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Alan S. Gerber
Yale University, Professor
Department of Political Science
Institution for Social and Policy Studies
77 Prospect Street, PO Box 208209
New Haven, CT 06520-8209
alan.gerber@yale.edu

Gregory A. Huber
Yale University, Professor
Department of Political Science
Institution for Social and Policy Studies
77 Prospect Street, PO Box 208209
New Haven, CT 06520-8209
gregory.huber@yale.edu

Marc Meredith
University of Pennsylvania, Associate Professor
Department of Political Science
Stiteler Hall, Room 238
Philadelphia, PA 19104-6215
marcmere@sas.upenn.edu

Daniel R. Biggers
Yale University, Postdoctoral Associate
Institution for Social and Policy Studies
77 Prospect Street, PO Box 208209
New Haven, CT 06520-8209
daniel.biggers@yale.edu

David J. Hendry
Aarhus University, Assistant Professor
Department of Political Science and Government
Bartholins Allé 7, DK-8000 Aarhus C, Denmark
david.hendry@ps.au.dk

Does Incarceration Reduce Voting? Evidence about the Political Consequences of Spending Time in Prison from Pennsylvania and Connecticut

The rise in mass incarceration provides a growing impetus to understand the effect that interactions with the criminal justice system have on political participation. Although some states impose restrictions on voting by former felons, a large number of released prisoners who are legally eligible to vote nevertheless participate at very low rates. We use administrative data on voting and interactions with the criminal justice system from Pennsylvania and Connecticut to examine whether the association between incarceration and reduced voting is causal. Several strategies are employed to investigate the possibility that the observed strong negative correlation between incarceration and voting might result from differences across individuals that both lead to incarceration and low participation. We find that as this selection bias issue is addressed, the estimated effect of serving time in prison on voting falls dramatically and for some research designs vanishes entirely.

The massive expansion of the American criminal justice system in the last several decades has generated enormous concerns about the political consequences of the "carceral state."[1] While some work has considered the effects that law enforcement organizations and the "prison industry" have had on shaping enacted laws and policing practices, an important line of scholarship has focused on the effect of mass incarceration on those who are incarcerated. Among the potential political consequences of incarceration, evidence has been adduced to support the claim that spending time in prison significantly reduces political participation. If this claim is correct, there are important implications. In our democratic system of government, the fact that formerly incarcerated individuals fail to vote limits the incentives that politicians have to pay attention to the views of those likely to be most concerned about mass incarceration. Thus, if incarceration does reduce participation, it may increase the relative influence of those actors with a stake in further expanding the scope of the criminal justice system.

There are two primary mechanisms by which spending time in prison might reduce political participation: through the effect of laws curtailing voting rights and through the effect of spending time in prison on attitudes and human and social capital. Although the political consequences of laws directly prohibiting released felons from voting is a topic of frequent research, much less attention has been given to the broader question of whether the experience of incarceration itself disrupts patterns of post-release

[1] The "carceral state" refers to the totality of the "surveillance- and punishment-oriented system of governance" (Weaver and Lerman 2010, 818) that encompasses not only jails and prisons but also the extensive range of other forms of penal punishments and state control (see Gottschalk 2006).

participation when there are no legal prohibitions on voting by released convicts. In 2010, 15 million formerly incarcerated individuals in the United States were eligible to vote (Shannon et al. 2011), which is 6 times larger than the roughly 2.6 million formerly supervised individuals who were legally prevented from voting (Uggen et al. 2012, 16). Because so many eligible voters have been incarcerated, if incarceration reduces participation it could have a larger effect on electoral outcomes than legal restriction on felons voting rights. This concern is underscored by the fact that a number of studies show that eligible voters who have been released from prison vote at much lower rates than those who have not served time in prison (Haselswerdt 2009; Hjalmarsson and Lopez 2010; Lerman and Weaver 2014a; Weaver and Lerman 2010). The purpose of this paper is to investigate the observed relationship between incarceration and low levels of post-release voting. We seek to address the key question of whether, and to what extent, incarceration causes a reduction in individual-level political participation.

Knowing how incarceration affects voting is crucial to understanding "how policies make citizens," a core construct in both the policy feedback and political behavior literatures. Pioneering work by Weaver and Lerman (2010) and Lerman and Weaver (2014a) theorize that contact with the criminal justice system (which includes interactions ranging from police stops to prison) will decrease political participation by depleting citizens' resources, making them distrustful of government, and reducing commitments to civic norms. Because incarceration is the most severe form of criminal justice contact, it should depress participation more than other forms of criminal justice contact. Assessing the consequences of incarceration for participation is also important for policy makers considering whether aggressive crime control efforts generally, and

incarceration in particular, are superior to other efforts to deter, punish, and reform criminal offenders.

Because we know of no large scale domestic trial in which individuals have been sent to prison at random, it is difficult to ascertain whether the negative relationship between serving time and political participation reported in previous work is *causal*.[2] To make progress in answering this question, we gather and analyze novel data from two states: Pennsylvania and Connecticut. In contrast to most previous work, we rely on administrative records of both interactions with the criminal justice system and over time political participation to assess the effect of incarceration on subsequent political participation. These administrative records provide us with a relatively large sample of cases in which an individual served time in prison between the 2008 and 2012 presidential elections, but were legally eligible to vote in both elections. An additional benefit to using these administrative records is that it reduces concerns about measurement error in studies that use self-reported participation and interactions with the criminal justice system.

Perhaps the most difficult challenge for measuring the effect of incarceration on voting is that those who spend time in prison may be different from those who are not incarcerated in myriad ways that are also likely correlated with political participation. Put plainly, released prisoners might not vote, but many of those individuals who are

[2] There have been small scale randomized experiments of assignment to prison or a non-prison alternative for certain classes of offenders (typically first time or juvenile offenders). Examples of this research include Sherman and Berk (1984) and Sherman et al. (1992). None of these studies examine the political effects of the assigned punishment.

convicted of crimes resulting in prison sentences might not have voted even if they had never gone to prison. If this heterogeneity explains both who serves time in prison and who votes, and it is not fully accounted for in the research design, then the observed negative effect of incarceration on voting may be in part a consequence of selection bias. For this reason, our first set of analyses uses corrections records from both states to measure differences in pre-incarceration voting and post-release voting for those who first served time in prison between 2008 and 2012. We find that these prisoners did not vote very frequently in 2012, after going to prison, or in 2008, before going to prison, and that their turnout rate was only slightly lower in 2012 than in 2008.

Next, we use these data to compare the 2012 voting of those 2008 registrants who spend time in prison to the 2012 voting of all other 2008 registrants. Consistent with previous research, those who spend time in prison vote at a much lower rate in 2012 than those who do not. A naïve regression of voting on having spent time in prison produces a very large (negative) estimated effect of incarceration on the likelihood of voting. These estimates decline substantially, however, as we take steps to make the two groups (those who spend time in prison and those who do not) more similar through the inclusion of observable covariates.

Of course, conditioning on observable control variables does not control for one of the key differences between those who spend in time in prison and those who do not, which is the fact that all individuals who are sentenced to prison have been found guilty of a crime. It is for this reason that our third set of analyses takes advantage of a unique set of administrative sentencing data available only in Pennsylvania to compare those who spend time in prison to similar convicts who are given a sentence other than time in

prison. That is, the sample for this comparison consists entirely of individuals who have been convicted of a crime, but who differ in whether or not the sentence they receive includes prison. Additionally, because the sentencing data also include a rich set of measures about past criminal behavior and the severity of a criminal act, this analysis allows us to compare individuals who appear very similar apart from which sentence they are given.

In this design, which arguably does the most thorough job of accounting for the selection effects that could produce a negative bias in the estimated effects of prison on voting, we estimate statistically insignificant and substantively unimportant effects of incarceration on voting. In contrast to the large negative effects in analyses that are most vulnerable to selection effects, in the Pennsylvania sentencing data it appears that going to prison is associated with, at most, a 0.3 percentage point decrease in the rate of voting in 2012, although some of our estimates are actually positive. We also use these data to compare individuals who are sentenced to spend time in jail to those given less severe non-incarcerative sentences, and again find little difference in their rates of participation.

Overall, the research designs we implement show that as we take additional steps to account for many of the potential differences between those who spend time in prison and those who do not, our large negative estimates of the effect of going to prison on voting (among previously registered individuals) decline substantially. In the research design where we minimize the potential differences between those sentenced to prison and those who are not, our estimates of the effect of incarceration on voting shrink to near 0. Our analysis should not be taken to minimize the potentially traumatic effects of serving time in prison, a subject that is well beyond the scope of our analysis. Rather, we

merely find that, although prison surely has many negative effects for individuals who spend time there, it does not appear to significantly reduce political participation.

There are also two broader implications of this work for political science and the social sciences more generally. First, while a great deal of literature has sought to understand the effect of policies on political attitudes and behaviors, one persistent challenge has been that those who are policy beneficiaries (or targets) are different from those who are not "treated" by a given intervention. For example, welfare recipients are different both from the non-poor and poor individuals who are ineligible for these benefits. The pattern we present in this work, where initially large treatment effect estimates decline as we take additional steps to make those who are treated more similar to those who are not, offers a cautionary warning for empirical researchers more generally. If, as we discuss more fully below, patterns of estimated effects decline with more similar comparisons, scholars should exercise extreme caution in assuming that initial statistical analysis provides estimates of the correct magnitude. More technically, this pattern implies that the assumption that assignment to treatment is ignorable (random) given typical observable factors is highly suspect.

Second, for scholars interested in the effects of living in a country where a huge share of our population spends time in prison, our results hint that a focus on the negative effects of incarceration may miss many of the more subtle and yet important ways in which the criminal justice apparatus affects participation. Incarceration is an outcome that often follows a long series of interactions with various criminal justice system institutions. More work is needed to understand how these earlier interactions shape political participation. The prior literature's general focus on incarceration may also be

missing the far more expansive and somewhat subtle ways in which the last three decades of aggressive crime control have affected entire communities (in this vein, see Burch 2013, Lerman and Weaver 2014a).[3] Lerman and Weaver argue, for example, that the widespread presence of the police and aggressive street patrol efforts send signals to citizens in these heavily policed neighborhoods about expectations for appropriate behavior, which may include keeping quiet and not questioning authority. This may mean that criminal justice contact affects entire communities that are the targets of aggressive crime control efforts. Of course, our empirical approach shows that great care must be undertaken when evaluating the effects of these community-level interventions, because it is clear that the communities that are most affected by aggressive crime control efforts are different in significant ways from those that are not similarly targeted.

The Criminal Justice System and Political Participation

Formerly incarcerated individuals participate at low rates once they are eligible to vote. What explains this pattern? While multiple explanations have been advanced in the literature, the one that has received the most attention in political science is that contact with the criminal justice system and incarceration may demobilize individuals. There are a variety of mechanisms by which time in prison might reduce political involvement, some of which apply not just to incarceration but also to contact with the criminal justice

[3] A contrary argument is that those crime control efforts have created safer neighborhoods, which might in turn encourage civic activity and greater voting (see, for example, Wilson and Kelling 1982).

system more generally.[4]

For example, the criminal justice system may be the primary means by which citizens encounter the state in many at-risk communities (Weaver and Lerman 2010), and such interactions may shape their attitudes toward political participation. Lerman and Weaver (2014b) describe how citizens learn that they have less standing in the social and political community through this contact with the "carceral" state. Criminal convictions also reduce labor-force stability (Western 2002), which may depress subsequent turnout. Other mechanisms apply more specifically to individuals who are incarcerated. Lerman (2013) argues that spending time in prison has particularly negative consequences on social capital, which is an important determinant of political participation. Other factors that are positively associated with the propensity to vote, such as marriage and residential stability, are also negatively affected by incarceration (Fleisher and Decker 2001). Burch (2013) argues that the demobilizing effect of incarceration is sufficiently strong to explain part of the depressed rates of participation in communities with large formerly imprisoned populations.

In contrast to this account, which posits a causal effect of the criminal justice

[4] Confusion about voting rights may also reduce participation among the previously incarcerated population. States differ substantially in when and how formerly incarcerated individuals regain the right to vote. One consequence of this wide variation is that formerly incarcerated individuals who are eligible may nonetheless incorrectly believe that they are disenfranchised (Meredith and Morse 2014a). Supporting this account, Gerber et al. (forthcoming) show that outreach to eligible released felons can increase their registration and voting rates.

system on individual behavior, a second literature argues that participation among formerly incarcerated individuals is lower than that of comparable populations because those who end up in prison are different from those who do not. Uggen et al. (2006, 295) summarize these differences between prisoners and non-prisoners: “Compared to the nonincarcerated population, prisoners have long been undereducated, underemployed, relatively poor, and disproportionately nonwhite.” Thus, the observed correlation between incarceration and low voting rates may not measure the causal effect of incarceration, but instead selection. Selection in this context refers to the (unobservable) differences between the formerly incarcerated population and the general population that both exist prior to incarceration and that also affect participation. Most specifically, former convicts may participate less than otherwise similar looking members of the general public not because they have been convicted of a crime or sent to prison, but because the same choices and circumstances that eventually lead to a prison sentence also explain reduced participation (Miles 2004).

Empirically distinguishing the effect on political participation of criminal justice system contact from the effect of all the other variables that are associated with criminal justice system contact is difficult. For example, both political science and criminology focus on the importance of parental socialization for developing prosocial norms and other traits that may reduce criminal behavior and increase voter turnout (Jennings and Markus 1984; Smith and Farrington 2004). Similarly, a host of familial factors (Farrington 1998; Roettger and Swisher 2009) and tendencies toward antisocial behavior (Farrington 1998; Wilderman 2010) are correlated with the propensity for future criminal behavior. Together, these factors offer an alternative explanation for why some

individuals both go to prison and do not vote (Gottfredson and Hirschi 1990). Because many of these individual traits and attitudes are difficult or expensive to measure, they are often either missing or measured with error in the standard datasets used to study the correlates of political participation. This makes it hard to rule out the possibility that omitted variables are the source of the correlation between reduced political participation and criminal justice contact.[5]

Prior Empirical Research

Table 1 summarizes the previous literature about how contact with the criminal justice system affects turnout. These studies consider a range of different interactions with the criminal justice system, but we focus our attention on those studies that examine the relationship between serving time in prison and voting. This is because incarceration is the sanction that prior work has found has the largest negative effect on voting. As the table makes clear, there is a relatively persistent pattern in these data: Those who experience state supervision are less likely to vote than those who do not.

In Panel A of the table, we list the three studies that specifically examine the relationship between spending time in prison and voting. Hjalmarsson and Lopez (2010),

[5] Indeed, some scholars have argued that punishing individuals who commit criminal acts is necessary to prevent recidivism. If ceasing to commit crimes means instead respecting community norms about appropriate behavior, punishment may facilitate community reintegration which may in turn be associated with greater participation. For example, Sherman and Berk (1984) show in a field experiment that the mandatory arrest of alleged domestic violence perpetrators reduces recidivism more than not arresting the alleged attacker. That study did not examine the effect of the intervention on voting.

Weaver and Lerman (2010), and Lerman and Weaver (2014a) each show that people who report prior incarceration are noticeably less likely to report post-release participation. Specifically, compared to those who have not experienced criminal justice contact, those who are sent to prison are between 11 and 52 percentage points less likely to vote.

Panel B of the table shows that these and other studies also find smaller but still significant decreases in turnout associated with other forms of reported interactions with the criminal justice system. Compared to individuals claiming no contact with the criminal justice system, those who report being arrested, but not convicted, are less likely to vote by between six and sixteen points, while those who are convicted of a crime, but not imprisoned, are less likely to participate by between ten and eighteen points.

Cumulatively, these results demonstrate that people who admit contact with the criminal justice system report voting less than observably similar people who do not. These studies therefore describe a robust correlation in the data. What is less clear from these studies, however, is whether coming into contact with the criminal justice system *causes* people to vote less, or whether these turnout differences reflect bias due to selection or measurement error.[6] We discuss each of these threats to interpreting the

[6] An additional concern with prior scholarship relates to sampling variability induced by small sample sizes. Because incarceration is an infrequent event, even relatively large samples of nationally representative populations typically include only a small number of formerly incarcerated people. Lerman and Weaver (2014a), for example, must rely on the 57 people who report their first incarceration between two elections. The numbers are larger in the cross-sectional studies of Hjalmarsson and Lopez (2010) and Weaver and

Table 1: Prior Research on the Effect of Interactions with the Criminal Justice System on Voting

Panel A: Studies Examining the Effect of Incarceration (Relative to No Criminal Justice Contact) on Voting:				
Study	**Data**	**Cross-Sectional or Panel?**	**Estimated Effect on Turnout (In Percentage Points)**	**Effective N (Treated Cases)**
Hjalmarsson and Lopez (2010)	Add Health	Cross-Sectional	-11	61
	NLSY97	Cross-Sectional	-13	359
Weaver and Lerman (2010)	Add Health	Cross-Sectional	-16 to -29	156
	Fragile Families	Cross-Sectional	-22 to -26	723
Lerman and Weaver (2014a)	NLSY97	Panel	-52	57
Panel B: Studies Examining Effects of Other Interactions on Voting:				
Study	**Data**	**Cross-Sectional or Panel?**	**Estimated Effect on Turnout (In Percentage Points)**	**Effective N (Treated Cases)**
Effect of Arrest without Conviction (Relative to No Criminal Justice Contact):				
Uggen and Manza (2002)	St. Paul Youth Development Survey	Cross-Sectional	-6	127
Hjalmarsson and Lopez (2010)	Add Health	Cross-Sectional	-7	514
	NLSY97	Cross-Sectional	-7	1795
Weaver and Lerman (2010)	Add Health	Cross-Sectional	-7	578
	Fragile Families	Cross-Sectional	-16	356
Lerman and Weaver (2014a)	NLSY97	Panel	-13	191
Effect of Conviction without Imprisonment (Relative to No Criminal Justice Contact):				
Weaver and Lerman (2010)	Add Health	Cross-Sectional	-10	582
	Fraglie Families	Cross-Sectional	-18	114
Effect of Conviction (Relative to Future Conviction):				
Weaver and Lerman (2010)	Add Health	Cross-Sectional	-5	607
Burch (2011)	FL Administrative Data	Cross-Sectional	no effect	3099
	GA Administrative Data	Cross-Sectional	+3	10773
	MI Administrative Data	Cross-Sectional	-1	8841
	MO Administrative Data	Cross-Sectional	+9	4332
	NC Administrative Data	Cross-Sectional	+6	24403

Note: Effective N is number of cases experiencing treatment (e.g., number of people incarcerated).

studies listed in Table 1 as providing causal estimates of the effect of incarceration on participation.

Selection

The challenge of addressing selection problems in this study is that those who are incarcerated are on average different from those individuals who are not convicted and not incarcerated in both observed and unobserved ways. If any of the myriad factors noted above that explain the risk of incarceration and affect participation are not accounted for, then any estimate of the effect of incarceration will be biased. Although all of the designs listed in Table 1 account for many demographic and other covariates that may explain future criminality, it is not clear that they control for *all* pertinent influences. It is for this reason that a key and very difficult problem of research design is finding a counter-factual comparison group whose behavior can be compared to those sentenced to prison.

Of the studies listed in Table 1 (including those that address other types of contact with the criminal justice system), all but one (Lerman and Weaver 2014a) are cross-sectional, meaning they include observations for an individual at only a single point in time. Cross-sectional designs are particularly vulnerable to selection concerns because without measurement of prior participation any unmeasured factor that is correlated with both reduced participation and the risk of incarceration will yield a biased estimate of the effect of incarceration on voting. In other words, one cannot separate the effects of incarceration from pre-incarceration differences in political participation.

Lerman (2010), but at most the surveys used in these studies provide 723 citizens who report prior incarceration.

Panel studies, by contrast, use a combination of past measures of behavior (i.e., prior turnout) and measured covariates to account for *static* differences between those who are incarcerated and those who are not. This is the strategy used in the single panel study to date (Lerman and Weaver 2014a). However, even in a panel setting, estimates will be biased if there are *changes* in any unobserved factor that explains both decreased participation and increased chances of incarceration. For example, if those who later end up in prison "fall in with a bad crowd" and thereby simultaneously deviate from their prior levels of participation and become more likely to be convicted of a crime, the apparent effect of incarceration could still be due to changes in individual-level factors rather than the effect of incarceration.

Burch (2011) examines the effect of conviction (rather than incarceration) on participation and provides evidence that highlights selection concerns. She compares the political participation of a population that will experience a treatment (will be convicted in the future) with a different population that has already experienced that treatment (those who have been convicted and released). Unlike the other studies listed in Table 1 that use survey data, her study matches administrative records of people who have been convicted of a felony to 2008 turnout in five states. These administrative data eliminate concerns about measurement error in reports of both treatment and participation and provide many more cases where conviction is observed. In contrast to what one would expect to find if contact with the criminal justice system reduced turnout, Burch shows that in 3 of the 5 states included in her study, people who were convicted *after* the 2008 election were significantly *less* likely to vote in 2008 than people who were convicted

before the election.[7] These two groups, however, are not fully comparable because only a subset of released felons are eligible to participate in these states and because there may be differences in who ends up in prison over time.[8]

Measurement Error

A second concern with prior scholarship is the reliance on self-reported measures of turnout, contact with the criminal justice system, and other factors thought to affect both outcomes. As Table 1 makes clear, only one study (Burch 2011) uses administrative records to measure both contact with the criminal justice system and participation. There are a number of limitations with using self-reported measures. For example, comparing

[7] A robustness check reported by Weaver and Lerman (2010) presents a similar pattern. Specifically, in one analysis (see Figure S2 in the supplemental appendix) they restrict their attention to a set of individuals who have reported or will in the future report being convicted of a crime, therefore holding constant all static factors that explain the chances of at some point reporting a conviction. They then compare the reported participation of these convicts to those who will later become convicts. Turnout rates are statistically indistinguishable between these two groups after controlling for differences in their observable characteristics.

[8] Burch also shows that people who were incarcerated were significantly less likely to vote than people who only received probation in four of the five states included in the study. But the sparse set of control variables available—race, age, and sometimes education—makes it impossible to account for important differences in the types of people who receive incarceration and probation, including differences in the crimes they commit.

self-reported voter turnout across groups requires a strong assumption about the relative frequency with which people in different groups over-report voting (Bernstein et al. 2001). If there is measurement error in the reporting of the key variables—for example, if people who are arrested understate their prior level of criminal activity or overstate their prior level of participation, then estimates relying on those survey measures as treatments, outcomes, or control variables will be biased.

Vavreck (2007) shows that more civically engaged individuals are more likely to misreport voting when they did not in fact participate. If civic engagement is also correlated with a reduced likelihood of being incarcerated, it may appear that individuals who are incarcerated are less likely to vote, but that could be an artifact of misrepresentations of participation. Thus, measurement error is a threat to inference even in a model that accounts for the complete set of factors that jointly explain actual participation and risks of incarceration.

More narrowly, in the area of criminal justice research, a large literature explores the validity of self-reported measures of criminal involvement relative to official sources, like arrest and court records (see Thornberry and Krohn 2000, pp 52-57 for an overview). Official records are generally preferred to self-reports when studying the consequences of criminal convictions. In part, this is because a small, but not trivial, percentage of people will fail to report their own arrest or incarceration (Maxfield et al. 2000). Morris and Slocum (2010) find that people are even worse at reporting the timing of these events. Thus, survey data may not be effective for measuring prior criminal activity and may also be particularly ill-suited for studying the effects of incarceration that occur over a specific time period.

Research Design

In light of the concerns about selection and measurement error discussed in the previous section, we use three different research designs to estimate how incarceration affects political participation. In all three approaches we use administrative records of interactions with the criminal justice system and participation to reduce concerns about measurement error.

Approach One: Comparing Voting and Registration Rates Before and After Experiencing Incarceration

Our first approach examines the change over time in registration and voting for individuals who spend time in prison between two elections. We therefore compare formerly incarcerated individuals to themselves before they experienced incarceration. An advantage of this approach is that, by comparing the same individual at two points in time, we know that any change in voting behavior could not have been caused by any of the static individual-level factors that explain persistent patterns of participation in cross-sectional analyses. However, if individuals change in ways that both increase their chances of being incarcerated and reduce their propensity to participate in the future, this analysis will not account for these differences. Additionally, this analysis does not account for other factors that change over time, like electoral context and demographics changes (e.g., becoming older), that may also affect participation in each election.

Approach Two: Comparing 2012 Voting of 2008 Registrants by Incarceration Status

If those who spend time in prison are thought of as "treated" and those who do not are considered "untreated," approach two adds an untreated "control group" to the

previous analysis. To account for differences in electoral context and the effect of changing demographics on expected rates of participation, our second analysis compares the 2012 voting of 2008 registrants, some of whom spend time in prison between these elections. While this comparison is similar to prior work, we note two limitations of this empirical strategy. First, it excludes the large proportion of incarcerated individuals who were not registered in 2008 (before going to prison). We exclude these individuals because we do not have information about comparable non-registered individuals who are not incarcerated between these elections. Second, we observe only the relatively sparse set of controls that are contained in the voter file (including 2008 participation) and so it is unlikely that we can fully account for all of the factors besides incarceration that may make the formerly incarcerated population vote less than the general population of 2008 registrants. The advantage of our second approach is that we can measure the effect of changing context and aging by following the individuals who do not spend time in prison, and then (under the assumption that the effect of these factors is similar for the "treated" and "untreated" individuals) we can use this information to eliminate the influence of these factors that change over time on our estimates of the effect of prison on voting. The selection bias problems remain unaddressed; if individuals who are later incarcerated change in ways that both cause their incarceration and reduce their expected rates of participation, the estimated effects of prison on voting are larger than the true causal effect.

Approach Three: Comparing 2012 Voting of Convicts Given Different Sentences

In light of the remaining concerns about selection bias, our third approach is to compare the 2012 participation of convicts who are given different sentences. This

analysis, like our first approach, therefore holds constant the fact that everyone in this sample is convicted of a crime. That is, everyone in the sample has been convicted, but only some of those who are convicted are "treated" (serve time in prison). As a result, the approach minimizes concerns about selection bias that arise when comparing convicts to other citizens. Although we still must take steps to ensure that convicts sent to prison are comparable to those given different sentences, this is a more conservative approach than assuming convicts are like ordinary citizens. Additionally, like our second approach, only some individuals in this sample are sent to prison. This allows us to account for the effects of electoral context and changing demographics on voting using the subset of convicts given different sentences.

Overall, we believe our third approach is most likely to provide an unbiased estimate of the effect of incarceration on voting. However, it may still be biased if there are unobserved factors that explain why otherwise similar convicts are given prison sentences and also less likely to vote. (For example, if criminals who do not display remorse are more likely to be sentenced to prison and also less likely to vote, these estimates will be too large.)

Data

In this section we describe the basic features of the Pennsylvania and Connecticut data sets and in the next section we present our results.

Pennsylvania

We gathered Pennsylvania administrative records about individuals who came into contact with the criminal justice system. We then merged these records of sentencing

and time spent in prison to information about registration and voting from voter records. In addition to the quality of its data, Pennsylvania is also an attractive study location for theoretical reasons. Our primary goal is to isolate the causal effect of incarceration on voter turnout. To do this, we must account for other factors that might lead people who had been incarcerated to vote at low rates apart from the experience of having been in prison. One possibility (see footnote 4) is that released convicts might not vote because they believe they are legally disenfranchised. For this reason, Pennsylvania is relatively promising. In Pennsylvania the rules for voting by those who are convicted of a felony are straightforward; individuals convicted of a felony lose the right to vote only while incarcerated. It is also a presidential battleground state where campaigns are active in both registering and mobilizing potential voters. We believe that this pressure to register potential voters is likely to increase the chances that released convicts are made aware of their right to participate.

Pennsylvania Sentencing Data

We obtained records from the Pennsylvania Sentencing Commission (PSC) for all individuals convicted of a crime in state court and sentenced between Election Day 2008 and December 31, 2010 (the most recent available data). These data were processed and cleaned to identify the first date of sentencing for each individual sentenced between these two dates. The PSC data include information about the name, date of birth, gender, race, county of residence, and prior criminal record for each person convicted of a crime in state court. Additionally, they include information about the offense an individual committed, including whether it was a felony or drug crime, its severity (scored using an "Offense Gravity Score" [OGS]), and a recommended sentence under Pennsylvania's

structured sentencing guidelines. Because we also wish to account for pre-conviction participation, we restrict our attention to individuals who were old enough to register and vote in the 2008 presidential election.

Individuals may be convicted of multiple counts. In these cases, we may not have sufficient information about subsidiary counts to explain variation in either sentencing or participation. For this reason, our analysis is restricted to cases where individuals are convicted of only a single count.

The PSC data include information on the most serious sentence assigned, which ordered from most to least serious are: state prison, state intermediate punishment, county jail, restrictive intermediate punishment, probation, and other restorative sanction. We use the most serious sentence to construct our two key treatment variables. First, the variable *Sentenced to Prison* is coded 1 if the most serious sanction was confinement in a state prison and 0 for all other sanctions. Second, the variable *Sentenced to Jail* is coded 1 if the most serious sanction was confinement in a county jail and 0 if it was a lesser sentence. The jail measure is coded as missing for offenders sentenced either to state prison or state intermediate punishment, a step down treatment program for eligible drug or alcohol users. Thus, for our analysis focusing on the effect of being sentenced to jail, those cases are discarded.

An important substantive point about these data is that some individuals who are coded as not being sentenced to prison will ultimately spend time in prison if they recidivate or violate the terms of their probation. In particular, we know from the Corrections data described next that 5.6 percent of the people in our sample who are assigned to a sentence other than time in prison end up serving time in prison between

2008 and 2012. If we instead were to simply compare those who spend time in prison to those who do not, this could yield a biased estimate of the effect of incarceration because the same behavior that causes people to end up in prison despite an initially more lenient sentence may also be associated with reduced political participation (for example, committing another crime while on probation will result in being sent to prison and may also cause someone to be less likely to participate). Thus, we use whether someone was sentenced to prison, rather than whether someone spent in time in prison, as our treatment indicator. Our estimates are therefore analogous to intent-to-treat (ITT) effects where being sentenced to prison is associated with about a 95 percent increase in the probability that someone spent time in prison.

Our PSC data analysis focuses on the subsample of sentencing records for people who have not been previously convicted of a crime in Pennsylvania. We do so because prior scholarship suggests this is the population for which a prison sentence is most likely to disrupt participation. To identify this subsample of first time offenders, we use the PSC's coding of the individuals' prior record score (PRS), and include only individuals with a PRS of 0. Finally, we restrict our sample to less serious crimes – those with an OGS of nine or less – because individuals who are sentenced to prison for more serious crimes are likely to still be in prison during the 2012 presidential election and are therefore mechanically unlikely to vote. The resulting dataset contains observations for 33,999 individuals.

Although we focus on people convicted of less serious crimes, some people sentenced to prison in our data might still be in prison during the 2012 election. Unfortunately, the sentencing data do not contain information about the date of discharge.

We therefore also create a variable using the Corrections data described next that is whether someone was still in state prison during the 2012 presidential election because of their initial sentence.[9]

Pennsylvania Corrections Data

We supplement our sentencing data with records obtained from the Pennsylvania Department of Corrections (PDC) for the 204,254 people who have been incarcerated in Pennsylvania prisons since 1990. The PDC data include full name, date of birth, gender, race, and a unique identifier that allows us to link the individual's prison experiences over time. In addition to merging these data to the PSC records, we also directly linked these data to voter records from 2008 and 2012.. 13,296 individuals first serve time in a Pennsylvania prison between these two elections.

Pennsylvania Voting Data

Voting records come from the Pennsylvania Voter File (PVF), which contains the full name, address, gender, birthdate, and vote history of all individuals registered to vote in Pennsylvania. One potential issue with using voter file records to measure participation is that registration records may be removed ("purged") from the voter file when a voter is no longer an active registrant. This issue is particularly salient when measuring the participation of people who have been incarcerated. For this reason, we use voter files collected close to each election to measure participation. Specifically, we use a PVF from April 2009 to measure registration and turnout in the 2008 presidential election and a

[9] This coding does not code individuals as still in prison if they return for reasons apart from completing their initial period of incarceration.

PVF from December 2012 to measure registration and turnout in the 2012 presidential election.[10] Individuals retain common unique identifiers in the state voter file across elections even when they change or update their registration.

Measuring the turnout behavior of people in the sentencing and corrections datasets requires that we link observations in those sources to the voter file. There is no common unique identifier across the sentencing, corrections, and voting datasets. Additionally, neither the sentencing nor corrections datasets contain addresses. Thus, we follow Meredith and Morse (2014b) and search the PVF for records with a similar name and birthdate as records in the PSC and PDC. Details on this merging process and a discussion of measurement error appear in the supplemental appendix.

Connecticut

Our data from Connecticut are similar to the corrections and voter file data from Pennsylvania. We have records of felons who are incarcerated in state prisons and match those records to pre- and post-incarceration voter files. One contextual difference is that Connecticut is not a contested presidential election state, which when compared to Pennsylvania may reduce efforts to register and mobilize eligible released felons.

To identify formerly incarcerated felons, the Connecticut Secretary of State's office provided us with monthly records of all individuals convicted of a felony from January 2004 to the end of September 2012, as well as lists of all individuals released

[10] The April 2009 datafile is the first statewide file with accurate 2008 turnout and does not appear to have been subject to a post-election purge. Because not all counties fully updated their 2012 presidential turnout records in the December 2012 PVF, we also used a December 2013 PVF to identify 2012 voters.

from prison and discharged from parole during this period after having been sentenced for a felony.[11] In Connecticut, incarcerated felons lose their voting rights until they complete both their prison sentence and the terms of their parole. Using the unique state inmate identifier, these records were compiled to create a list of 4,572 individuals who were old enough to vote in 2008, convicted of a felony and first incarcerated in Connecticut after May 1, 2009, and released from prison and discharged from parole by October 1, 2012. This means that our list is first time Connecticut felons who were incarcerated and then released between the 2008 and 2012 elections.

We merge these data to a Connecticut voter file produced by a third party vendor after the November 2008 election. (The file date is April 2009; unlike in Pennsylvania, we do not know whether the file has been purged post-election, so we restrict attention to individuals who were incarcerated beginning in May of that year.) We also independently merge it to a state voter file produced after the 2012 election (The file date is January 2013). To conduct this merge, which is explained in greater detail in the supplemental appendix, we relied on the date of birth, name, and address information for felons obtained from the Connecticut Secretary of State.[12]

[11] The Secretary of State's office did not produce files for December of 2007, July or December of 2008, and August or October of 2010, so we lack information for these five months. Some of these future felons can be identified from subsequent release files, but we exclude such cases due to the inability to capture the entire population for these months.

[12] We note that these incarceration data include a home address, which makes matching to the voter files easier than in Pennsylvania.

Results

Approach One

We first present analysis from both Pennsylvania and Connecticut using corrections data merged to participation records to estimate the effect of incarceration on voting over time. This analysis compares incarcerated individuals to themselves before and after spending time in prison. Estimates for 13,296 Pennsylvania residents first incarcerated after the 2008 election and released before the 2012 election appear in Table 2. Columns (1) and (2) show that the formerly incarcerated individuals participate at low rates after going to prison. 43.8% of these released prisoners were registered to vote in 2012 and 13.9% voted in the 2012 election. Apart from the fact that participation is generally increasing in age for young adults, if the prison experience causes people to be less likely to vote, we would expect these individuals' turnout rate to be substantially higher in the 2008 presidential election than it was in 2012. However, columns (3) and (4) show that this group also registered and voted at low rates in 2008: 44.4% were registered in 2008 and 13.5% voted. Thus in columns (5) and (6) when we compare 2012 to 2008 registration and turnout for members of this group, we see that they are 0.6 percentage points less likely to be registered but 0.4 percentage points more likely to vote after going to prison than before doing so. Neither estimate is very large.

Paralleling our analysis from Pennsylvania, we next examine participation over time for Connecticut residents who first spend time in prison between the 2008 and 2012 elections. There are 4,572 felons who are incarcerated after the 2008 election and complete their entire sentence before the 2012 election. Table 3 displays their over-time

Table 2: 2012 and 2008 Voting and Registration Among Pennsylvania Residents Incarcerated in State Prison after 2008 Election and Released before 2012 Election

	(1) Registered 2012 (1=yes)	(2) Voted 2012 (1=yes)	(3) Registered 2008 (1=yes)	(4) Voted 2008 (1=yes)	(5) Change in Registration 2012-2008	(6) Change in Voting 2012-2008
Mean	0.438	0.139	0.444	0.135	-0.006	0.004
[Standard Deviation]	[.4961]	[.3464]	[.4969]	[.3421]	[.4247]	[.3913]

Note: N=13,296. Means with standard deviations in brackets. See text for details.

patterns of registration[13] and voting in a manner parallel to Table 2. As in Pennsylvania, released felons in Connecticut have low levels of participation. Column (1) shows that 40.2% of these felons are registered in November 2012 and column (2) shows that 6.7% voted in that election. By contrast, column (3) shows that pre-incarceration, 35.6% are registered and turnout is 10.8% (column 4). Thus, per column (5), incarceration is associated with a modest 4.6 percentage point increase in registration in Connecticut, while voting declines by 4.1 points (column 6). These estimates for turnout are slightly more negative than the comparable figures for Pennsylvania.[14] Overall, when comparing released criminals to themselves, it is clear that this group votes at much lower rates than the general population prior to going to prison. Additionally, incarceration is associated with a small decrease in post-release voting in Pennsylvania but a larger decline in

[13] Some felons appear in the Connecticut voter file with the flag for felon, meaning that they cannot vote until they reregister. These cases are coded as unregistered. In Connecticut, lists of incarcerated felons are made available to local registrars of voters who have the responsibility for removing those individuals from the rolls. However, many of the registrants in the 2013 voter file have registration dates preceding their release date, implying they were never successfully purged.

[14] One possible explanation for the larger decline in turnout in Connecticut relative to Pennsylvania is a different electoral context. Because Connecticut was not competitive in the 2012 presidential election, there may have been fewer efforts to register or mobilize formerly incarcerated individuals. Additionally, the turnout rate in Connecticut declined between these two elections. For the entire state, the voting age population turnout rate declined by 4.7 points, compared to a 4.1 point decline in Pennsylvania.

Table 3: 2012 and 2008 Voting and Registration Among Connecticut Residents Incarcerated in State Prison after 2008 Election and Released before 2012 Election

	(1)	(2)	(3)	(4)	(5)	(6)
	Registered 2012 (1=yes)	Voted 2012 (1=yes)	Registered 2008 (1=yes)	Voted 2008 (1=yes)	Change in Registration 2012-2008	Change in Voting 2012-2008
Mean	0.402	0.067	0.356	0.108	0.046	-0.041
[Standard Deviation]	[.4903]	[.2503]	[.4788]	[.3099]	[.3413]	[.3455]

Note: N=4,572. Means with standard deviations in brackets. See text for details.

Connecticut.

Approach Two

Our first approach, although informative, is somewhat crude, because it does not account for the changing effects of demographics and electoral context. In approach two, by contrast, we compare the 2012 participation of formerly incarcerated individuals to other 2008 registrants. As we explain above, this requires us to discard previously unregistered released prisoners. As with all of our attempts to isolate the causal effect of serving prison time, the key threat to inference is that those who spend time in prison may be different from those who do not serve time. Our findings here demonstrate the importance of the selection issue. We show that as we take steps to make these two groups (those who serve prison time and those who do not) more similar through the inclusion of additional covariates and past records of participation, our estimates of the negative effect of incarceration on voting decline substantially. In Pennsylvania, we initially estimate that 2008 registrants who spend time in prison are 31 percentage points less likely to vote in 2012 than other registrants, but this estimate declines to 6 points in our final analysis. In Connecticut, our initial baseline estimate is larger, 37 points, but declines by a similar amount to 14 points.

We first use our matched Pennsylvania Corrections and Pennsylvania Voter File data to estimate how experiencing incarceration affects participation. The sample for this analysis is the 8,544,483 people who were registered in Pennsylvania in 2008, 5,897 of whom were incarcerated and then released between 2008 and 2012. Estimates using these data appear in Table 4. The results shown in column (1) reveal that formerly incarcerated individuals were 30.6 percentage points ($p<.01$) less likely to vote in the 2012

presidential election than the average 2008 registrant. In column (2) we add fixed effects for zip codes as a measure of the effect of community characteristics, which reduces the estimated effect to 27.2 points. In column (3) we add available demographics variables, which in conjunction with zip code fixed effects together explain about a quarter of the originally estimated turnout gap. Per column (3) formerly incarcerated individuals are 23.5 points ($p<.01$) less likely to vote in 2012 when we parametrically control for age, party of registration, gender, and the zip code of residence. One initial conclusion from this analysis is that even after eliminating measurement error due to self-reporting there is still a very sizable difference in the voting rates of those who serve time in prison and those who do not. The open question is how much of the observed difference in voting rates in 2012 (that is, participation after serving time) is attributable to differences across groups that predate the spell of incarceration.

Before the experience of going to prison, future inmates were already much less likely to vote than other registrants. This level of pre-incarceration voting may explain low rates of voting in 2012, and this confounds our efforts to isolate the causal effect of incarceration on voting. Up to now, however, our analysis has not accounted for 2008 (pre-incarceration) participation. In column (4) we do so, adding to our previous specification an indicator for voting in 2008. This substantially reduces the apparent negative effect of incarceration on voting. Per column (4), turnout is 5.6 points ($p<.01$) lower among those who serve time. Cumulatively, this analysis shows that more than 80% of the difference in 2012 turnout between those 2008 registrants who serve time and those who do not can be explained by a sparse set of pretreatment controls including pre-

Table 4: 2012 Pennsylvania Voting Among 2008 Registrants by Incarceration Status

	(1)	(2)	(3)	(4)
	Dependent Variable is Voted 2012 (1=yes)			
Formerly Incarcerated (=1 if incarcerated in State Prison after November 5, 2008 and released by November 6, 2012)	-0.306	-0.272	-0.235	-0.056
	[0.006]***	[0.010]***	[0.010]***	[0.008]***
Voted 2008 (1=yes)				0.552
				[0.004]***
Age in years			0.002	0.001
			[0.000]***	[0.000]***
Age squared / 100			0.000	0.000
			[0.000]	[0.000]
Registered Democrat (1=yes)			0.120	0.046
			[0.002]***	[0.001]***
Registered Republican (1=yes)			0.143	0.071
			[0.002]***	[0.001]***
Gender=Male			-0.017	-0.002
			[0.002]***	[0.001]**
Gender=Unknown			-0.137	-0.087
			[0.003]***	[0.003]***
Constant	0.550	0.550	0.384	0.091
	[0.000]***	[0.000]***	[0.004]***	[0.004]***
Observations	8,544,483	8,544,483	8,544,483	8,544,483
R-squared	0.000	0.000	0.030	0.290
Zip Code Fixed Effects	No	2,099	2,099	2,099

Note: There are 5,897 formerly incarcerated individuals in this sample. OLS coefficients with robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%, clustered at the zip code level in columns (2)-(4). See text for details.

incarceration participation.[15]

We now replicate this analysis using data from Connecticut. There are 2,399,214 Connecticut residents who were registered in 2008 according to an April 2009 voter file, 1,627 of whom first spend time in Connecticut prisons and completed their sentence between these two elections. Results using this sample appear in Table 5, which parallels Table 4. In column (1), we begin by predicting voting in the 2012 general election using an indicator for former felon status and find that felons are 36.8 ($p<.01$) percentage points less likely to vote than other 2008 registrants. In column (2), we add precinct fixed effects and the estimate is reduced to 29.0%. In column (3) we then add the same covariates used in Pennsylvania as well as an indicator for race (made available by the voter file vendor). This reduces the effect of former felon status to 23.6%. Finally, in column (4) we add an indicator for 2008 voting which vastly reduces the negative effect of incarceration on voting, to 13.8%. Thus, moving from column (1) to column (4) reduces the estimate effect of incarceration on voting by 23 points, or about 63%.

Overall, in both Pennsylvania and Connecticut we initially estimate a large negative effect of incarceration on voting that declines when we include covariates, including past participation, to account for differences between those who spend time in prison and those who do not. What remains uncertain, however, is whether the smaller figures (6 points in Pennsylvania, 14 in Connecticut) are accurate estimates of the direct effect of incarceration on future participation among prior registrants. There may be other

[15]If we employ a non-parametric matching technique instead of using OLS regression, we find a slightly less negative relationship between incarceration and 2012 turnout. Using matching has a similar effect for the Connecticut data. Results available upon request.

Table 5: 2012 Connecticut Voting Among 2008 Registrants by Incarceration Status

	(1)	(2)	(3)	(4)
	Dependent Variable is Voted 2012 (1=yes)			
Formerly Incarcerated (=1 if incarcerated in State Prison after May 1, 2009 and released by October 1, 2012)	-0.368	-0.290	-0.236	-0.138
	[0.008]***	[0.011]***	[0.011]***	[0.010]***
Voted 2008 (1=yes)				0.532
				[0.007]***
Age in years			0.031	0.020
			[0.001]***	[0.000]***
Age squared / 100			-0.029	-0.018
			[0.001]***	[0.000]***
Gender=Male			-0.022	-0.008
			[0.001]***	[0.001]***
Gender=Unknown			-0.149	-0.044
			[0.007]***	[0.006]***
Race=Asian			-0.066	-0.032
			[0.004]***	[0.003]***
Race=Black			0.036	0.012
			[0.003]***	[0.003]***
Race=Hispanic			-0.037	-0.016
			[0.003]***	[0.002]***
Race=Jewish			0.007	0.005
			[0.002]***	[0.002]***
Race=Middle Eastern			-0.034	-0.024
			[0.004]***	[0.004]***
Race=Native American			-0.016	-0.006
			[0.010]	[0.008]
Race=Unknown			-0.040	-0.020
			[0.006]***	[0.005]***
Registered Democrat (1=yes)			0.085	0.029
			[0.002]***	[0.001]***
Registered Republican (1=yes)			0.071	0.034
			[0.002]***	[0.001]***
Constant	0.487	0.487	-0.276	-0.313
	[0.000]***	[0.000]***	[0.015]***	[0.015]***
Observations	2,399,214	2,399,195	2,399,195	2,399,195
R-squared	0.000	0.000	0.070	0.320
Precinct Fixed Effects	No	789	789	789

Note: There are 1,627 formerly incarcerated individuals in this sample. * significant at 10%; ** significant at 5%; *** significant at 1%, clustered at the precinct level in columns (2)-(4). See text for details.

static omitted factors that explain future criminal behavior and low levels of participation, as well as factors that change over time and explain both the likelihood of being incarcerated and changes in political participation. Additionally, this analysis discards previously unregistered prisoners. In light of these concerns, we now turn to our comparison of different groups of convicts.

Approach Three

The key concern about Approach Two is that individuals who are convicted of a crime are likely different from those who are not convicted of a crime in unobserved ways even after we account for demographic factors and past participation. By contrast, Approach One does not allow us to control for the changing effects of demographics or electoral context on voting. For these reasons, in this section we use the dataset created by merging sentencing data to voter records to compare the political participation of two groups of convicts: those who spend time in prison or jail and those who are never incarcerated. This analysis therefore holds constant that a person has been found guilty of a crime and exploits variation in the sentence they are assigned. Additionally, we use the behavior of convicts given non-prison sentences to account for the changing effects of electoral context and demographics on voting. Overall, this approach addresses many of the weaknesses of both earlier approaches. As a reminder, this sample is restricted to the 33,999 individuals who are first convicted of a single count of a less-serious crime between November 12, 2008 and January 1, 2010.

To give a sense of these data, Table 6 presents summary statistics showing the number of people given different sentences in our dataset and the change in their turnout between 2008 and 2012. The first column shows that 16.5 percent of people sentenced to

prison voted in the 2012 presidential election, a decline of 6.9 points from their 23.4% rate of voting in 2008. The second column of the table displays similar data for individuals who are given any sentence besides time in state prison. Their 2012 turnout is actually slightly *lower*, 15.9 percent, but declined by less from 2008 (by 6.0 points) than for those who are sentenced to prison. In the remainder of the table, we further partition the data for those given any sentence other than time in prison. Overall, two patterns stand out in Table 6. First, turnout is modest for all of these groups in 2008 prior to being sentenced. Second, turnout declines by a roughly similar amount for individuals given different sentences (the one exception is the very small number of people given State Intermediate Punishment, whose 2008 turnout is less than half of any other group but whose 2012 turnout increases), providing initial evidence that the experience of going to prison is not uniquely demobilizing.

Interpreting the patterns in Table 6 is complicated, however, by differences in the demographic and other characteristics (e.g., crimes committed) of people sentenced to prison instead of other forms of punishment. For example, in our sample, those who are sentenced to prison rather than given another sentence are younger, more likely to be male, and more likely to be Black or Hispanic. Additionally, those sentenced to prison have committed more serious offenses, are more likely to have committed a felony or a drug crime, and have a higher guideline recommended minimum sentence. Because all of those factors could have an independent effect on participation, we next present multivariate analysis accounting for these differences across sentences.

Specifically, Table 7 compares 2012 participation for 33,999 individuals found guilty between 2008 and 2010, some of who were sentenced to prison (N=449) and the

Table 6: 2012 and 2008 Voting for Pennsylvania Convicts Sentenced to Different Punishments Between These Two Elections, Summary Statistics by Group

	Sentence						
			Partition of Non-Prison Sentences, by Decreasing Severity of Sentence				
Variable	State Prison	Non-Prison Sentence	State Intermediate Punishment	County Jail	Restrictive Intermediate Punishment	Probation	Other Restorative Sanction
Voted in 2012 (1=yes)	0.165	0.159	0.143	0.159	0.154	0.161	0.158
	[.3714]	[.3655]	[.3563]	[.3658]	[.3609]	[.3671]	[.3652]
Voted in 2008 (1=yes)	0.234	0.219	0.107	0.217	0.216	0.221	0.224
	[.4238]	[.4133]	[.315]	[.412]	[.4118]	[.4149]	[.4173]
Change in turnout 2012-2008	-0.069	-0.060	0.036	-0.058	-0.063	-0.060	-0.066
	[.4352]	[.406]	[.3313]	[.4041]	[.4103]	[.4053]	[.4213]
End up in prison at any time (1=yes)	1.000	0.056	0.643	0.062	0.032	0.058	0.062
	[0]	[.229]	[.488]	[.2403]	[.1765]	[.2342]	[.2411]
N	449	33,550	28	12,026	5,777	14,990	727

Note: N=33,999. Means with standard deviations in brackets. See text for details.

remainder who were not (N=33,550).[16] Even without any additional controls, we see in Column (1) that those convicts sent to prison are no less likely to vote in 2012 than those who were not sent to prison. Columns (2) through (5) show that this relationship remains statistically insignificant when we control for pretreatment registration and participation (column 2); demographic variables like age, gender, and race (column 3); and the type and severity of the crime as well as the recommended minimum sentence (column 4). The complete model in column (5) shows that people who were sentenced to prison were 0.3 percentage points (standard error 1.7 points) less likely to vote in 2012 than people sentenced to other forms of punishment after controlling for all of these variables. Thus, our best regression estimate is that for this population of first-time convicted criminals, spending time in prison reduces participation by less than one-third of a percentage point, and any effect larger than 3.7 percentage points falls outside the 95% confidence interval. In column (6) when we exclude people who are sentenced to prison and who are still in prison in 2012 for their initial sentence (and are therefore mechanically unable to vote), the estimate is smaller: 0.1 points.

These specifications use regression adjustments to account for the observed differences between those sent to prison and those not sentenced to prison. Column (7) shows that we obtain substantively similar results when we instead employ matching to pair individuals who are sent to prison with similar individuals who are not sent to prison. Specifically, we match exactly on 2008 participation and registration, gender, race, and type and severity of the convicted crime. If there are multiple matches, we then select the person closest to them in age, while also requiring that the person be no more than 2.5

[16] Summary statistics for the regression sample appear in the supplemental appendix.

Table 7: 2012 Voting for Pennsylvania Convicts Sentenced to Prison or Another Punishment Between 2008 Election and 2012 Election

	Dependent Variable is Voted 2012 (1=yes)						
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Bivariate Regression	Controlling for Past Participation	Adding Demographics	Adding Crime Characteristics	Complete Model	Excluding People Still in Prison in '12	Matched Pairs
Any prison sentence assigned (1=yes)	0.006	-0.001	0.000	-0.003	-0.003	-0.001	0.008
	[0.018]	[0.016]	[0.016]	[0.017]	[0.017]	[0.018]	[0.029]
Registered in 2008 (1=yes)		0.071	0.071	0.071	0.072	0.072	
		[0.004]***	[0.004]***	[0.004]***	[0.004]***	[0.004]***	
Voted in 2008 (1=yes)		0.362	0.361	0.362	0.361	0.361	
		[0.007]***	[0.007]***	[0.007]***	[0.007]***	[0.005]***	
Age in years (2012)			0.001		0.001	0.001	0.150
			[0.001]		[0.001]	[0.001]	[0.170]
Age squared / 100 (2012)			-0.001		-0.001	-0.001	-0.156
			[0.001]		[0.001]	[0.001]	[0.199]
Gender=Male			-0.003		-0.004	-0.004	
			[0.004]		[0.004]	[0.004]	
Race=Other			0.000		0.000	0.000	
			[0.007]		[0.007]	[0.007]	
Race=Black			-0.003		-0.004	-0.005	
			[0.005]		[0.005]	[0.005]	
Race=Hispanic			0.010		0.008	0.008	
			[0.013]		[0.013]	[0.014]	
Guideline recommended minimum sentence (in years)				0.001	0.001	0.002	1.098
				[0.014]	[0.014]	[0.014]	[0.758]
Offense Gravity Score = 2				-0.003	-0.004	-0.004	
				[0.006]	[0.006]	[0.006]	
Offense Gravity Score = 3				0.005	0.006	0.006	
				[0.005]	[0.005]	[0.005]	
Offense Gravity Score = 4				0.018	0.018	0.018	
				[0.012]	[0.012]	[0.012]	
Offense Gravity Score = 5				-0.011	-0.012	-0.012	
				[0.006]*	[0.006]*	[0.006]*	
Offense Gravity Score = 6				0.003	0.003	0.002	
				[0.015]	[0.015]	[0.015]	
Offense Gravity Score = 7				-0.013	-0.011	-0.012	
				[0.017]	[0.018]	[0.018]	
Offense Gravity Score = 8				0.025	0.026	0.021	
				[0.024]	[0.024]	[0.024]	
Offense Gravity Score = 9				0.028	0.035	0.036	
				[0.031]	[0.031]	[0.031]	
Any felony conviction (1=yes)				0.002	0.003	0.003	
				[0.007]	[0.007]	[0.008]	
Is drug offense (1=yes)				-0.008	-0.006	-0.007	
				[0.005]*	[0.005]	[0.005]	
Constant	0.159	0.045	0.025	0.046	0.023	0.024	-3.436
	[0.002]***	[0.002]***	[0.020]	[0.003]***	[0.021]	[0.021]	[3.411]
Observations	33,999	33,999	33,999	33,999	33,999	33,944	638
R-squared	0.000	0.219	0.221	0.220	0.221	0.221	0.010
Prop. Voting 2012	0.159	0.159	0.159	0.159	0.159	0.159	0.163
Prop. Voting 2008	0.219	0.219	0.219	0.219	0.219	0.219	N/A
Number Treated (to Prison)	449	449	449	449	449	449	319
County Fixed Effects?	No	No	Yes	No	Yes	Yes	No
Number of pairs (fixed effects)							319

Note: OLS coefficients with robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%, robust standard errors. See text for details.

years older/younger than the incarcerated person and have a guideline recommended sentence that differs by no more than 4 months. We find control observations that satisfy these criteria for 319 of the 449 cases in which people were sentenced to prison. In this analysis, we estimate that people sentenced to prison were 0.8 points (standard error 2.9) *more* likely to vote than observably similar people sentenced to some other form of punishment.

Table 6 highlighted that a vast majority of people who are incarcerated in our sample are sentenced to serve time in a county jail rather than in a state prison. Because many of the mechanisms that previous research has highlighted for how time in prison might reduce participation could also cause people to vote less after spending time in county jail, we also explore whether people who are sentenced to serve time in county jail vote at a different rate than people sentenced to less serious sanctions. The dataset for this analysis is 33,562 people sentenced to jail (N=12,026) or a less serious non-incarcerative penalty. Results appear in Table 8 and show that people who are sentenced to jail vote at a similar rate to people sentenced to these other punishments.

For example, the complete model presented in column (5) shows that people sentenced to jail are 0.2 percentage points (standard error 0.4 points) *more* likely to vote than people sentenced to less serious sanctions when we control for pretreatment registration and participation, demographic variables, the type and severity of the crime, and the recommended minimum sentence. Because the number of people sentenced to jail is much larger than the number of people sentenced to prison, these estimates are very precise. In column (6) when we estimate results using the same matching algorithm described above, our estimate is -0.1 points.

Table 8: 2012 Voting for Pennsylvania Convicts Sentenced to Jail or a Non-incarcerative Punishment Between 2008 Election and 2012 Election

	Dependent Variable is Voted 2012 (1=yes)					
	(1) Bivariate Regression	(2) Controlling for Past Participation	(3) Adding Demographics	(4) Adding Crime Characteristics	(5) Complete Model	(6) Matched Pairs
Any jail sentence assigned (1=yes)	0.001	0.002	0.002	0.002	0.002	-0.001
	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]	[0.004]
Registered in 2008 (1=yes)		0.071	0.071	0.071	0.071	
		[0.004]***	[0.004]***	[0.004]***	[0.004]***	
Voted in 2008 (1=yes)		0.363	0.362	0.362	0.362	
		[0.005]***	[0.005]***	[0.005]***	[0.005]***	
Age in years (2012)			0.001		0.001	0.096
			[0.001]		[0.001]	[0.036]***
Age squared / 100 (2012)			-0.001		-0.001	-0.105
			[0.001]		[0.001]	[0.045]**
Gender=Male			-0.004		-0.004	
			[0.004]		[0.004]	
Race=Other			0.000		0.000	
			[0.007]		[0.007]	
Race=Black			-0.003		-0.005	
			[0.005]		[0.005]	
Race=Hispanic			0.012		0.010	
			[0.014]		[0.014]	
Guideline recommended minimum sentence (in years)				-0.012	-0.013	0.004
				[0.018]	[0.018]	[0.091]
Offense Gravity Score = 2				-0.003	-0.004	
				[0.006]	[0.007]	
Offense Gravity Score = 3				0.006	0.006	
				[0.005]	[0.005]	
Offense Gravity Score = 4				0.016	0.016	
				[0.012]	[0.012]	
Offense Gravity Score = 5				-0.008	-0.009	
				[0.007]	[0.007]	
Offense Gravity Score = 6				0.003	0.004	
				[0.016]	[0.016]	
Offense Gravity Score = 7				-0.001	0.001	
				[0.019]	[0.019]	
Offense Gravity Score = 8				0.034	0.034	
				[0.026]	[0.026]	
Offense Gravity Score = 9				0.050	0.057	
				[0.034]	[0.034]*	
Any felony conviction (1=yes)				0.002	0.002	
				[0.008]	[0.008]	
Is drug offense (1=yes)				-0.008	-0.006	
				[0.005]	[0.005]	
Constant	0.159	0.044	0.028	0.046	0.026	-1.810
	[0.002]***	[0.003]***	[0.020]	[0.004]***	[0.021]	[0.730]**
Observations	33,562	33,562	33,562	33,562	33,562	22,862
R-squared	0.000	0.220	0.222	0.220	0.222	0.001
Prop. Voting 2012	0.159	0.159	0.159	0.159	0.159	0.156
Prop. Voting 2008	0.219	0.219	0.219	0.219	0.219	N/A
Number to jail (treated)	12,026	12,026	12,026	12,026	12,026	11,431
County Fixed Effects?	No	No	Yes	No	Yes	No
Number of pairs (fixed effects)						11,431

Note: OLS coefficients with robust standard errors in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%, robust standard errors. See text for details.

Overall, the data shown in Table 6 and analysis presented in Table 7 provide the most credible causal estimates of the effect of incarceration in prison (or spending time in jail, per Table 8) on voting. These results show that spending time in prison (or jail) is not associated with a large reduction in voting. In fact, there may be no negative effect of spending time in prison on voting. Our *largest* estimate is -0.3 points, which is 95% smaller than our smallest (and most credible) estimate of the effect of incarceration on voting from our comparison of incarcerated individuals to other registrants in Pennsylvania (column [4] of Table 4). Thus, once we account for selection bias by restricting our attention to convicted criminals, we find that incarceration appears to have almost no effect on voting. Moreover, even our most robust model that compares convicts to other registrants by controlling for past participation cannot account for those unobserved factors that likely explain changes in participation and the occurrence of being a prisoner. In fact, the estimates from Table 7 are very similar to the simple over time comparison of people who spend time in prison to themselves (Approach One, Table 2), which showed that having spent time in prison had almost no effect on changes in voting.

Discussion and Conclusion

What are the effects of the expansion of the criminal justice system on mass political behavior? More generally, how do state programs affect citizen behaviors? This paper addresses the specific question of whether one (negative) way in which the state "makes citizens," by subjecting them to time in prison, subsequently reduces their political participation. In contrast to earlier research that has often relied on self-reports of

participation and experience with the criminal justice system, we use administrative records from two states, Pennsylvania and Connecticut, to estimate the effect of incarceration on voting.

The most striking finding from the analyses presented in this paper is that it appears that spending time in prison does not have enormous effects on subsequent participation. Our initial examination of all prisoners shows that they vote at low rates both before and after going to prison. Focusing on those who were registered prior to experiencing incarceration, when we simply compare the voting rates in a given election of released prisoners to that of other registrants, we initially estimate that serving time in prison is associated with large reductions in participation: By 31 percentage points in Pennsylvania and 37 points in Connecticut. However, as we include additional covariates that account for observable differences between released prisoners and other registrants, such as past participation and geography, these estimates shrink dramatically. Thus, our most persuasive estimates that compare first time incarcerated prisoners to all registrants are that spending time in prison reduces subsequent voting by 6 percentage points in Pennsylvania and 14 percentage points in Connecticut. As we note, however, even these estimates are likely too large for the effect of incarceration on all incarcerated individuals, both because they are restricted to a subset of the released prisoner population (those who were registered before going to prison) and, of greatest significance, because they do not account for many other important differences that likely explain the low participation of those who serve time in prison.

Thus, the most credible overall estimates we provide are those that take advantage of our sentencing data from Pennsylvania. In this comparison, we examine how 2012

participation differs for those criminals who are sentenced to prison compared to similar convicts who are given a sentence other that time in prison. In this more comparable group of individuals, convicts given different sentences, our best estimate is that going to prison reduces participation by 0.3 percentage points when we use a parametric regression model (Column 5 of Table 7) or increases it by 0.8 points when we preprocess using matching (Column 7 of Table 7). Similarly, the effect of being sentenced to jail rather than some lesser punishment appears to either slightly increase or have no effect on participation. One important caveat should be highlighted. Our estimated effect of prison time is for a particular group of subjects, those who were convicted of a crime and had an offence gravity score and record of previous convictions such that prison was an option but not a requirement.

What should one make of the magnitude of these estimates and this pattern? In an experimental design, random assignment ensures that in expectation those treated and those assigned to control are no different. In observational analysis, by contrast, we cannot make this assumption and must instead take steps to account for differences between the treatment and control groups. Here, it appears that the more similar we make our treatment and control groups, the smaller the apparent effect of incarceration becomes. As we show, simply controlling for past behavior and the fact that one is convicted of a crime accounts for an extremely large portion of the apparent effect of incarceration on voting. When we make our treatment and control groups as similar as is possible by comparing convicts to other convicts, the apparent negative effect of incarceration on voting largely evaporates.

Furthermore, even when relying on the subset of convicted individuals, we should

remain cautious in attributing these small estimates to the effect of incarceration per se because selection issue may still be present. It could be that judges are more likely to sentence to prison individuals who display (unmeasured) markers of negative behavior, and that those markers are also likely to predict low levels of future participation. Of course, there is also sufficient sampling error in our estimates of the effects of prison that we cannot reject there being small demobilizing effects of going to prison even if our model completely eliminates selection bias. This caveat aside, these data arguably provide the most credible causal estimate of the effect of incarceration on participation for a particularly important group of individuals: Those experiencing their first conviction. Because this group votes at a low rate post-incarceration, one may be tempted to conclude that this is because of their experience while incarcerated. However, the results in this paper cast doubt on this interpretation by showing that this group only voted at a slightly higher rate prior to incarceration and that people convicted of similar crimes, but not sentenced to incarceration, experience a similar decline in turnout after their first conviction.[17]

This point also helps illustrate a more general implication of this work. While we have focused on whether incarceration reduces voting, scholars are broadly interested in how the experience of incarceration affects different outcomes such as earnings, marital prospects, and recidivism (e.g., Hjalmarsson 2009; Pager 2003; Western 2002). The approaches we undertake here show the difficulty of providing credible causal estimates

[17] Suggesting that these differences are not driven by large differences across subgroups, if we restrict the Table 7 column (7) matching analysis to 2008 registrants, the estimated effect of incarceration is -.7 points (p=.89).

in the absence of careful attention to selection bias. Furthermore, while spending time in prison does not appear to reduce voting, there may be other outcomes for which the research designs we use here would reveal persuasive evidence of the negative effects of incarceration.

Returning to our original question, if incarceration does not cause reduced participation, why do prisoners vote at such low rates before going to prison, and why is participation so much lower than average in those poor and crime-ridden neighborhoods that are the homes to many criminals before they go to prison? In answering this question, we highlight two broad areas for subsequent research. Of course, it could be the case that convicts do not vote because of the same personal characteristics and unfortunate life events that lead to an increased risk of serving time in prison. Alternatively, however, their experiences with, and treatment by, the state prior to being convicted may explain low rates of voting. Although we find little change in participation after people are first incarcerated, first incarceration spells often occur after a long series of negative interactions with other criminal justice institutions. Moreover, many individuals who become incarcerated live in neighborhoods with aggressive policing that may discourage citizen involvement (Lerman and Weaver 2014b) or attend poor schools that are argued to teach young men that their voices are unworthy of attention (Bowles and Gintis 1976). Thus, the narrow focus on incarceration may miss the many other ways in which the state has already "unmade" these citizens. Other scholars have offered these potential explanations and most research finds that going to prison has a more negative effect on voting than other state interactions, but we know of little research that has systematically examined how state treatment affects the broader worldview of those most

at risk for criminal convictions.

A second area for study, and one that may have implications regarding how policing policies affect social capital, is how community characteristics and the neighborhood environment shape voting. Thus, while sending many criminals to prison may not affect their voting, it might be because patterns of behavior are shaped not by that individual experience, but instead, for other (law-abiding) citizens, by the community environment. In that case, it might be fruitful for scholars to understand how changes in policing practices and the general decline of the crack epidemic and crime have affected citizen perceptions of crime and therefore the willingness by individuals living in these neighborhoods to become engaged in civic life and the act of voting.

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Supplemental Appendix for:

Does Incarceration Reduce Voting? Evidence about the Political Consequences of Spending Time in Prison from Pennsylvania and Connecticut

This appendix contains the following material:

Supplemental Appendix 1

This section describes the data preparation procedures for the analyses presented in Tables 2-8.

Pennsylvania Data Merge

We measure the political participation of individuals listed in the Pennsylvania Corrections and Sentencing data using the Pennsylvania Statewide Voter File (PSVF). The PSVF contains the name, address, birthdate, party registration, date of registration, and vote history of every person who is registered to vote in Pennsylvania. Unfortunately, there is no unique identifier that allows us to link records in the criminal justice data to the PSVF. We thus wrote a computer program to search the PSVF for a record that matched each record in the criminal justice data. In this case, a criminal justice record matches to the PSVF if there is a record in the PSVF that shares a similar name and the same birthdate.

We face a tension between two forms of measurement error when matching records across these sources. One form of measurement error occurs when we incorrectly match an individual in the criminal justice data to another individual's voter file record. The other form of measurement error occurs when we fail to match an individual in the criminal justice data to their own voter file record. When deciding whether a name in the PSVF matches a name in the criminal justice records, we face a tradeoff between these two forms of measurement error. Allowing for more discrepancies between matched names across the two sources reduces the number of matches we miss because of issues like the use of nicknames in one source, misspellings, or typos. Doing so, however, also expands the potential pool of other people that have a sufficiently similar name and the same birthdate.

Column 1 of Table SA1 shows the number of matches we find between the criminal justice records and the PSVF when we use different matching criterion. For example, the first row shows that we find 34,698 records in the corrections data that share the exact same first name, middle name, last name, and birthdate as a record in the PSVF. Table SA1 illustrates the importance of using a flexible matching definition when matching names across sources. There are about 48,000 more records in the corrections data that share the exact same first name, last name, and birthdate, and have a similar middle name, as a record in the PSVF (see Meredith and Morse (2014b) for more details on how we define a fuzzy match).

There are also thousands of additional potential matches that have the same birthdate and similar, but not identical, first or last names.

[Insert Table SA1 about Here]

We use the placebo matching approach proposed by Meredith and Morse (2014b) to make an informed tradeoff between these two forms of measurement error when deciding which of the matches presented in Table SA1 to include in our analysis. We match criminal justice records with slightly permuted birthdates to the PSVF. Permuting the birthdates in the criminal justice records makes it so the matches that we find are examples of the first form of measurement error. The total number of matches we find between the permuted criminal justice records and the PSVF approximates the number of matches between the actual criminal justice records and the voter file that are false. Thus, the additional matches we find in the actual data relative to the permuted data provide a sense of the number of additional true matches we capture by expanding our matching criteria. Based on the results of the placebo match presented in Table SA1, we count any criminal justice record that satisfies matching criterion 1 through 10 to match the PSVF.

Connecticut Data Merge

We also measure the participation of individuals who have been incarcerated in Connecticut. In cooperation with the Connecticut Secretary of State's office, we obtained lists of every person convicted of a felony in the state, as well as those released, between the beginning of January 2004 and the end of September 2012. These files included each individual's name, date of birth, address,[1] date of conviction, most serious crime for which they were convicted, release date and sentence length (in the release file only), and a unique identifier (an inmate number).[2] Using this inmate number, we merged the conviction and release files to compile a master list that consists of all individuals convicted of a felony since the

[1] The addresses in the conviction and release files may differ due to an individual's decision to reside in another location after they are released from prison.

[2] The Secretary of State's office did not produce files for December of 2007, July or December of 2008, and August or October of 2010, so we lack information for these five months. Some of these future felons can be identified from subsequent release files, but we exclude such cases due to the inability to capture the entire population for these months.

beginning of 2004, as well as anyone convicted of a felony prior to this date but released later than 2003.[3] Because we are interested in participatory history before the initial entrance into prison, we kept only the earliest conviction observation for those cases in which someone was incarcerated at multiple points during our time period. Because our analyses focus on registration before and voting in the November 2008 and 2012 elections, we removed from this list anyone whose first conviction was on or before the November 4, 2008 presidential election (i.e., these individuals became a felon prior to the first election we examine), as well as those too young to partake in both contests (i.e., those under eighteen on Election Day in 2008).

To measure the political participation of future felons relative to other citizens, we merged the list of these individuals with a Connecticut voter file[4] produced by a third party vendor shortly after the November 2008 presidential election.[5] Unfortunately, no unique identifier existed to facilitate this procedure. The only reliably consistent variables across both the conviction and voter files were the released felon's date of birth and last name. We might also suspect that the addresses were the same, but it is probable that a number of individuals moved but failed to re-register before their conviction. In addition, informal versions or inaccurate recordings of first names hindered an exact match. In the former situation, a name may have been listed as "Mike" in the release file but took the more proper "Michael" form on the voter roll. With respect to the latter, variations of the same name (e.g., "Steven" vs. "Stephen") or simple misspellings (e.g., "Raymond" vs. "Reymond") appeared.

We thus merged the third party voter file with the list of future felons by creating every potential pairwise combination based on the two common and reliably consistent variables (date of birth and last

[3] In restricting the data in this manner, we can characterize this list as containing the first felony conviction entry for all felons convicted on or after January 1, 2004, or for any felon convicted prior to but released after this date. Unfortunately, we cannot rule out the possibility that those remaining were not both convicted of a felony and released from jail prior to 2004.

[4] Prior to merging the felon records with a Connecticut voter file, we removed those too young to vote in 2008, those who missed the registration deadline of one week before the election, and those without a registration date.

[5] The vender who compiled the data regularly collects Connecticut voter files, cleans the records, makes them uniform, and merges these data with vote history information from previous voter files as well as a number of other variables sold by consumer data venders. They also process their file and verify registrants' addresses using a National Change of Address filter. The voter file includes information on active registrants and for large numbers of inactive or ineligible voters. Cumulative turnout in the file is 78.5% of the statewide total in 2008.

name). After this merge, we excluded cases for which the conviction date precedes the registration date (meaning these entries, if valid matches, correspond to convicted felons registered after their incarceration). We then assigned various levels of quality and confidence to the matches of future felons to the voting file via a multi-stage process. Based on the information common to both data sets, we determined six possible scenarios in which we might consider an individual in the voter file to be the same person as an individual in the conviction file. In addition to date of birth and last name, these included (in decreasing levels of quality) (1) same first name and town of address; (2) same first name but different town; (3) first name phonetically the same but spelled differently and same town; (4) first name phonetically the same but spelled differently and different town; (5) same first initial and town but first name phonetically and grammatically different; (6) same first initial, different town, and first name phonetically and grammatically different.[6] Potential matches were machine coded for reliability and matches of lower quality were hand coded to eliminate suspect matches. Given our ability to match on address (which was not possible in the case of Pennsylvania), we prefer this more nuanced approach to the placebo matching technique employed above.

We examined each of the six match classifications separately, beginning with that in which we had the most confidence: identical first name, last name, town, and birth date. From these matches, we removed all duplicate entries.[7] Then, since we considered these the highest quality matches possible, we dropped all pairs from the other five matching scenarios that contained one of these future felons' id numbers. We then turned to the remaining matches that share the same date of birth, first name, and last name, but had different towns. We treated these cases as valid matches, and dropped both the duplicates and remaining, unchecked entries with an inmate id number from these observations.

[6] All six categories relied on matching the last name. It is conceivable, however, that an individual would have changed his or her last name after registration but before incarceration, perhaps due to marriage or for religious reasons. Unfortunately, the lack of other identifying indicators shared by the two datasets made the identification of any such cases impossible.

[7] Among the reasons for duplicates in the third party voter file is the presence of inactive and dropped entries, as well as bureaucratic errors that permit multiple entries for the same individual. In selecting which duplicate case to drop, we kept that which was an active record. When both observations were active, we retained the record with the most recent registration date. For the remaining duplicates (which all contained identical voting histories), we chose arbitrarily to keep the observation with the higher voter id number.

For the rest of the scenarios, determining proper matches required varying degrees of subjectivity. As a next step, we examined those cases with the same last name, date of birth, town, and whose first names were phonetically the same but spelled differently. We relied on the Stata command "soundex" to identify these cases. For example, the program flagged as the same combinations such as "Terry" and "Terri," "Lorenzo" and "Larenzo," and "Stephen" and "Steven." Each case that met this criterion was manually checked to ensure both a visual and audial match. After dropping duplicates and unchecked cases with the same felon id number, we repeated this process for matches that did not share the same town.

The last two matching scenarios included pairs with the same date of birth, last name, and first initial, but entire first names that were neither spelled the same nor phonetically identical. For these cases that shared towns in common, we manually compared their first names, kept the correct matches, and removed duplicates and unverified observations with matching felon identifiers. We conducted the same procedure for the pairs that had different towns. Finally, we combined those cases that matched one of the six matching criteria into a single file and dropped any existing duplicate entries.[8] This file was merged to a cleaned version of the original third party voter file,[9] and duplicate entries were again removed.[10] Given that the third party voter file was created in April of 2009, and that we do not know whether this file underwent a post-election purge, we then removed from the file any individual with a conviction date

[8] These eight duplicates were generated by matching the same observation in the voter file to either two different felons based on their inmate id number or the same felon who through a bureaucratic error was assigned two different id numbers. In three of these cases, the matches were of differing quality, so we kept the highest quality match. Two other pairs were identical except for a missing felon id number or release date, in which case we kept the complete entries. For the remaining three cases, the observations were identical and there was no obvious available manner with which to determine the match that had a greater probability of being correct. For these, we retained the entry with the earlier conviction date.

[9] This involved keeping only those eighteen or older on Election Day in 2008, cases without missing registration information, and individuals who met the registration deadline of one week before the election.

[10] Duplicates existed for those voter id numbers with an active entry in the cleaned voter file and an inactive or dropped entry that was originally matched to a future felon. For these cases, we kept the originally matched felon entry and discarded the active observation for that voter id. We were also concerned that the same person may appear in the voter file twice, having been assigned two distinct voter id numbers. As such, we treated as the same any entries that shared identical first names, last names, dates of birth, registration dates, towns, and voter histories. When we identified duplicates, we first kept the entry without a missing value for the controls used in our analyses, then kept from the remaining duplicates the entry with the higher voter identification number.

prior to May 1, 2009. The same process was used to merge our list of incarcerated individuals to a January 2013 voter file obtained from the Secretary of State's office.

Table SA1: Results of Placebo Matching Technique for Pennsylvania

		Corrections Data			Sentencing Data	
	Date of Birth	Actual	Actual - 35 Days	Actual + 35 Days	Actual	Actual - 35 Days	Actual + 35 Days
1	Exact First, Exact Middle, Exact Last, Exact DOB	34,698	37	27	19,903	8	11
2	Exact First, Fuzzy Middle, Exact Last, Exact DOB	15,766	9	6	15,768	2	7
3	Exact First, Missing Middle, Exact Last, Exact DOB	32,157	158	135	12,353	32	28
4	Fuzzy First, Exact Middle, Exact Last, Exact DOB	2,066	32	36	720	6	11
5	Fuzzy First, Fuzzy Middle, Exact Last, Exact DOB	634	8	9	474	2	5
6	Fuzzy First, Missing Middle, Exact Last, Exact DOB	2,444	188	167	936	42	40
7	Exact First, Exact Middle, Fuzzy Last, Exact DOB	1,566	101	114	554	34	30
8	Exact First, Fuzzy Middle, Fuzzy Last, Exact DOB	361	34	25	269	36	30
9	Exact First, Missing Middle, Fuzzy Last, Exact DOB	1,781	575	646	533	185	179
10	Exact First, Not Middle, Exact Last, Exact DOB	594	106	100	274	62	55
11	Fuzzy First, Exact Middle, Fuzzy Last, Exact DOB	202	114	144	66	30	38
12	Fuzzy First, Fuzzy Middle, Fuzzy Last, Exact DOB	33	25	26	21	24	24
13	Fuzzy First, Missing Middle, Fuzzy Last, Exact DOB	498	590	603	129	200	190

Table SA2: Summary Statistics for Pennsylvania Cases Used in Tables 6 and 7 Analysis

Variable	All Table 6 Cases	Sentenced To Prison	Not Sentenced To Prison
Voted in 2012 (1=yes)	0.1589	0.1648	0.1588
	[.3656]	[.3714]	[.3655]
Any prison sentence assigned (1=yes)	0.0132	1.0000	0.0000
	[.1142]	[00000]	[00000]
Registered in 2008 (1=yes)	0.4927	0.5189	0.4924
	[.5000]	[.5002]	[.4999]
Voted in 2008 (1=yes)	0.2188	0.2339	0.2186
	[.4134]	[.4238]	[.4133]
Age in years (2012)	35.8477	35.1098	35.8576
	[11.6728]	[11.5228]	[11.6746]
Age squared / 100 (2012)	14.2131	13.6518	14.2206
	[9.7574]	[9.3344]	[9.7629]
Gender=Male	0.7328	0.8641	0.7311
	[.4425]	[.3430]	[.4434]
Race=Other	0.0710	0.0312	0.0715
	[.2568]	[.1740]	[.2577]
Race=Black	0.1555	0.3363	0.1531
	[.3624]	[.4730]	[.3601]
Race=Hispanic	0.0188	0.0668	0.0181
	[.1357]	[.2500]	[.1334]
Offense Gravity Score	2.6723	5.8864	2.6293
	[1.8475]	[1.9198]	[1.8082]
Any felony conviction (1=yes)	0.1322	0.7261	0.1243
	[.3387]	[.4465]	[.3299]
Is drug offense (1=yes)	0.1684	0.4321	0.1649
	[.3742]	[.4959]	[.3711]
Guideline recommended minimum sentence (years)	0.0829	0.7197	0.0744
	[.2075]	[.7666]	[.1740]
Observations	33999	449	33550

Standard deviations in brackets