Does Incarceration Reduce Voting? Evidence about the Political Consequences of Spending Time in Prison

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The rise in mass incarceration provides a growing impetus to understand the effect that interactions with the criminal justice system have on political participation. While a substantial body of prior research studies the political consequences of criminal disenfranchisement, less work examines why eligible ex-felons vote at very low rates. We use administrative data on voting and interactions with the criminal justice system from Pennsylvania to assess whether the association between incarceration and reduced voting is causal. Using administrative records that reduce the possibility of measurement error, we employ several different research designs to investigate the possibility that the observed negative correlation between incarceration and voting might result from differences across individuals that lead both to incarceration and to low participation. As this selection bias issue is addressed, we find that the estimated effect of serving time in prison on voting falls dramatically and for some research designs vanishes entirely.

The massive expansion in the scope of the American criminal justice system over the past 50 years has generated enormous concerns about the political consequences of the development of the "carceral state."[1] One concern is that coming into contact with the criminal justice system erodes the political power of an already marginalized population by reducing political participation. If elected officials pay less attention to the views of those who do not participate, and if the political opinions of individuals who come into contact with the carceral state diverge from those of the broader population, then incarceration may systematically alter which views are represented in government.

Nearly every state prohibits at least some felons from voting, with a few states continuing to disenfranchise ex-felons even after they have completed their sentences. Starting with Uggen and Manza (2002), a substantial body of work examines how election outcomes would change absent criminal disenfranchisement. Less attention, however, has been paid to whether carceral state contact changes the participatory patterns of people who are eligible to vote. Pioneering work by Weaver and Lerman (2010) and Lerman and Weaver (2014a) theorizes that contact with the criminal justice system—which includes interactions ranging from police stops to spending time in prison—decreases political participation by depleting citizens' resources, making them distrust government, and reducing commitments to civic norms. Consistent with this theory, they present survey data showing that people who report more extensive contact with the carceral state also report less

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This research was not conducted in association with any legal proceedings or funded by any external funder. Data and supporting materials necessary to reproduce the numerical results in the article are available in the *JOP* Dataverse (https://dataverse.harvard.edu/dataverse/jop). An online appendix with supplementary material is available at http://dx.doi.org/10.1086/692670.

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).

The Journal of Politics, volume 79, number 4. Published online July 19, 2017. http://dx.doi.org/10.1086/692670
 0022-3816/2017/7904-0002$10.00

political participation. Furthermore, in line with the experience of prison life as a "total institution," Weaver and Lerman (2010) and Lerman and Weaver (2014a) find that incarceration, among all forms of criminal justice contact, is associated with the largest decrease in participation.

Building on this prior work, we estimate the extent to which incarceration causes a reduction in 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. Furthermore, assessing the participatory consequences of incarceration is 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. Finally, a negative participatory effect of incarceration could be politically consequential, because so many people are incarcerated at some point during their lives. In 2010, 15 million formerly incarcerated individuals in the United States were eligible to vote (Shannon et al. 2011), vastly outnumbering the roughly 2.6 million formerly supervised individuals who are legally prevented from voting (Uggen, Shannon, and Maza 2012, 16). If incarceration reduces participation, it could have a larger effect on electoral outcomes than explicit legal restrictions on ex-felons' voting rights, because so many formerly incarcerated people are eligible to vote.

Previous work shows that eligible voters who have been released from prison vote at much lower rates than those who have not served time in prison (e.g., Hjalmarsson and Lopez 2010). It is difficult to ascertain, however, whether the negative association between serving time and political participation reported in prior work is causal. Those who spend time in prison are different from those who do not in myriad ways that also likely correlate with political participation. If any of the many factors that jointly affect who serves time in prison and who votes are not fully accounted for, then the observed negative associations between incarceration and voting may be a mere consequence of selection bias. Because many of the same factors that predict the increased risk of incarceration (e.g., low socioeconomic status) are also associated with lower probabilities of voting, selection bias will tend to produce a negative association between incarceration and voting that is larger than the true causal effect.

In this article, we gather and analyze novel over-time administrative data on both interactions with the criminal justice system and political participation from Pennsylvania.[2] These administrative data reduce concerns about both statistical power and measurement error that limit prior studies that use survey data to estimate the effect of incarceration on voting. In contrast to prior analyses that rarely include more than a few hundred cases of self-reported incarceration, we observe thousands of individuals who were legally eligible to vote in both the 2008 and 2012 presidential elections and were incarcerated at some point in between, but not during, these two elections. Because incarceration and voting behavior are measured using administrative sources rather than self-reports, our approach also reduces concerns about correlated measurement error for these outcomes and behaviors biasing estimates of the effect of imprisonment on voting. Finally, our access to past measures of participation and other pre-incarceration characteristics allows us to control for many underlying differences between those who are incarcerated and those who are not.

We present three sets of analysis, each iteration of which seeks to further minimize unobserved pre-incarceration differences between those who are incarcerated and those who are not. Thus, to the degree to which these unobserved differences account for the negative association between incarceration and voting found in prior work, we expect each step of our analysis to provide a less biased estimate of the effect of incarceration on voting. In each case, we estimate the local-average-treatment effect (LATE) of prison on subsequent participation for the subpopulation that serves modest stints in prison (no more than four years). This subpopulation includes a substantial share of those who are incarcerated, as Bonczar et al. (2011) estimate that the median maximum prison sentence and median actual time served was 36 and 16 months, respectively, in 2009.

First, we examine both pre-incarceration and post-release voting for those who first served time in prison between 2008 and 2012. We find that individuals who spent time in prison between these two elections did not vote frequently in 2008, before going to prison, and that their turnout rate was nearly identical in 2008 and 2012. Second, among 2008 registrants, we compare the 2012 turnout of those who first served time in prison between the two elections to those who did not spend time in prison. While people who spend time in prison vote at substantially lower rates than people who do not, accounting for observable pre-incarceration differences, including past participation, substantially reduces the size of these negative estimates. Third, we compare 2012 turnout among observably similar individuals who have been convicted of a crime but who differ in whether or not their sentences included time in prison. Once we account for observable differences between those who do and do not receive prison time, we estimate that spending time in prison has almost no negative

2. We also present a more limited analysis using data from Connecticut in the online appendix.

effect on voting.[3] Importantly, this design gives us the most leverage to isolate the effect of spending time in prison on participation, because it holds fixed the other "treatments," such as arrest and conviction, that accompany being imprisoned. Of course, there are still likely to be unobserved differences between those who are sentenced to prison and those who are not. But insofar as the remaining unobserved factors that increase the likelihood a convict is sentenced to prison also predict reduced political participation, even this comparison is likely to be biased toward finding a negative effect of spending time in prison on voting.

Our findings have important implications for research on the effects of incarceration, and interactions with the criminal justice system more broadly, both for political and nonpolitical outcomes. We show that prior estimates of the negative effect of incarceration on voting appear inflated by selection bias and measurement error, a result that may also inform evaluations of the accuracy of research that uses similar designs to estimate either the effect of other forms of criminal justice contact on political outcomes or the effects of incarceration on nonpolitical outcomes.

The patterns we uncover have implications for what interactions with the criminal justice system deserve greater scrutiny. As we discuss in the conclusion, imprisonment rarely happens after someone's first encounter with the criminal justice system. Rather, it typically arises after a long series of interactions with various parts of the criminal justice system. The finding that incarceration per se does not appear to cause a large reduction in participation suggests that scholars should follow the path of recent research that examines how citizen preferences and behaviors are shaped by lower-level contact with these other elements of the state.

THE CRIMINAL JUSTICE SYSTEM AND POLITICAL PARTICIPATION

Social scientists have shown that formerly incarcerated individuals participate at low rates once their legal voting rights are restored and have identified a variety of mechanisms by which incarceration might reduce political involvement. Lerman (2013) argues that spending time in prison has particularly negative consequences for social capital, an important determinant of political participation. Several 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). Other mechanisms apply not just to incarceration but also to contact with the criminal justice system more generally.[4] The criminal justice system is the primary means by which citizens encounter the state in many at-risk communities (Weaver and Lerman 2010), and such interactions may shape attitudes toward political participation. Lerman and Weaver (2014b) describe how citizens learn that they have less standing in the social and political realms through this contact with the "carceral" state. Criminal convictions also reduce labor force stability (Western 2002), which may depress subsequent turnout.

Prior empirical research

Table 1 summarizes the previous literature regarding how contact with the criminal justice system affects turnout. While these studies consider a range of different interactions with the criminal justice system, we focus our attention in panel A on those studies that examine the relationship between incarceration and voting because incarceration is the sanction that prior work finds has the largest negative association with turnout. Table 1 clearly shows a consistent pattern: those who experience incarceration are less likely to vote than those who do not. 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. And panel B shows that studies find smaller, but still significant, decreases in turnout associated with other forms of reported interactions with the criminal justice system (e.g., being arrested but not convicted, or being convicted but not imprisoned).

These results demonstrate that people who report contact with the criminal justice system also report voting less than those who do not. The studies therefore describe a robust correlation in the data. What is less clear, however, is whether coming into contact with the criminal justice system causes people to vote less or whether these turnout differences reflect selection bias or measurement error.[5] We discuss each of these threats to interpreting the studies listed in

3. We also use these data to compare individuals sentenced to spend time in jail to those given a sentence with no incarceration, and again we find little difference in their rates of participation.

4. States differ substantially in when and how formerly incarcerated individuals regain the right to vote. Consequentially, eligible formerly incarcerated individuals may incorrectly believe that they are disenfranchised (Meredith and Morse 2014). Supporting this account, Gerber et al. (2015) show that outreach to eligible released felons can increase their registration and voting rates.

5. An additional concern with prior scholarship relates to sampling variability induced by small sample sizes. Because incarceration is an infrequent event, even large nationally representative samples typically include a small number of formerly incarcerated people. Lerman and Weaver's (2014a) data, e.g., has 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 Lerman (2010), but the surveys used in these studies provide, at most, 723 citizens who report prior incarceration.

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

Focus of Study	Data	Cross-Sectional or Panel?	Estimated Effect on Turnout (in Percentage Points)	Effective *N* (Treated Cases)
A. Effect of incarceration (relative to no criminal justice contact) on voting:				
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
B. Effects of other interactions on voting:				
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	1,795
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	Crosss-sectional	−18	114
Effect of conviction (relative to future conviction):				
Weaver and Lerman (2010)	Add Health	Cross-sectional	−5	607
Burch (2011)	Florida administrative data	Cross-sectional	No effect	3,099
	Georgia administrative data	Cross-sectional	+3	10,773
	Michigan administrative data	Cross-sectional	−1	8,841
	Missouri administrative data	Cross-sectional	+9	4,332
	North Carolina administrative data	Cross-sectional	+6	24,403

Note. Effective *N* is number of cases experiencing treatment.

table 1 as providing causal estimates of the effect of incarceration on participation.

Selection

One alternative explanation for the low rate of political participation among the formerly incarcerated is that the same circumstances or choices that eventually lead to incarceration also cause people to abstain from voting (Miles 2004). Thus, the observed association 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 also affect participation.

Previous research identifies many characteristics that jointly affect both political participation and proclivity to commit crimes. Uggen, Manza, and Thompson (2006, 295) summarize the differences between prisoners and nonprisoners: "Compared to the nonincarcerated population, prisoners have long been undereducated, underemployed, relatively poor, and disproportionately nonwhite." These same traits are also widely recognized as being associated with

lower participation in the nonincarcerated population. Many other individual traits and attitudes that are more difficult or expensive to measure have also been shown to affect participation and criminal behavior. 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; Wildeman 2010) are correlated with the propensity for future criminality and may plausibly be associated with less frequent political participation.

Empirically distinguishing between the effect of criminal justice contact on political participation and the effect of all other factors that jointly affect contact with the criminal justice system and political participation is difficult. If any of the above-noted factors that explain the risk of incarceration and also affect participation are not accounted for, then any estimated causal 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 clear that they do not control for all pertinent influences. It is for this reason that a key, and difficult-to-solve, problem of research design is finding a counterfactual comparison group whose behavior can be compared to those sentenced to prison.

All of the studies listed in table 1, except Lerman and Weaver (2014a), include only a single snapshot of observed covariates for an individual. Such cross-sectional designs are particularly vulnerable to selection concerns, because any unmeasured factor 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.

Panel studies, by contrast, use a combination of past measures of behavior (e.g., 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 by Lerman and Weaver (2014a), which is the only panel study listed in table 1. However, even in a panel setting, estimates will be biased if changes in an unobserved factor explain both decreased participation and increased likelihood of incarceration. For example, if those who later become incarcerated "fall in with a bad crowd," simultaneously deviating from their prior levels of participation and becoming 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.

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. There are a number of limitations with self-reports. In cross-sectional analysis, for example, comparing self-reported turnout across groups requires a strong assumption about the relative frequency with which people in different groups overreport voting (Bernstein, Chadha, and Montjoy 2001). If there is measurement error in the reporting of the key variables—for example, if people who are arrested understate their prior levels of criminal activity or overstate their prior levels of participation—then estimates that rely 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 models that account for the complete set of factors that jointly explain actual participation and risks of incarceration.

In the area of criminal justice research, a large literature explores the validity of self-reported measures of criminal involvement (see Thornberry and Krohn [2000, 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 nontrivial, percentage of people will fail to report their own arrests or incarceration (Maxfield, Weiler, and Widom 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.

In panel designs, measurement error is also a threat to inference. Errors in reported participation that are correlated with treatment status are especially likely to arise if data are gathered at one point in time about both past and current behavior, as individuals are systematically likely to misreport their past behavior. For example, convicted criminals may inflate their rates of past voting more than current rates of voting. When data are gathered over time, the problem can also arise if there is correlation in reported participation and reported criminal justice contact. For example, people who are comfortable reporting that they have spent time in prison may also be comfortable reporting that

they no longer vote, while individuals who do not admit to being punished may also exaggerate their current participation, which would inflate the apparent negative effect of spending time in prison on voting. More generally, any difference in the meaning of a measure across groups (e.g., when someone compares two cohorts who voted in two different elections) may create subtle differences between treatment and control groups.

Do selection and measurement error matter?

Burch (2011) provides suggestive evidence that accounting for selection and measurement error may dramatically alter the estimated relationship between criminal justice contact and participation. Her study examines the effect of a conviction, rather than incarceration, on 2008 turnout in five states. As table 1 makes clear, this is the only study that measures both contact with the criminal justice system and participation using administrative records. Burch conducts a cross-sectional analysis that compares the political participation of the population that has been convicted and released to the population that will be convicted in the future. Burch shows that in three of the five states included in her study, people who were convicted before the election were significantly more likely to vote in 2008 than people who were convicted after the 2008 election.[6] While this contrasts with the expected relationship if contact with the criminal justice system reduces turnout, this estimate may not be fully informative because the two groups are not fully comparable.[7] For example, when someone is sentenced may relate to whether they are legally eligible to participate in these states.

RESEARCH DESIGNS

In light of the selection concerns discussed in the previous section, we use three different research designs to estimate the effect of incarceration on participation. In all three approaches we use administrative records of interactions with the criminal justice system and participation to reduce concerns about measurement error. Each successive approach further reduces the expected unobserved differences between those who experience incarceration (i.e., "treated") and those who do not (i.e., "control").

Approach 1: Compare formerly incarcerated individuals to their pre-incarceration selves

Our first approach examines change over time in participation for individuals who first spend time in prison between the 2008 and 2012 elections. By comparing the same individual at two points in time, an advantage of this panel approach is that 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. However, if individuals change in ways that both increase their chances of being incarcerated and reduce their propensity to participate in the future, this design will not account for these differences. Additionally, this analysis does not account for other factors that change over time, like electoral context and demographic changes (e.g., becoming older), which may also affect participation.

Approach 2: Over time, compare formerly incarcerated individuals to observably similar nonincarcerated individuals

Our second approach compares the 2012 participation of individuals who first spent time in prison between the 2008 and 2012 elections to observably similar nonincarcerated individuals. In this analysis, we restrict our sample to those who were registered to vote in 2008 and account for preexisting differences among this population with characteristics and behaviors measured in 2008, before anyone goes to prison. We then compare the 2012 political participation of registrants who spend time in prison between these elections to those registrants who were not incarcerated. Benchmarking the 2012 participation of the formerly imprisoned population against a group of observably similar individuals who were never incarcerated allows us to measure the effect of changing context and aging under the assumption that the effect of these factors on voting is similar for 2008 registrants who were and were not first incarcerated and then discharged between 2008 and 2012.

While this comparison is most similar to prior work, we note two limitations. First, it requires excluding the large proportion of incarcerated individuals not registered in 2008 (i.e., before going to prison), because we lack information about comparable nonregistered individuals who are not incarcerated between these elections. Second, we likely cannot account for all selection bias with the sparse set of controls contained in the 2008 voter file. Thus, if 2012 turnout in the formerly incarcerated population is estimated to be lower than

6. A robustness check reported by Weaver and Lerman (2010) raises similar concerns about selection. Figure S2 in their Supporting Information compares the reported participation of people who report having been convicted of a crime to those who have not reported having been convicted of a crime but who will in a later wave of the survey. Turnout rates are statistically indistinguishable between these two groups after controlling for differences in their observable characteristics.

7. Burch (2011) also finds that those 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.

in the nonincarcerated population, this estimate of the effect of incarceration is still likely upwardly biased by the unobserved differences between these two groups.

Approach 3: Compare formerly incarcerated individuals to individuals convicted of crimes but not incarcerated

Our third approach compares the 2012 participation of convicts who were given different sentences. This analysis, like our first approach, holds constant the fact that everyone in this sample is convicted of a crime. Because only some individuals in this sample are sent to prison, however, we can account for the effects of electoral context and changing demographics on voting using the behavior of convicts given different sentences. Overall, although we must still take additional steps to account for pre-existing differences between convicts sent to prison and those given other sentences, these two groups are likely more similar on pertinent unobserved characteristics than when comparing convicts to those not convicted of a serious crime.

We believe our third approach minimizes concerns about selection bias that arise when comparing convicts to other citizens. However, it may still produce biased estimates if there are unobserved factors that explain why some convicts are sentenced to prison and other observably similar convicts are sentenced to probation and those unobserved factors also affect political participation. For example, criminals who are known to have more stable employment or stronger ties to their communities may be more likely to both receive probation and vote. Thus, even these estimates may still overstate any estimated demobilizing effects of incarceration.

DATA

Pennsylvania is an attractive study location for both theoretical and data quality reasons. To isolate the causal effect of incarceration on voter turnout, we must account for other factors that might lead people who have been incarcerated to vote at low rates apart from the experience of having been in prison. One possibility is that released convicts might not vote because they believe they are legally disenfranchised (see footnote 4). In Pennsylvania, convicted felons lose the right to vote only while incarcerated. Although some nonincarcerated ex-felons in Pennsylvania certainly believe that they are ineligible to vote (Meredith and Morse 2014), there is likely to be less confusion in Pennsylvania than in states that condition voting rights on post-release supervision status or the nature of the crime of conviction. Furthermore, as a presidential "battleground" state, the pressure for campaigns to actively register and mobilize potential voters likely increases the chances that released convicts are made aware of their participation rights.

Sentencing data

We obtained records from the Pennsylvania Sentencing Commission (PSC) for all individuals convicted of a crime in state court and who committed an offense on or after November 6, 2008, and were sentenced by 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 of each person convicted of a crime in state court. Additionally, they include information about the offense 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. The PSC data also 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 sanctions. 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.[8] 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.

While the sentencing data contain 102,368 observations, most of our analyses focus on a restricted sample of 34,231 individuals. Some of these restrictions are made because of

8. Note that some individuals sentenced to prison may not actually serve any time in prison, while others who are not sentenced to prison will ultimately spend time in prison if they recidivate or violate the terms of their probation. Simply comparing the political participation of those who spent time in prison to those who did not could yields a biased estimate of the effect of imprisonment if the same behavior that causes people to end up in prison despite initially more lenient sentences is also associated with reduced political participation (e.g., people who violate their terms of probation may be sent to prison and also be less likely to participate). Thus, we use whether someone was sentenced to prison, rather than whether someone spent time in prison, as our treatment indicator. We show later in table 4 that people sentenced to prison were about 75 percentage points more likely to be first admitted to prison between 2008 and 2012 than people who received a nonprison sentence. Thus, our estimates are analogous to intent-to-treat effects, where being sentenced to prison is associated with about a 75% increase in the probability that someone first spent time in prison between the two elections.

data limitations. We drop cases with convictions on multiple counts because variation in sentencing may reflect differences in unobserved severity in the subsidiary counts. We also drop individuals who were not yet 18 years old before the 2008 presidential election, because we cannot observe preconviction participation. Other restrictions are made because we believe that making them gives us the maximum leverage to identify the effect of incarceration on political participation. We focus on cases where someone was first convicted of a crime in Pennsylvania after the 2008 election. We do this to minimize the chances that included individuals were ineligible to vote in the 2008 election and because prior scholarship suggests that one's first incidence of incarceration would be most likely to disrupt participation.[9] To identify this subsample of first-time offenders, we use the PSC's coding of each individual's prior record score (PRS) and include only individuals with a PRS of zero. We also drop people convicted of crimes with an OGS of 12 or more. We do this because everyone convicted of such a crime was sentenced to prison, and thus we cannot compare the political participation of such individuals to similar people who received a nonprison sentence.

Because our restricted sample includes people convicted of less serious crimes in 2010 or earlier, most people sentenced to prison will be discharged from their initial sentence by the 2012 election. But some people in the restricted sample were in prison during the 2012 election, and thus were ineligible to vote, either because they had an especially long first sentence or because of a subsequent infraction. We therefore create a variable using the corrections data described below indicating whether someone was incarcerated in state prison during the 2012 presidential election. Our baseline analysis keeps these individuals in the analysis, because this is a post-treatment outcome and we want to avoid conditioning on a variable that could introduce post-treatment bias. But we also run secondary analysis that drops these cases from the analysis to explore the robustness of our results to the exclusion of these cases.

Corrections data

We supplement our sentencing data with records obtained from the Pennsylvania Department of Corrections (PDC) for the 204,254 people incarcerated in Pennsylvania prisons since 1990. The PDC data include offenders' full names, dates of birth, gender, race, and unique identifiers that allow us to link each individual's prison experiences over time. We focus on the 12,284 individuals who first served time in a Pennsylvania prison after the 2008 presidential election and who were discharged before September 30, 2012. As discussed in the previous subsection, we also link PSC data to these records to measure which people in the sentencing data were imprisoned during the 2012 presidential election.

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, or purged, from the voter file when a voter is no longer an active registrant. Because convicted felons' records are often purged, we use voter files collected close to each election. Specifically, we use a PVF from April 2009 to measure registration and turnout in the 2008 presidential election and a PVF from December 2012 to measure registration and turnout in the 2012 presidential election.[10] Individuals retain common unique identifiers in the state voter files across elections even when they change or update their registration.

Measuring the turnout behavior of people in the sentencing and corrections data sets requires that we link observations in those sources to the voter file. There is no common unique identifier across the sentencing, corrections, and voting data sets. Additionally, neither the sentencing data sets nor the corrections data sets contain addresses. Thus, we follow Meredith and Morse (2015) 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 online appendix.

RESULTS

Approach 1

We first use the corrections data to compare the 2008 and 2012 participation of Pennsylvania residents first imprisoned and then released between these two elections. 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 rates to be substantially higher in the 2008 presidential election than in 2012.

9. This selection rule does not preclude the possibility that some individuals may have been previously incarcerated in another state.

10. The April 2009 voter file is the first statewide file with complete 2008 turnout, and it 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.

Table 2. 2012 and 2008 Voting and Registration among Pennsylvania Residents Incarcerated in State Prison after 2008 Election and Released before 2012 Election: Means and Standard Deviations

	Registered 2012 (1 = Yes) (1)	Voted 2012 (1 = Yes) (2)	Registered 2008 (1 = Yes) (3)	Voted 2008 (1 = Yes) (4)	Δ Registration 2012–2008 (5)	Δ Voting 2012–2008 (6)
Full sample (N = 12,284)	.439	.144	.441	.135	−.002	.009
	(.496)	(.351)	(.497)	(.342)	(.428)	(.394)
Restricted sample (N = 2,482)	.436	.148	.438	.152	−.002	−.004
	(.496)	(.355)	(.496)	(.359)	(.415)	(.390)

Note. The restricted sample includes people admitted to prison on or after January 1, 2010 and discharged on or before December 31, 2011. Standard deviations are in parentheses.

In contrast to this expectation, the full sample data of table 2 show modest change in the participatory patterns of the 12,284 people first imprisoned in Pennsylvania after the 2008 election and released before the 2012 election. Consistent with previous literature, columns 1 and 2 show that the formerly incarcerated individuals participate at low rates after going to prison: 43.9% of these released prisoners were registered to vote in 2012, and 14.4% voted in the 2012 election. However, columns 3 and 4 show that this group also registered and voted at low rates in 2008: 44.1% 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 0.2 percentage points less likely to be registered but 0.9 percentage points more likely to vote after going to prison than before doing so.

One concern with the results presented in the previous paragraph is that while all of these people were nominally eligible to vote, some may not have voted because of issues associated with their criminal conviction. For example, someone could have been undergoing prosecution during the 2008 election or have been discharged so close to the 2012 election that they had not had a chance to register. To limit such concerns, the restricted sample portion of table 2 focuses on a subset of ex-prisoners who were first incarcerated more than a year after the 2008 election and discharged at least 10 months before the 2012 election. Participatory patterns are similar in this subpopulation, suggesting that the aforementioned concerns are not a substantial threat to inference.

In contrast to Lerman and Weaver (2014a), who find a large decline in self-reported turnout after people's first reported spell of incarceration, we find little evidence that turnout rates change after someone's first spell of imprisonment. Instead, it is clear that released felons already voted at much lower rates than the general population prior to going to prison. What remains unclear is how much we should expect turnout to have changed within this subpopulation absent a spell of incarceration. It could be, for example, that young people voted much more in 2012 than in 2008, so finding a small decline in participation for this group masks the fact that similar individuals would have experienced much larger increases in participation absent experiencing incarceration. In light of this concern, we turn to comparing among 2008 registrants the 2012 participation of formerly imprisoned individuals to an observably similar set of 2008 registrants.

Approach 2

We next use our matched PDC and PVF data to estimate how experiencing imprisonment in Pennsylvania affects participation. We compare the 2012 participation of formerly incarcerated individuals who were registered in 2008 to other 2008 registrants. The sample for this analysis is the 8,544,483 people who were registered in Pennsylvania in 2008, 5,414 of whom were imprisoned and then released between 2008 and 2012. The benefit of this approach is that we can account for the changing effects of demographics and electoral context.[11]

Table 3 shows that the relationship between incarceration and turnout attenuates dramatically once we account for a relatively limited set of pre-incarceration differences in the characteristics of the imprisoned and nonimprisoned populations. Column 1 of table 3 reveals that formerly incarcerated individuals were 29.9 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 26.5 points ($p < .01$).

11. This benefit comes at the aforementioned cost of having to discard previously unregistered released prisoners from the analysis because we lack an enumeration of the comparable population of previously unregistered nonprisoners.

Table 3. 2012 Pennsylvania Voting among 2008 Registrants by Incarceration Status

	Bivariate Regression (1)	ZIP Code Fixed Effects (2)	Demographics (3)	Prior Participation (4)	Matched Pair Subsample (5)
Formerly incarcerated (1 = admitted to state prison after November 5, 2008, and released by September 30, 2016)	−.299***	−.265***	−.229***	−.051***	−.043***
	(.006)	(.006)	(.006)	(.006)	(.008)
Voted 2008 (1 = yes)				.552***	
				(.000)	
Age in years (2012)			.002***	.001***	.084**
			(.000)	(.000)	(.036)
$(Age)^2/100$			.000	.000	
			(.000)	.000)	
Registered Democrat (1 = yes)			.120***	.046***	
			(.001)	(.000)	
Registered Republican (1 = yes)			.143***	.071***	
			(.001)	(.000)	
Gender = male			−.017***	−.002***	
			(.000)	(.000)	
Gender = unknown			−.137***	−.087***	
			(.000)	(.000)	
Constant	.550***	.550***	.384***	.091***	−2.744**
	(.000)	(.000)	(.001)	(.001)	(1.311)
Observations	8,544,483	8,544,483	8,544,483	8,544,483	10,758
Number treated to prison	5,414	5,414	5,414	5,414	5,379
R^2	.000	.051	.082	.324	.610
ZIP code fixed effects		2,099	2,099	2,099	
Matched pair fixed effects					5,379

Note. Cell entries are ordinary least squares estimates with robust standard errors in parentheses. Dependent variable for all models is a binary indicator for whether an individual voted in the 2012 US general election (yes = 1, no = 0).

* $p < .10$.

** $p < .05$.

*** $p < .01$.

In column 3, we add parametric controls for age, party of registration, and gender, which further shrinks the estimated effect to 22.9 points ($p < .01$). In column 4, we also control for pre-incarceration participation by adding to our previous specification an indicator for voting in 2008. This substantially reduces the apparent negative effect of incarceration on voting to 5.1 points ($p < .01$). The estimated effect is further reduced to 4.3 points in the column 5 specification, where we use a matched pairs design in which each formerly incarcerated individual is matched to the nonincarcerated registrant who is closest to them in age (within 2.5 years) and shares the same 2008 participation history, party of registration, gender, and ZIP code.

These findings demonstrate the importance of accounting for selection when estimating the effect of incarceration on participation. As we make those who serve prison time observably more similar to those who do not, our estimates of the negative effect of incarceration on voting decline substantially. 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 our pre-treatment controls. What remains uncertain, however, is whether the 5-point difference we estimate accurately identifies the causal effect of imprisonment on future participation among prior registrants, or if this estimate would shrink further if we could obtain additional measures of pre-incarceration differences between these two groups. In addition to these sorts of static omitted factors that explain future criminal behavior and low levels of participation, there might also be factors that change over time and explain both the likelihood of incarceration and deviations from past levels of political participation. In light of these concerns, we now turn to our comparison of different groups of convicts, which further minimizes unobserved pre-

incarceration differences between those who experience time in prison and those who do not.

Approach 3

In this section, we use the data set created by merging sentencing data to voter records to compare the political participation of two groups of convicts: those who are sentenced to prison and those who do not receive prison time as part of their sentence. This approach simultaneously addresses many of the weaknesses of both earlier approaches. It allows us to hold constant that a person has been found guilty of a crime and instead exploits variation in the sentences convicted criminals are assigned. We can then use the behavior of convicts given nonprison sentences to account for the changing effects of electoral context and demographics on voting.

Table 4 shows that turnout declines modestly between 2008 and 2012 among both those who were sentenced to prison and those who received a less severe sentence. The top portion of table 4 presents summary statistics for our restricted sample of 34,231 individuals who were first convicted of a single count of a less serious crime. Column 1 shows that 15.9% of people ($N = 603$) sentenced to prison voted in 2012. This is a 7.8 point decline from their turnout rate in 2008. Column 2 shows that the share of people sentenced to something other than prison ($N = 33{,}628$) who voted in 2012 was also 15.9%. But because slightly fewer of these individuals voted in 2008, the turnout decline in this group was only 6 points. The fact that turnout is relatively similar between these two groups is notable, because about 79% of those sentenced to prison first spent time in prison between these two elections, as compared to about 3% of those who received some other sentence.

Two other patterns in table 4 are worth noting. First, the type of nonprison sentence that someone receives is not systematically related to 2012 turnout or the change in turnout between 2008 and 2012.[12] Second, the patterns described in the restricted sample are also generally present in the full sample: 14.8% of people sentenced to prison voted in 2012, as compared to 15.3% of people sentenced to some other punishment. These figures represent 6.2 point and 6.1 point declines from 2008 turnout among people sentenced to prison and people sentenced to something else, respectively. One downside of limiting our analysis to the restricted sample is that, by construction, we are estimating a local average treatment effect of prison for a subset of people who have not previously gone to prison, have committed less serious crimes, and have served shorter prison sentences. But observing similar patterns in the full sample suggests that our results may also apply to the broader population of people who spend time in prison.

Interpreting the patterns in table 4 as the effects of different punishments on voting is complicated, however, by differences in the demographic and other characteristics (e.g., crimes committed) of people sentenced to prison instead of other punishments. Table SA2 in the online appendix (tables SA1–SA6 are available online) shows that people sentenced to prison have committed more serious offenses, are more likely to have committed a felony, and have a higher guideline recommended minimum sentence than those sentenced to something else. Additionally, those who are sentenced to prison are younger, more likely to be male, and more likely to be black or Hispanic than those who receive a more lenient sentence. Because these factors could have an independent effect on participation (or be correlated with factors that affect participation), we next present multivariate analyses accounting for these differences. Specifically, we estimate OLS regression models predicting 2012 voting as a function of being sentenced to prison while controlling for a variety of observable features of convicts and their past participation.

Table 5 shows that we continue to find little relationship between being sentenced to prison and turnout after including additional controls. Column 1 replicates the finding from table 4 that people sentenced to prison rather than some other punishment voted at the same rate in 2012. In columns 2–5, we continue to find no relationship between being sentenced to prison and 2012 participation when we control for observable differences between these two populations. Specifically, this relationship remains substantively small and statistically insignificant when we control for pretreatment registration and participation (col. 2), demographic variables like age, gender, race, and county of residence (col. 3), and the severity of the crime, as well as the recommended minimum sentence (col. 4). The complete model in column 5 shows that people who were sentenced to prison were 0.6 percentage points less likely to vote in 2012 (NS) 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 about half of a percentage point, and any effect larger than 3.7 percentage points falls outside the 95% confidence interval.

These specifications use regression adjustments to account for the observed differences between those sent to prison and those not sentenced to prison. Column 6 shows that we obtain similar results when we instead employ matching to pair

12. An exception is the small number of people given State Intermediate Punishment, whose 2008 turnout is less than half of any other group but whose 2012 turnout increases.

Table 4. Summary Statistics by Group for 2012 and 2008 Voting for Pennsylvania Convicts Sentenced to Different Punishments

Variable	State Prison (1)	Nonprison Sentence (2)	State Intermediate Punishment (3)	County Jail (4)	Restrictive Intermediate Punishment (5)	Probation (6)	Other Restorative Sanction (7)
Restricted sample:							
Voted in 2012 (1 = yes)	.159	.159	.161	.159	.154	.161	.158
	(.366)	(.366)	(.374)	(.366)	(.361)	(.367)	(.365)
Change in turnout 2012–2008	−.078	−.060	.065	−.058	−.063	−.060	−.066
	(.426)	(.406)	(.359)	(.404)	(.410)	(.405)	(.421)
Corrections record pre-2012 (1 = yes)	.874	.041	.677	.047	.023	.041	.047
	(.332)	(.197)	(.475)	(.211)	(.151)	(.198)	(.211)
Change in corrections record 2012–2008	.786	.031	.677	.033	.017	.033	.029
	(.410)	(.172)	(.475)	(.178)	(.130)	(.178)	(.168)
Full sample:							
Voted in 2012 (1 = yes)	.148	.153	.165	.154	.153	.152	.146
	(.355)	(.360)	(.372)	(.361)	(.360)	(.359)	(.353)
Change in turnout 2012–2008	−.062	−.061	−.029	−.059	−.065	−.063	−.060
	(.407)	(.407)	(.4056)	(.408)	(.412)	(.405)	(.410)
Corrections record pre-2012 (1 = yes)	.952	.143	.893	.178	.106	.116	.155
	(.213)	(.350)	(.310)	(.383)	(.308)	(.320)	(.362)
Change in corrections record 2012–2008	.642	.070	.753	.081	.045	.063	.070
	(.479)	(.255)	(.432)	(.272)	(.208)	(.242)	(.254)
Observations in preferred sample	603	3,3628	31	12,075	5,779	15,014	727
Observations in full sample	9,993	92,375	279	37,868	12,349	39,775	2,102

Note. Cell entries are means with standard deviations in parentheses.

individuals sent to prison with similar individuals not sent to prison. We match exactly on 2008 participation and registration, gender, race, and type and severity of the convicted crime. If there are multiple matches, we select the person closest in age, while also requiring that the person be no more than 2.5 years older/younger than the incarcerated person and have a guideline-recommended sentence that differs by no more than four months. We find control observations that satisfy these criteria for 381 of the 603 cases in which people were sentenced to prison. In this analysis, we estimate that people sentenced to prison were 0.1 points more likely to vote (NS) than observably similar people sentenced to some other form of punishment.

Some people included in the sample for columns 1–6 may have wished to vote but were legally barred from voting because they were incarcerated for a felony during the 2012 election. Because this is more likely for those sentenced to prison, leaving these people in the sample could potentially bias us toward finding a negative effect of a prison sentence on 2012 turnout. Column 7 shows that when we exclude people who were incarcerated in state prison during the 2012 election, and therefore legally prevented from voting, the estimated effect of being sentenced to prison on voting is positive and statistically insignificant.

We also investigate in the online appendix whether being sentenced to serve time in a county jail in Pennsylvania diminishes participation compared to receiving a less serious sanction. Many of the mechanisms that previous research highlights for how time in prison might reduce participation could also cause people to vote less after spending time in county jail (see, e.g., White 2016). And because the number of people sentenced to jail is much larger than the number of people sentenced to prison, we can estimate this relationship with much greater statistical precision. Table SA3 shows that

Table 5. 2012 Voting for Pennsylvania Convicts Sentenced to Prison or Another Punishment between 2008 and 2012 Elections

	Bivariate Regression (1)	Past Participation (2)	Demographics (3)	Crime Characterstics (4)	Complete Model (5)	Matched Pairs (6)	Excluding Potentially Imprisoned 2012 (7)
Any prison for any offense? (1 = yes)	.000	−.008	.003	−.003	−.006	.001	.008
	(.015)	(.014)	(.015)	(.018)	(.016)	(.025)	(.019)
Registered in 2008 (1 = yes)		.071***			.072***		.071***
		(.004)			(.004)		(.004)
Voted in 2008 (1 = yes)		.362***			.362***		.362***
		(.007)			(.007)		(.007)
Age (years) 2012			.000		.001	.046	.001
			(.001)		(.001)	(.039)	(.001)
$(\text{Age})^2/100$ 2012			.000		−.001		−.001
			(.001)		(.001)		(.001)
Gender (1 = male, 0 = female)			−.003		−.003		−.003
			(.005)		(.004)		(.004)
Race = black			−.003		−.004		−.004
			(.006)		(.005)		(.005)
Race = Hispanic			.001		.008		.012
			(.015)		(.013)		(.013)
Race = other			.002		.000		−.002
			(.008)		(.007)		(.007)
Guideline recommended minimum sentence (years)				.013	.003	.933*	−.013
				(.015)	(.013)	(.542)	(.015)
Any felony conviction (1 = yes)				−.004	.002		.000
				(.008)	(.007)		(.008)
Constant	.159***	.045***	.150***	.157***	.02	−1.991	.023
	(.002)	(.002)	(.023)	(.003)	(.021)	(1.407)	(.021)
Observations	34,231	34,231	34,231	34,231	34,231	762	33,276
R^2	.000	.220	.002	.000	.222	.009	.221
Proportion voting 2012	.159	.159	.159	.159	.159	.154	.159
Proportion voting 2008	.219	.219	.219	.219	.219	.213	.219
Number treated to prison	603	603	603	603	603	381	396
County fixed effects	No	No	Yes	No	Yes	No	Yes
Offense gravity score fixed effects	No	No	No	Yes	Yes	No	Yes
Matched pair fixed effects	No	No	No	No	No	Yes	No

Note. Cell entries are ordinary least squares estimates with robust standard errors in parentheses.

* $p < .10$.

** $p < .05$.

*** $p < .01$.

people sentenced to jail vote at similar rates as people sentenced to less serious sanctions. The complete model presented in column 5 shows that people sentenced to jail are 0.2 percentage points more likely to vote (NS) 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. In column 6, when we estimate results using the same matching algorithm described above, our estimate is 0.1 points.

Overall, the results presented in this section provide the most credible evidence that spending time in prison does not cause a large reduction in turnout. Our largest estimate is −0.8 points, which is 84% 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 (table 3, col. 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, the estimates from table 5 are very similar to the simple over-time comparison presented in table 2, which showed that people turned out at similar rates before and after their first incarceration spell.

External validity

Are formerly incarcerated individuals in Pennsylvania representative of the broader population of people who have previously spent time in prison? One way to examine this is to compare the turnout behavior of recently discharged ex-prisoners in Pennsylvania to similar populations in other states. Table 2 shows that about 14% of recently discharged ex-prisoners voted in Pennsylvania in 2012. Meredith and Morse (2014, 2015) report estimated 2012 turnout rates for similar populations in Iowa, Maine, New York, North Carolina, and Rhode Island. Those estimates range between 8% and 18%, which is reassuring because it means recently discharged ex-prisoners in Pennsylvania vote at roughly the same rate as ex-felons in these other states.

To further explore the external validity of our Pennsylvania results, we also replicated approaches 1 and 2 for Connecticut.[13] These analyses, which are discussed in more detail in online appendix 2, again demonstrate that released convicted criminals voted at much lower rates than the general population prior to going to prison. Table SA4 shows that 10.8% of people who were first incarcerated in Connecticut after the 2008 election, but released before the 2012 election, voted in 2008. So while turnout increased by about 1 point post-incarceration in Pennsylvania, it declined by about 4 points in Connecticut. It should be noted that average turnout among all registrants declined by 4 points in Connecticut between these two elections, while only declining by 1 point in Pennsylvania.

Pre-treatment observables also explain less of the difference in the 2012 turnout of 2008 registrants who did and did not go the prison between the two elections in Connecticut. Controlling for 2008 participation, age, gender, party of registration, and ZIP code accounts for about 22 percentage points, or about 60%, of the 37 percentage point difference in 2012 turnout between these two groups. But this leaves almost a 15-point difference in the turnout rates of the formerly incarcerated registrants and observably similar nonincarcerated registrants when all of the controls are included. As a point of comparison, we estimated only a 5-point difference in Pennsylvania.

Combining the results described in the two previous paragraphs, turnout appears to decline more noticeably after a first incarceration spell in Connecticut than it does Pennsylvania. Looking back at table 1, previous work estimated that, holding many characteristics fixed, reported turnout is between 11 and 29 percentage points lower if someone reported a previous spell of incarceration. Using all three of the approaches we apply in Pennsylvania and one of the two approaches we apply in Connecticut, we can reject the null hypothesis at conventional levels that incarceration causes an 11-point decline in participation. And even when we apply approach 2 in Connecticut, our estimated effect is still on the low end of the estimates reported in prior work using self-reported turnout and incarceration status, despite only holding fixed a subset of the characteristics for which previous work controls.

At the same time, observing somewhat different patterns in Connecticut and Pennsylvania suggests that state context may still shape how incarceration affects turnout. Given Gerber et al.'s (2015) finding that mobilization is important for getting ex-felons to vote, particularly among the subset that participated previously, it would be useful to investigate whether the lower rate of participation among released prisoners in Connecticut is because that state was less contested in the presidential election than Pennsylvania, a characterization which is supported by the fact that average turnout rates declined more in Connecticut than in Pennsylvania. If context is important, applying approach 3 to Connecticut (if the data were available) would also allow us to understand how context is important. Does a more contested electoral environment mobilize released prisoners or mobilize the types of people most at risk of being incarcerated, regardless of whether they have been incarcerated? It

13. We could not replicate approach 3 because we did not have access to similar sentencing data.

would also be promising to examine, in states where released prisoners can vote, how state electoral context affects the rates at which released prisoners rejoin the pool of registrants and voters.

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 article 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 to estimate the effect of incarceration on voting.

Our most striking finding is that it appears that spending time in prison does not have large negative effects on subsequent participation. We summarize the results from each of our three approaches in table 6, which explains the explicit comparisons made in each set of analysis and the estimated effect of incarceration on voting from our most credible specification using that approach. Former prisoners do vote at low rates. For example, focusing on those who were registered prior to experiencing incarceration, we find released prisoners are much less likely to vote than the general population of registrants. However, as we include additional covariates that account for observable differences between released prisoners and other registrants, such as past participation and area of residence, these estimates shrink dramatically. Thus, our most persuasive estimate that compares first-time incarcerated prisoners to all registrants is that spending time in prison reduces subsequent voting by 5 percentage points in Pennsylvania.

As we note, however, even these estimates are likely too large to represent the effect of incarceration on all incarcerated individuals, both because they are restricted to only those released prisoners 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. For example, we do not observe, and hence cannot control for, pre- or post-incarceration criminal behavior, income, or educational differences between released prisoners and the general population. Accounting for these differences would likely further shrink the estimated difference in turnout between these groups.

The most credible estimates we present of the effect of incarceration on voting use sentencing data to compare different groups of convicts. These data allow us to account for a broad range of individual-level differences, including the severity of the crimes individuals committed and their prior criminal and voting records. We examine how 2012 participation differs for those criminals who are sentenced to prison compared to similar convicts given sentences other than time in prison. In this more comparable group of individuals, our most complete model estimate is that going to prison reduces participation by 0.6 percentage points when we use a parametric regression model (table 5, col. 5) or increases it by 0.1 points when we preprocess using matching (table 5, col. 6). Similarly, as we show in the online appendix, the effect of being sentenced to jail rather than some lesser punishment appears to either slightly increase or have no effect on participation.

This point also helps illustrate a general implication of our work for research on the myriad effects of incarceration. While we have focused on whether incarceration reduces voting, scholars are broadly interested in how the experience

Table 6. Preferred Estimate of Effect of Incarceration on Voting by Approach

	Comparison	Model Specification	Estimated Effect of Incarceration on Voting (%)
Approach 1	Among people admitted to and released from prison between the 2008 and 2012 elections, compare 2012 to 2008 turnout	table 2, column 6 (full sample)	.9
Approach 2	Among 2008 registrants, compare 2012 turnout between those never incarcerated and those admitted to and released from prison between the two elections (after accounting for 2008 participation and a variety of other pre-incarceration factors)	table 3, column 4	−5.1
Approach 3	Among people convicted of a less serious crimes and who had never previously gone to prison, compare 2012 participation of those sentenced to prison to those not sentenced to prison (after accounting for 2008 participation and a variety of other pre-incarceration factors)	table 5, column 5	−.6

of incarceration affects different outcomes such as earnings, marital prospects, and recidivism (e.g., Hjalmarsson 2009; Pager 2003; Western 2002). The approaches we use here show the difficulty of providing credible causal estimates 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 these research designs would reveal persuasive evidence of the negative effects of incarceration.

Several important caveats should be highlighted. Our estimated effect of prison time is for a particular group of subjects in Pennsylvania: those who have no prior record score and are convicted of a single count of a crime that had an offense gravity score such that prison was an option but not a requirement. Additionally, there is sufficient sampling error in our estimates of the effects of prison that we cannot reject there being small demobilizing effects of going to prison if our model completely eliminates selection bias. Finally, our analysis should not be taken to minimize the often traumatic effects of serving time in prison, a subject that is well beyond the scope of our analysis

One reason that incarceration may not significantly reduce political participation is that it is an outcome that often follows a long series of interactions with the criminal justice system. To illustrate how prison is often the last in a series of increasingly punitive interactions with the state, we estimated the number of times that first-time inmates had been arrested prior to being incarcerated using the 2004 Survey of State Inmates. Among respondents who had never previously been incarcerated for a major infraction, only about 24% had never been previously arrested. Moreover, 53% of this population had previously been sentenced to probation, and 38% had been arrested three or more times.[14] Our work therefore suggests a fruitful area of research may be trying 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 phe last three decades of aggressive crime control have affected entire communities (see Burch 2013; Lerman and Weaver 2014a). 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 implies that criminal justice contact affects entire communities. Of course, our empirical approach highlights that great care must be undertaken when evaluating the effects of these community-level interventions, because it is clear that the communities most affected by aggressive crime control efforts are different in significant ways from those that are not similarly targeted.

There are also broader implications of this work for political science and the social sciences more generally. While a great deal of literature has sought to understand the effects of different government 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. 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 general warning for scholars that the assumption that assignment to treatment is ignorable (random) given typical observable factors is highly suspect. Such problems may be exacerbated when relying on self-reported measures of program participation and attitudes or other behaviors, which introduce the possibility of correlated measurement error. More subtly, this pattern suggests an important diagnostic tool for researchers about the "most credible" estimates presented in existing research. If treatment effects tend toward zero as the potential for bias declines, the "best" estimates may still be too large (too far from zero) unless the researcher has fully accounted for all of the ways in which those who receive treatment are different from those who do not.

14. See table SA6 in the online appendix for a full presentation of these results.

ACKNOWLEDGMENTS

We thank Traci Burch, Ellen Donnelly, Alec Ewald, Al Fang, Seth Hill, John MacDonald, Michele Margolis, Michael Morse, Vesla Weaver, and seminar participants at University of California, Los Angeles, University of California, San Diego, the University of Chicago, Yale University, and the 2012 American Political Science Association annual meeting for helpful comments and advice.

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