. Despite the confidence intervals including a zero-effect, we choose this interpretation because the bounds employed to learn about the direct effect dispose of the ER while not adding other assumptions. Given these mild assumptions, it is remarkable that most of the bounds exclude zero effects, although the corresponding confidence intervals include zero effects at conventional statistical levels, especially once multiple testing procedures are employed; that is, the bounds seem to be imprecisely estimated. Of course, one can also interpret these results as simply not providing statistical evidence that the direct effects are non-zero. Under the former interpretation, the estimates suggest that the draft lotteries may be an invalid IV for military service in the context of at least some incarceration and recidivism outcomes, since the ER assumption must be satisfied by every unit in the sample. As a consequence, point estimates of the effect of military service on violent incarceration and recidivism outcomes based on conventional IV methods using the draft lotteries IV may have to be considered with caution. Lastly, note that the estimated direct effects of the draft lotteries presented in this section are for the draft avoiders, and thus cannot be generalized to the groups of draft volunteers and compliers.[21]

What channels might explain the presence of a direct effect on the violent crime incarceration and recidivism rates of draft avoiders? One plausible channel 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. Another possible channel is simply draft evasion, which in several instances resulted in prosecution and conviction (see, for instance, Peterson, 1998).[22] In both cases, early delinquency and incarceration can increase later years' incarceration and recidivism. The notion of increased adult criminal behavior after contacts with the judicial system as a youth has been documented in Bayer et al. (2009) and Aizer and

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[21] The groups of volunteers and compliers may experience direct effects on their incarceration outcomes. However, we do not estimate bounds for their effects since we are not aware of theoretical or anecdotal support for them to experience these effects. Still, the methods we employ next do not assume that those effects are zero.

[22] Another possibility, suggested to us by an anonymous referee, is labor market discrimination of potential employers against draft avoiders, since the RSNs were published and potential employers could have identified draft avoiders by their date of birth. This would make formal work less rewarding for draft avoiders relative to illegal work.

Doyle (2015). Both papers document that earlier year incarcerations significantly increase recidivism both for violent and nonviolent crime types.[23]

V.B The Effects of Military Service on the Outcomes of Volunteers and Veterans

We now turn to statistical inference on the effects of military service on incarceration and recidivism outcomes for volunteers and the population of veterans. We start with a detailed assessment of the key assumption we employ to construct bounds on this effect while disposing of the ER assumption. Subsequently we present results for volunteers, a group of singular importance, followed by secondary analyses aimed to understand some of the potential channels behind their effects. Lastly, we present results for the population of veterans.

V.B.1 Discussion and Assessment of Assumption A5

The key assumption we employ to derive bounds on the effects of military service on veterans is A5. This assumption implies that, conditional on the same eligibility to draft status and potential veteran status, compliers are (weakly) less likely to be incarcerated than the volunteers, who in turn are (weakly) less likely to be incarcerated than the draft avoiders. We offer three indirect assessments. One is based on the idea that estimated pre-draft incarceration outcomes by strata can inform the proposed ranking in A5. A second argument is based on the relative high school completion rate of the three strata, combined with the documented relationship between schooling and crime in the literature. The last argument is based on a testable implication of the bounds to be employed, which can be used to "falsify" the set of assumptions A1 to A3 plus A5.

The average pre-draft outcomes for each stratum can be estimated under assumptions A1 to A3 using individual-level data (FF-L, 2010, 2013; CFF-L, 2018, 2019).[24] However, the

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[23] An alternative channel through which the draft lotteries may have a direct effect on incarceration and recidivism outcomes is the "dodging-up" avoidance behavior, such as obtaining admissions into college to avoid the draft (for instance, Card and Lemieux, 2001). This type of avoidance behavior, resulting in higher educational attainment, is predicted to reduce incidence of criminal activities given the negative 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. Indeed, the counteracting effects of these avoidance behaviors may be a reason why the estimated bounds do not exclude zero more often and are relatively imprecisely estimated.

[24] Intuitively, for the *nt* stratum, the average pre-draft outcomes correspond to the mean pre-draft outcomes of eligible-to-draft non-veterans, 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 non-veterans, and both the strata proportions and the average pre-draft outcomes of *at* and *nt* are identified.

individual-level data available to us is only for inmates. Thus, we undertake two different suggestive exercises, each of which is imperfect since the resulting estimated average pre-draft outcomes are likely biased. The first is to employ data exclusively on inmates from the SISFCF. A problem is that the resulting estimates are likely not representative of the U.S. male population due to self-selection into incarceration. In the second exercise, we compute average pre-draft outcomes by strata by counting the number of inmates belonging to a specific stratum and who experience the pre-draft outcome, and dividing this number by the estimated U.S. male population who belong to that same stratum, using data from the VSUS and the estimated population strata proportions from Wang et al. (2020). In this case, bias in the estimates may arise because the average pre-draft outcomes in the non-incarcerated population may differ over strata. Even though the biases in the two methods are generally different from each other, they both lend indirect support to the weak ranking of the strata in A5.[25]

The estimated average pre-draft outcomes using the two methods explained above are presented in Panel A and Panel B of Table 6, respectively.[26] We focus on three pre-draft outcomes: arrests, probation, and incarceration before being subjected to the draft (turning 18 years old or before the year 1968). The estimated pre-draft averages under both methods indicate that draft avoiders were more likely to have been arrested, on probation, and incarcerated before they were subjected to the draft, relative to compliers and volunteers combined (Columns 5 and 6). Since 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 (see, for instance, Bayer et al., 2009; Aizer and Doyle, 2015), these estimates offer indirect support to the weak ranking of draft avoiders relative to the other two strata in A5. Turning to the weak monotonicity relationship in A5 between volunteers and compliers, Table 6 suggests that, on the population-scaled level (Panel B), volunteers have higher average rates of probation before the draft relative to a group that combines volunteers and compliers (bold figure in Column 7). Although on the inmate level (Panel A) the

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[25] Online Appendix E presents the formal mathematical expressions of the biases in these two ways of computing average pre-draft outcomes by strata using the inmates sample.

[26] Note that, in contrast to other papers using nonparametric bounds (FF-L, 2010, 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, as previously mentioned, the proportion of compliers in the inmate sample is quite low. These small proportions do not allow the estimation of the complier's average pre-draft outcomes with precision.

estimates suggest that volunteers have lower criminal justice contacts before the draft than the observational group of volunteers and compliers combined (Column 7), none of these differences between the two groups are statistically significant. Thus, overall, we find indirect evidence supporting the weak ranking of strata postulated in A5 and we do not find statistically significant evidence contradicting such ranking.

The second source of indirect support for A5 is based on the relative high school completion rate of the three strata which, in light of the literature on the relationship between schooling and crime outcomes, supports the weak ranking of strata in A5. More specifically, using the restricted-use representative data from the NHIS, Wang et al. (2020) report that, for whites, compliers have higher high school completion rate (0.93) relative to volunteers (0.88), whom in turn have higher high school completion rate relative to draft avoiders (0.85), with their differences being statistically significant. For nonwhites, compliers have higher high school completion rate (0.99) relative to volunteers (0.87), whom in turn have higher high school completion rate relative to draft avoiders (0.73), with their differences being statistically significant with the exception of the difference between compliers and volunteers. Given the negative relationship between education and incarcerations (see, for instance, Lochner and Moretti, 2004), this evidence indirectly supports the weak ranking of the potential incarceration outcomes in A5.

The final evidence we present in support of A5 relies on one testable implication that follows from the derivation of the bounds using assumptions A1 to A3 plus A5, which can be used to "falsify" those assumptions (CFF-L, 2019).[27] Recall that A5 indicates that the potential incarceration rate outcome of draft avoiders should not be lower than those for the compliers and the draft volunteers. 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 draft avoiders, $E[Y(1)|nt]$, must not be smaller than the conditional mean $E[Y|Z = 1, D = 1]$, which is the mean of the draft-eligible volunteers and compliers, $E[Y(1)|at, c]$. Table 7 presents 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. Thus, the testable implication is not statistically rejected for any of the outcomes or

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[27] 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.

analysis groups. The same conclusion is reached when these groups are broken down by birth cohorts (not shown).

V.B.2 The Effects of Military Service on the Outcomes of Volunteers

We now move on to the subpopulation of Vietnam era volunteers—an important group since it can be informative about the current AVF in the U.S. The results we present for them are novel in the context of IV methods since the subpopulation of focus there is the compliers. We have discovered heterogeneous effects of military service on incarceration and recidivism outcomes for volunteers born in different years. In Figures 4 and 5, the dark dots represent the mean incarceration rates of non-veterans, reported for reference. Results for white and nonwhite volunteers born in 1948-1952 and 1950 are presented in Figure 4. All the corresponding estimated bounds include zero, with the exception of the bounds on the nonviolent crime incarceration and recidivism for the 1950-born nonwhites (Panel D, Figure 4). However, the 90 percent confidence intervals on those bounds do not rule out a zero military effect. Thus, the results for the 1948-1952 and 1950 born whites and nonwhites in Figure 4 suggest that the military service crime effect may not be different from zero.

In contrast, turning to the 1951 and 1952 birth cohorts in Figure 5, most of the estimated bounds on the effect for volunteers—and some of their estimated confidence intervals—exclude zero. Specifically, for white volunteers 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. (Panels A and B of Figure 5), respectively. These are potentially large effects as they represent at least 144 and 160 percent of the mean outcome of non-veterans, respectively. For the outcome of nonviolent crime incarceration for the white volunteers born in 1951 and 1952, the estimated bounds indicate that military service increases the incarceration rates by at least 0.08 p.p. (49%) and 0.15 p.p. (86%), respectively. Furthermore, the 90% confidence intervals on the four estimated bounds just mentioned exclude zero. As for the recidivism outcomes, the estimated bounds for white volunteers born in 1951 and 1952 indicate that the effect of military service is to increase violent recidivism by at least 0.03 p.p. (44%) and 0.08 p.p. (80%) respectively, and nonviolent recidivism by at least 0.04 p.p. (56%) and 0.02 p.p. (23%), 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 violent recidivism for the 1952-born white volunteers.

The results for nonwhite volunteers born in 1951 and 1952 are presented in Panels C and D of Figure 5, respectively. The bounds for the 1951-born volunteers indicate an increase in violent incarceration rates of at least 0.32 p.p. (23%), and an increase in nonviolent recidivism rates of at least 0.15 p.p. (31%). The estimated bounds' 90% confidence intervals, however, do not exclude zero. For the 1952 birth cohort, military service increases violent crime incarceration of nonwhite volunteers by at least 0.72 p.p. (44%) and their corresponding 95 percent confidence intervals exclude zero. Military service also increases their nonviolent incarceration rate by 0.4 p.p. (34%), although the confidence intervals do not rule out zero. The rest of the estimated bounds for nonwhite volunteers do not exclude a zero effect. Thus, the evidence of military service effects on the incarceration and recidivism outcomes of nonwhite volunteers is more tenuous than for whites.

We perform statistical inference robust to multiple testing for the estimated military service effect for volunteers for the same reason we used those methods for the estimated direct effect for the draft avoiders in section V.A. After employing the three multiple testing methods across the five birth cohorts, we reject the null hypothesis of zero military service effects for white volunteers on their violent crime incarceration for the 1951- and 1952-born and for the nonviolent crime incarceration of the 1952-born. In contrast, we do not reject (at conventional significance levels) the null hypothesis of a zero military service effect on the 1952-born nonwhite volunteers' crime outcomes or the 1951-born white volunteers' nonviolent crime outcomes.

V.B.3 Additional Analysis on the Effect of Military Service on Volunteers

The previous results indicate that the estimated bounds for the 1948-1952 and the 1950 cohorts do not generally exclude zero while those for the 1951 and 1952 cohorts on the violent and nonviolent incarceration rates are predominantly positive and often exclude zero. In an effort to understand the factors that may lie behind the difference in these results, we use the inmate's data to compare several of the cohort's average characteristics in Table 8. 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 V.B.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 volunteers in the sample under A1-A3. The choice of characteristics to be compared is guided by the literature documenting channels through which military service affects crime outcomes: combat exposure (Rohlfs, 2010), drug use (Robins, 1973),

pre-service arrests and offending (Albæk et al., 2017), childhood physical abuse victimization and maltreatment (Khawand, 2009), and family background (Hjalmarsson and Lindquist, 2019).

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).[28] For each of these three violence exposure measures, the volunteers in the 1948-1950 cohort have higher averages relative to the volunteers 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 (for instance, Killgore et al. 2008; Rholfs, 2010; Sreenivasan et al. 2013). Nevertheless, this evidence is consistent with 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 (such as affirmation to patriotic beliefs, self-improvement, and solidarity with others) that are positively associated with a myriad of traumatic exposures (such as 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 (for instance, 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[29] effect of wartime combats 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 indeed possible that combat exposure can be related to lower incarceration rates for volunteers.

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[28] 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?".

[29] Posttraumatic growth is defined as positive psychological changes in response to trauma (Tedeschi and Calhoun, 1996).

The second set of characteristics compared in Table 8 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.[30] Table 8 shows that inmate volunteers 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 (for instance, Robins et al., 1975; Stanton, 1976), and the documented positive relationship between drug use and criminal offenses (for instance, Ellinswood, 1971; Tinklenberg, 1973), this evidence is consistent with the documented differential effects of military service on the incarceration rates of different cohorts of volunteers. 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 volunteers who had been using drugs before their military service. Furthermore, recent 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 8 is the involvement with the criminal justice system as a juvenile. The estimates indicate that inmate volunteers born in 1951-1952 were more likely to have had criminal justice contacts (arrests, probation, and incarceration) as juveniles relative to the earlier cohorts of 1948-1950, a difference only statistically significant for whites. One may conjecture that a criminal history prior to military service contributes to a larger crime instigation effect of the Vietnam era military service, which is consistent with the differential

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[30] 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?"

effects found for volunteers in different cohorts. This is also consistent with similar evidence reported in Hjalmarsson and Lindquist (2019) in the context of the mandatory military service in Sweden.

The last set of characteristics analyzed in Table 8 are three indicators of family background and socioeconomic status. The estimates suggest that the white inmate volunteers in the 1951-1952 cohorts 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 cohorts. For nonwhite inmates, volunteers 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 cohorts.[31] Therefore, inmate volunteers in the 1951-1952 birth cohorts, on average, tend to have worse family background and lower socioeconomic status relative to the 1948-1950 birth cohorts. This may be another reason why military service significantly increased the former cohorts' incarceration rates, given the literature documenting a strong negative correlation between socioeconomic status and incarcerations (for instance, Kearney et al., 2014) and the evidence in Hjalmarsson and Lindquist (2019) that military service has the most potent crime instigation effect among men with low socioeconomic status.

To summarize, the disadvantaged characteristics of volunteers from the 1951-1952 cohorts—with the possible exception of combat exposure, as previously discussed—appear consistent with the finding that the effect of military service on incarceration and recidivism outcomes is stronger for them 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.

V.B.4 The Effects of Military Service on the Outcomes of Veterans

We now present results on the crime instigation effects of military service on the population of Vietnam era veterans, that is, the treated population by military service—a population of first-order importance for policy. The veteran population consists of volunteers and compliers. To

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[31] 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)?".

provide context, based on the NHIS estimates of the proportions of volunteers and compliers, the volunteers account for about 78-84% of the population of veterans. As it was the case with volunteers, the results for the population of veterans are novel in the context of IV methods.

The results for white and nonwhite veterans are presented in Figure 6. Perhaps not surprisingly, since volunteers account for the majority of the Vietnam era veterans, the results mirror closely those already documented for the group of volunteers in section V.B.2. Namely, while for white and nonwhite veterans born in 1948-1952 and 1950 (Panel A to Panel D) the estimated bounds include zero (except in one instance), the majority of the estimated bounds for the 1951 and 1952 birth cohorts (Panel E to Panel H) exclude zero. Although the estimated bounds on the effects of the volunteers and veterans tend to be similar, the estimated bounds for the veterans tend to be wider and less precisely estimated. This stems from the fact that compliers consistently represent a smaller proportion of veterans relative to volunteers, and that the corresponding estimated bounds on the effects for compliers tend to be wide and centered around zero.[32] The estimated bounds for the 1951- and 1952-born veterans that statistically exclude zero and withstand multiple testing adjustments are for the military service effect on the violent incarceration of whites. First, the estimated lower bound on the military service effect on violent crime for white veterans born in 1951 indicates that it is at least 0.17 p.p. (Panel E), which represents 124% of the mean outcome of non-veterans. Second, the same effect for the 1952-born white veterans (Panel G) is estimated to be at least 0.25 p.p., a 128% increase relative to the mean outcome of non-veterans.

V.C Monetary Social Costs of Crime from the Draft Lotteries and Military Service

We provide simple estimates of the crime and incarceration consequences of the Vietnam military service in terms of societal monetary costs based on the previous results. To do this, we estimate 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 $403,915 (converted into 2019 U.S. dollars). For nonviolent crimes, we combine the estimates in McCollister et al. (2010) and the costs for drug violations in Delisi and

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[32] The estimated bounds for compliers are available in Online Appendix D. As previously mentioned, we do not discuss those results herein given that the estimated bounds are wide and imprecisely estimated, likely due to the relatively small size of this stratum.

Gatling (2003) to estimate an average per unit cost of $153,347 (in 2019 U.S. dollars). These costs include victim costs (such as medical expenses, cash losses, property theft or damage), criminal justice system costs, and crime career costs (that is, 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.[33] 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 and withstand multiple testing adjustments. Thus, the figures represent a conservatively estimated lower bound of societal monetary costs.

Our results show that Vietnam era military service significantly increased violent crime incarceration for white veterans born in 1951 and 1952, and significantly increased nonviolent crime incarceration for white volunteers. 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 1,274 violent offenses and 331 nonviolent offenses. The induced total tangible cost is at least about $565 million 10-20 years after the Vietnam era military service took place.[34] According to the Congressional Research Service (2010), the cost of the Vietnam war amounted to $111 billion ($843 billion in 2019 U.S. dollars). Our results suggest that this figure may be an underestimate given that the crime instigation effects of military service are not accounted for. Also, based on the current amount of resources that policymakers devote for corrective purposes, addressing early the crime instigation consequences of military service suggests cost-savings

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[33] Other studies (for instance, 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.

[34] To estimate the total number of offenses induced by military service, the total population of 1951- and 1952-born white veterans is, respectively, 326,933 and 288,935, while the total population of 1951-born white volunteers is 215,335. We then multiply the lower bound estimates of military service effect of the respective birth cohort by the corresponding population of volunteers or veterans, obtaining the increased number of offenses by race and birth year. Specifically, for 1951-born white veterans, their estimated ATT lower bound of military service on violent offenses is 0.0017203 times 326,933 = 562. For 1952-born white veterans their estimated ATT lower bound of military service on violent offenses is 0.0024635 times 288,935 = 712. For 1951-born white volunteers, the corresponding estimated $LATE_{at}$ lower bound of military service on nonviolent offenses is (0.0015379*215,335 = 331). Thus, the Vietnam War military service increased the violent and nonviolent offenses by at least 1274 and 331, 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, (1,274*$403,915)+(331*$153,347) = $565,345,567.

potential. For instance, in 2019, Congress allocated $23 million in funding for the 2020 fiscal year for Veterans Treatment Court programs (U.S. 116th Congress, 2019).

VI. Conclusion

We examined the effect of conscription and military service on incarceration and recidivism outcomes using the Vietnam era draft lotteries as a possibly invalid instrumental variable (IV) for military service. To do this, we employed recently developed nonparametric bounds that relax the so-called exclusion restriction assumption (ER) that requires the IV to have an impact on the outcome only through the treatment. The draft lotteries may violate the ER since some males subjected to them had an incentive to engage in adverse behaviors to avoid conscription (for instance, commit delinquencies or simply not comply) that can have an effect on future incarceration. The bounds we employ also allow conducting statistical inference on two important groups that conventional IV methods are silent about: the military service volunteers and the overall group of veterans (that is, the treated group). The overall group of veterans is of first-order importance for policy, while the group of volunteers may be informative about the current U.S. all-volunteer forces (AVF).

Our main findings are as follows. First, we find evidence that could be interpreted as suggestive of positive direct effects of the Vietnam era draft lotteries (net of the military service channel) on the violent crime incarceration and recidivism outcomes of draft avoiders, particularly among the 1950-born cohort. That the 1950-born cohort exhibits these effects is consistent with their short reaction time to the draft documented in the literature, which likely limited the range of other behavioral responses they had available (for instance, educational deferment). The estimated bounds, which rely on the same conventional assumptions of the conventional IV estimates, sans the ER, are positive and often exclude zero. However, the estimated bounds' confidence intervals on this effect—particularly those employing conservative multiple testing adjustments—do not exclude zero. For this reason, the evidence can also be interpreted as simply not providing statistical evidence that the direct effects of the draft lotteries are non-zero. Under the first interpretation, the evidence would point towards a violation of the ER in the current empirical context since the ER assumption is required to be satisfied by everyone in the sample.

Second, we find heterogeneous effects of the military service on the incarceration and recidivism rates of the different cohorts of volunteers exposed to the draft lotteries. We find no statistical evidence of effects of military service on violent or nonviolent incarceration outcomes

for males 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 era military service increased the violent incarceration rate of whites by at least 0.20 p.p. and 0.31 p.p., respectively, with the 95% confidence intervals on the estimated bounds excluding zero. Also, for the white volunteers born in 1952, military service increased the nonviolent incarceration rates by at least 0.15 p.p. A complementary analysis of average characteristics of volunteers from the different birth cohorts using our individual-level data on inmates suggests that the factors that appear to be significant contributors to the crime instigation effect of military service relate to drug use, pre-draft criminal justice contacts, low socioeconomic status, and adverse family background. If the Vietnam era military service had crime instigation effects on military service volunteers who had drug abuse and criminal history records prior to the service, as our results suggest, then an 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 the current post-service efforts.

The last set of results pertain to the effect of military service on the incarceration outcomes of the overall group of veterans (that is, the average treatment effect on the treated). The group of veterans consists of compliers and volunteers, with the latter group accounting for about 78-84 percent. Thus, it is not entirely surprising that the results for all veterans mirror those for volunteers, although the estimated bounds for veterans are wider and less precisely estimated. Particularly, the 1951- and 1952-born white veterans experience an effect of military service on their violent incarceration rate of at least 0.17 p.p. and 0.25 p.p., respectively. The results for volunteers and veterans are large, as they represent an increase of at least between 83 and 160 percent relative to the incarceration rates of non-veterans. Lastly, the results in this paper suggest that the long-term tangible social cost of the violent and nonviolent offenses caused by Vietnam era military service is at least in the order of $565 million in 2019 US dollars. Importantly, if the crime-instigation effect of military service is more potent among volunteers, the AVF system instituted after the Vietnam war could have heightened it, which could explain the level of funding for programs aimed at ameliorating the involvement of veterans with the judicial and incarceration systems.

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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	Draft avoiders & Compliers (*c*)	Draft avoiders & Defiers (*d*)
	1	Volunteers & Defiers (*d*)	Volunteers & Compliers (*c*)

Note: In the terminology of Imbens and Angrist (1994), draft avoiders are never takers (*nt*) and volunteers are always takers (*at*).

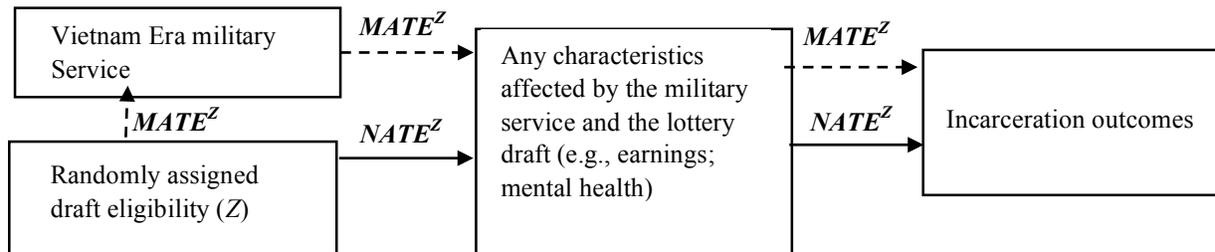**Figure 1. Illustration of the $MATE^Z$ and $NATE^Z$ of the Vietnam War Lottery Draft**

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

Mean characteristics	White	Nonwhite	Difference
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)
Violent crime offenders	0.5322***	0.5927***	-0.0605***
	(0.0108)	(0.0105)	(0.0150)
Recidivists:			
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)
Estimated Strata Proportions			
Draft avoiders	0.7298***	0.8068***	-0.0769***
	(0.0146)	(0.0129)	(0.0194)
Volunteers	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)
Total observations	2700	2619	

Note: Standard errors of the estimates are in parentheses; *, ** 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	3769
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 (0.0001)	0.0020 (0.0001)	0.0002 (0.0002)	0.0145 (0.0007)	0.0140 (0.0006)	0.0005 (0.0010)
Nonviolent Crime Incarceration	0.0017 (0.0001)	0.0018 (0.0001)	-0.0001 (0.0001)	0.0104 (0.0007)	0.0106 (0.0006)	-0.0001 (0.0009)
Violent Crime Recidivism	0.0010 (0.0001)	0.0008 (0.0001)	0.0001 (0.0001)	0.0077 (0.0005)	0.0069 (0.0004)	0.0007 (0.0007)
Nonviolent Crime Recidivism	0.0007 (0.0001)	0.0007 (0.0001)	-0.0000 (0.0001)	0.0062 (0.0005)	0.0057 (0.0004)	0.0005 (0.0007)
1979 Survey						
Violent Crime Incarceration	0.0019 (0.0001)	0.0017 (0.0001)	0.0002 (0.0002)	0.0156 (0.0010)	0.0172 (0.0010)	-0.0016 (0.0014)
Nonviolent Crime Incarceration	0.0014 (0.0001)	0.0015 (0.0001)	-0.0002 (0.0002)	0.0100 (0.0008)	0.0086 (0.0006)	0.0014 (0.0010)
Violent Crime Recidivism	0.0009 (0.0001)	0.0008 (0.0001)	0.0001 (0.0001)	0.0075 (0.0007)	0.0083 (0.0007)	-0.0008 (0.0010)
Nonviolent Crime Recidivism	0.0006 (0.0001)	0.0006 (0.0001)	-0.0000 (0.0001)	0.0042 (0.0005)	0.0039 (0.0004)	0.0003 (0.0007)
1986 Survey						
Violent Crime Incarceration	0.0022 (0.0002)	0.0020 (0.0002)	0.0002 (0.0002)	0.0157 (0.0012)	0.0170 (0.0010)	-0.0013 (0.0016)
Nonviolent Crime Incarceration	0.0014 (0.0001)	0.0015 (0.0001)	-0.0002 (0.0002)	0.0100 (0.0008)	0.0086 (0.0006)	0.0014 (0.0010)
Violent Crime Recidivism	0.0011 (0.0001)	0.0009 (0.0001)	0.0002 (0.0002)	0.0083 (0.0008)	0.0093 (0.0008)	-0.0010 (0.0012)
Nonviolent Crime Recidivism	0.0006 (0.0001)	0.0006 (0.0001)	-0.0000 (0.0001)	0.0042 (0.0005)	0.0039 (0.0004)	0.0003 (0.0007)
1991 Survey						
Violent Crime Incarceration	0.0026 (0.0001)	0.0022 (0.0001)	0.0003 (0.0003)	0.0152 (0.0014)	0.0123 (0.0010)	0.0029 (0.0017)
Nonviolent Crime Incarceration	0.0026 (0.0002)	0.0025 (0.0002)	0.0001 (0.0003)	0.0130 (0.0013)	0.0135 (0.0012)	-0.0005 (0.0018)
Violent Crime Recidivism	0.0010 (0.0002)	0.0009 (0.0001)	0.0001 (0.0002)	0.0084 (0.0009)	0.0057 (0.0007)	0.0027** (0.0012)
Nonviolent Crime Recidivism	0.0009 (0.0001)	0.0008 (0.0001)	0.0001 (0.0002)	0.0052 (0.0008)	0.0048 (0.0007)	0.0003 (0.0011)

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

Table 5. Estimated 1948-1952 Born Male Incarceration Rates

	$E[Y(1)|nt]$	$E[Y(0)|at]$	$E[Y|Z=0,D=0]$	$E[Y|Z=1,D=1]$	The upper bound of $E[Y(0)|nt]$	$LNATE_{nt}$
White						
Violent Crimes	0.0024 [0.0002]	0.0025 [0.0002]	0.0018 [0.0001]	0.0019 [0.0002]	0.0022 [0.0002]	(0.0002, 0.0024) [-0.0002, 0.0026]
Nonviolent Crimes	0.0020 [0.0002]	0.0016 [0.0002]	0.0018 [0.0001]	0.0011 [0.0001]	0.0022 [0.0001]	(-0.0002, 0.0020) [-0.0006, 0.0023]
Violent Recidivism	0.0012 [0.0000]	0.0009 [0.0001]	0.0008 [0.0001]	0.0006 [0.0001]	0.0010 [0.0001]	(0.0002, 0.0012) [-0.0000, 0.0014]
Nonviolent Recidivism	0.0009 [0.0001]	0.0005 [0.0001]	0.0008 [0.0001]	0.0004 [0.0001]	0.0010 [0.0001]	(-0.0001, 0.0009) [-0.0003, 0.0011]
Nonwhite						
Violent Crimes	0.0152 [0.0009]	0.0137 [0.0015]	0.0140 [0.0007]	0.0122 [0.0014]	0.0154 [0.0007]	(-0.0002, 0.0152) [-0.0022, 0.0167]
Nonviolent Crimes	0.0113 [0.0008]	0.0107 [0.0017]	0.0105 [0.0007]	0.0076 [0.0011]	0.0116 [0.0008]	(-0.0002, 0.0113) [-0.0022, 0.0128]
Violent Recidivism	0.0087 [0.0007]	0.0031 [0.0007]	0.0077 [0.0005]	0.0044 [0.0007]	0.0085 [0.0006]	(0.0003, 0.0088) [-0.0013, 0.0099]
Nonviolent Recidivism	0.0055 [0.0006]	0.0042 [0.0011]	0.0044 [0.0004]	0.0026 [0.0007]	0.0049 [0.0004]	(0.0006, 0.0055) [-0.0007, 0.0065]

Notes: Standard errors are in the brackets. The lower bound of E[Y(0)|nt] is 0 following the arguments explained in the text.

Figure 2. Trimming Bounds Illustration Using $E[Y(0, D_z)|nt]$

Y_i

The Upper Bound for $E[Y(0, D_z)|nt]$

Incarcerated Ineligible-to-draft Nonveterans: $Y_i|_{z=0,d=0}$ =1

Not Incarcerated Ineligible-to-draft Nonveterans: $Y_i|_{z=0,d=0}$ =0

1

0

1

Total ineligible-to-draft non-veteran population

The $\pi_{nt}/(\pi_{nt} + \pi_c)$% fraction of the largest values of Y

Y_i

The Lower Bound for $E[Y(0, D_z)|nt]$

Incarcerated Ineligible-to-draft Nonveterans: $Y_i|_{z=0,d=0}$ =1

Not Incarcerated Ineligible-to-draft Nonveterans: $Y_i|_{z=0,d=0}$ =0

1

0

1

Total ineligible-to-draft non-veteran population

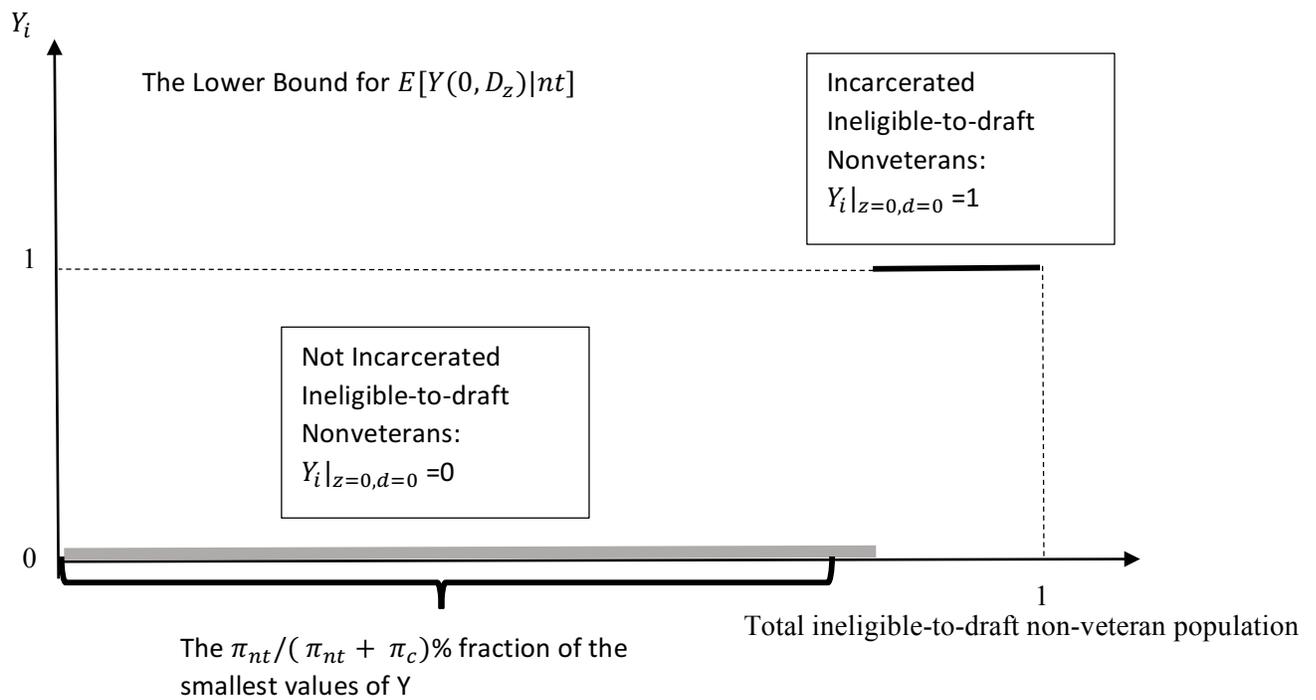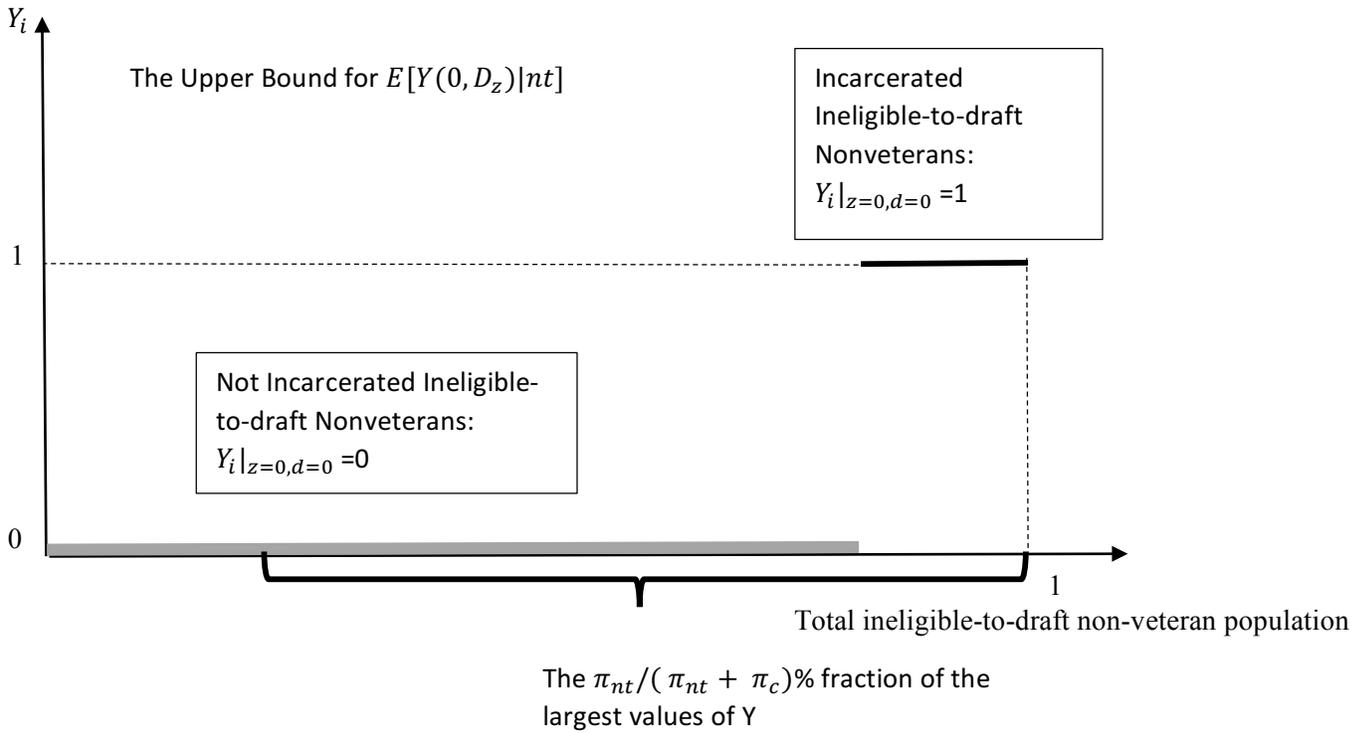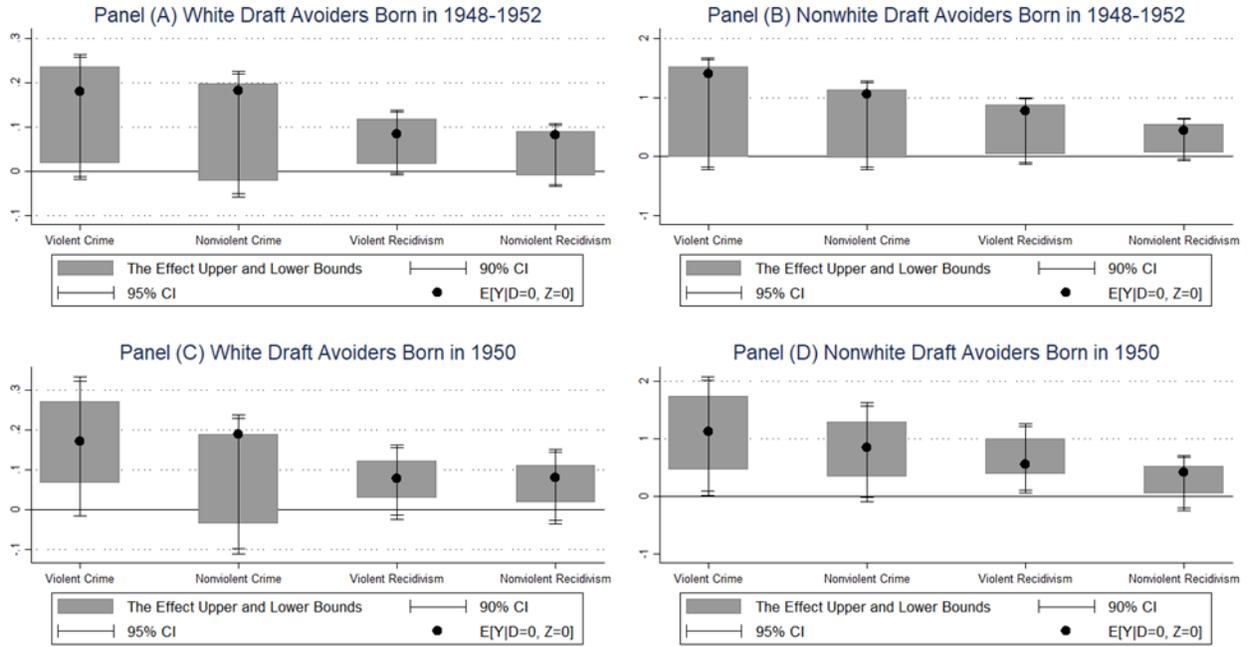The $\pi_{nt}/(\pi_{nt} + \pi_c)$% fraction of the smallest values of Y

Figure 3. Direct Effect of the Lottery Draft on Incarceration Outcomes of the Draft Avoiders

Note: The vertical axes in the figures are in the unit of percentage points.

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

|	(1) Draft Avoiders (nt)	(2) Draft Volunteers (at)	(3) Draft Volunteers and Compliers (*at & c*)	(4) Draft Avoiders and Compliers (*nt & c*)	(5) nt vs. at	(6) nt vs. at & c	(7) at vs. at & c	(8) nt vs. 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-Scaled 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 about Assumption A5 (under A1 – A3 plus A5)
(Males Born in 1948-1952)

| Crime Outcomes	White	Nonwhite
Violent Crime Incarceration	0.0005* (0.0003)	0.0030* (0.0017)
Nonviolent Crime Incarceration	0.0009*** (0.0002)	0.0037** (0.0014)
Violent Crime Recidivism	0.0006*** (0.0002)	0.0044*** (0.0010)
Nonviolent Crime Recidivism	0.0005*** (0.0001)	0.0029*** (0.0009)

Note: Under assumptions A1 to A3 plus A5, the construction of the bounds imply that $E[Y|Z = 1, D = 0] - E[Y|Z = 1, D = 1] \geq 0$. The figures in the table are estimates of that difference. Standard errors based on 2500 bootstrap replications are in parentheses; *, ** and *** indicate significance at 10%, 5% and 1% statistical significance levels, respectively.

Figure 4. Estimated Bounds for the Local Average Treatment Effect of Military Service on the Incarceration Rates of Volunteers Born in 1948-1952 and 1950
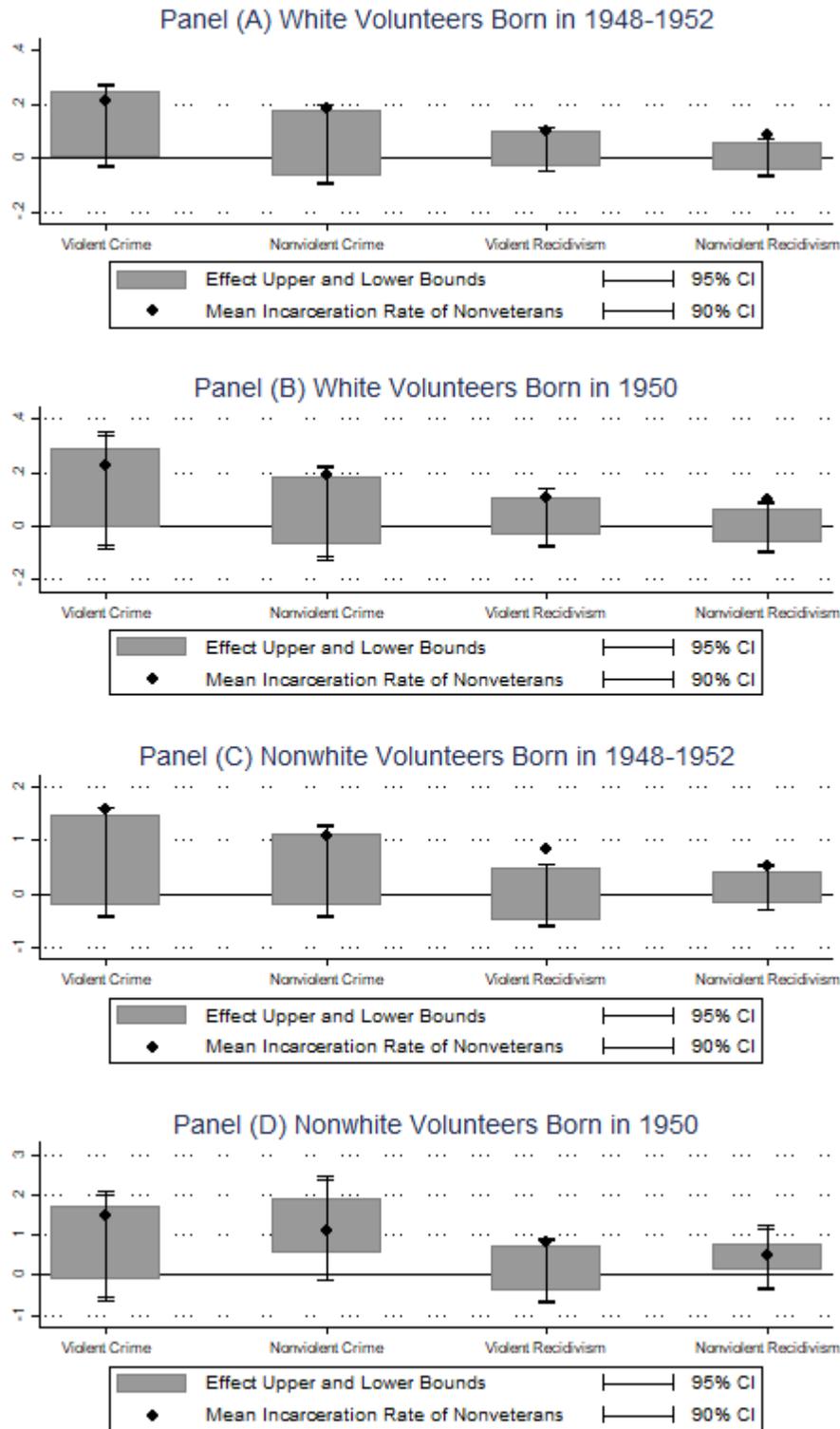
Note: The vertical axes in the figures are in the unit of percentage points.

Figure 5. Estimated Bounds for the Local Average Treatment Effect of Military Service on the Incarceration Rates of Volunteers Born in 1951 and 1952
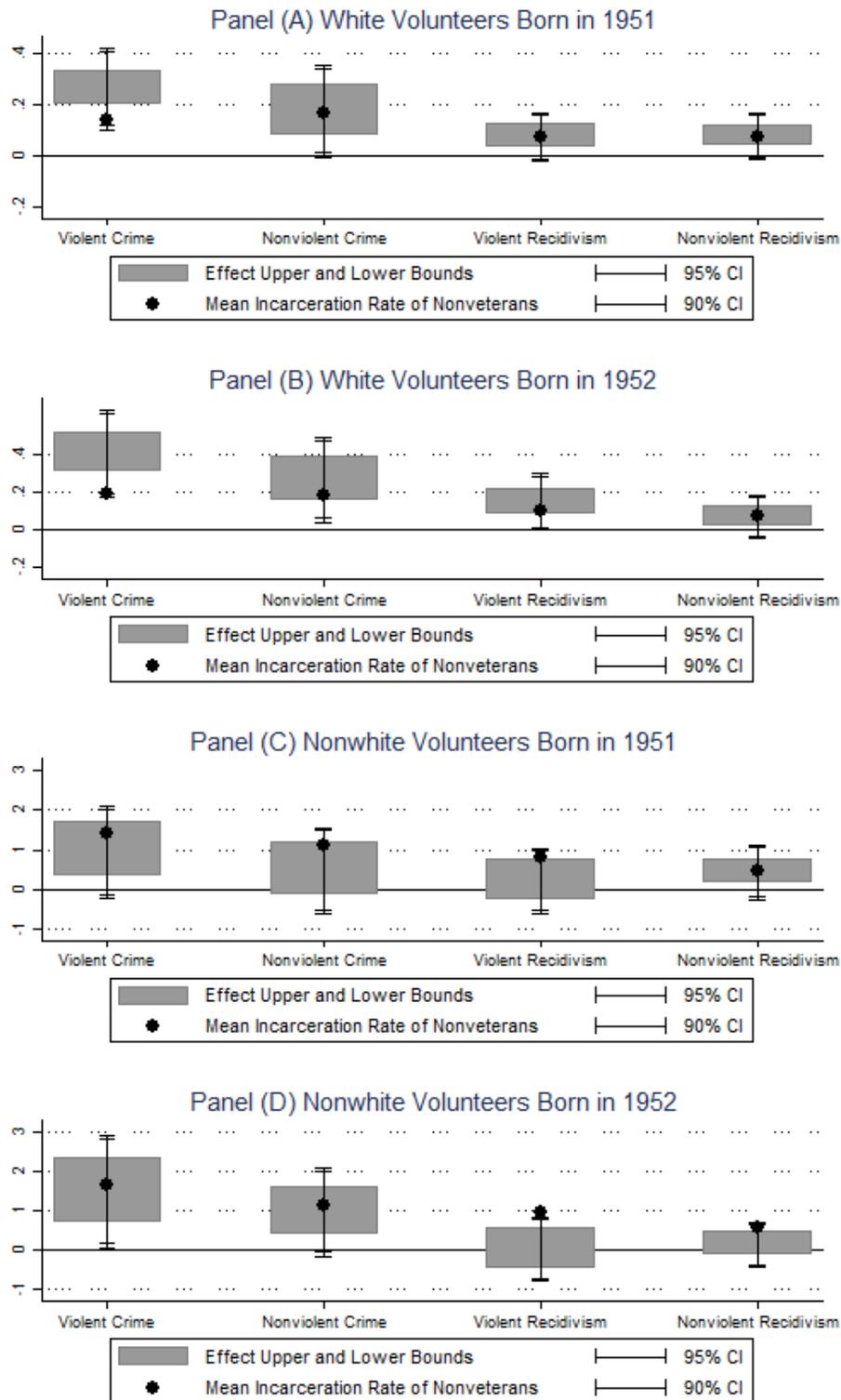
Note: The vertical axes in the figures are in the unit of percentage points.

Table 8. Characteristics of Volunteers Born in Different Years Using the Inmates Sample
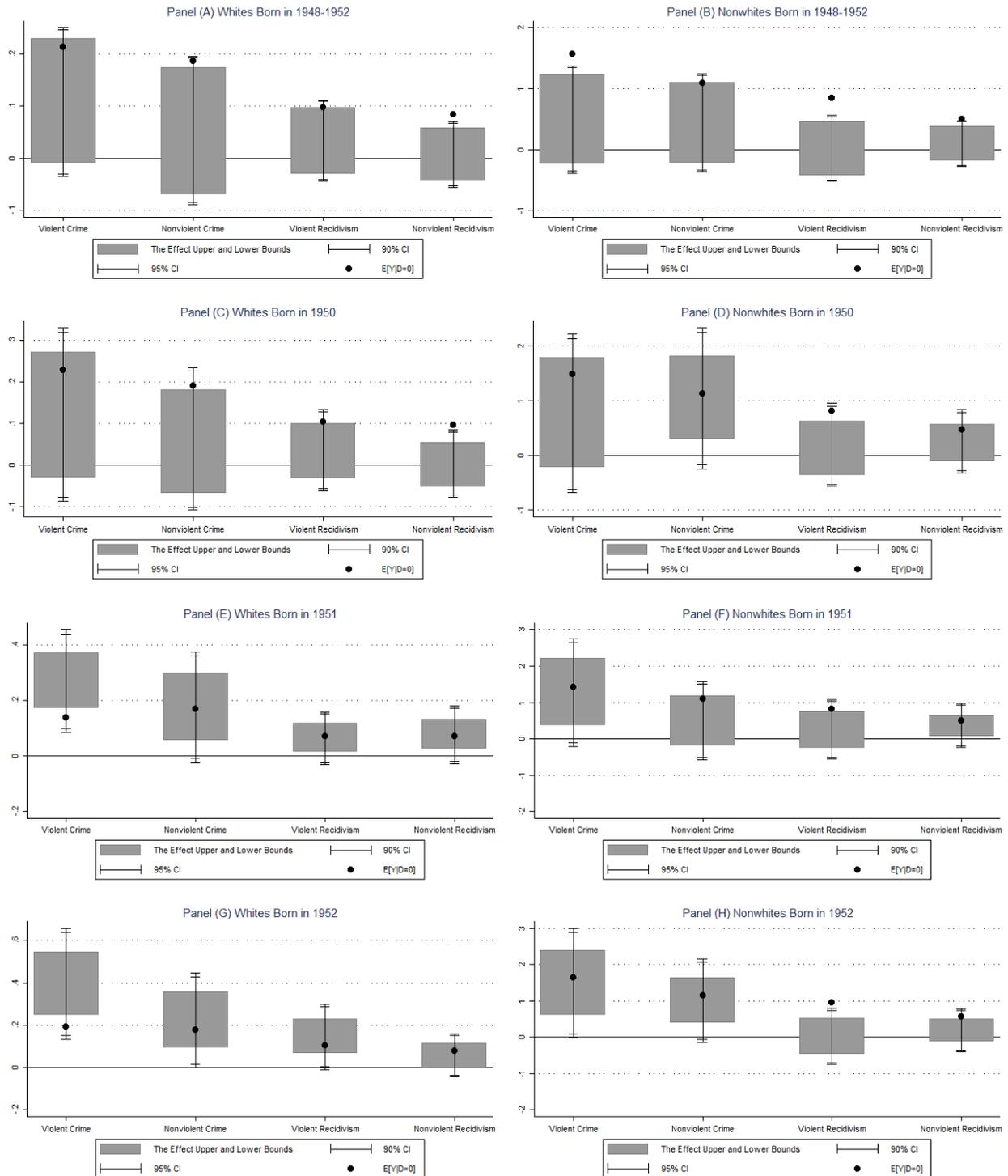
	White birth cohorts			Nonwhite birth cohorts		
	1948-1950	**1951-1952**	**Difference**	**1948-1950**	**1951-1952**	**Difference**
Always-taker proportion	0.1973 (0.01679)	0.2984 (0.0177)	-0.1012*** (0.0244)	0.1467 (0.0152)	0.1757 (0.0139)	-0.0290 (0.0206)
Military service related characteristics (Obs: 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[1]	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)[2]						
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.

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

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

Note: The vertical axes in the figures are in the unit of percentage points.