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# Political Participation Amid Mass Incarceration

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## Keywords

mass incarceration, political participation, voting, policing, felony disenfranchisement

## Abstract

Contact with the carceral state—ranging from police stops to prison time—is a frequent experience in the United States, particularly in communities marginalized on the basis of race and class. In recent years, political scientists have sought to measure the impacts of these encounters on individuals' and communities' political engagement. This review describes the main sources of evidence in this literature and what we learn from them. I present a series of stylized facts about the carceral state and political behavior, highlighting places where we know a great deal (such as the relative underrepresentation of people with criminal convictions among voters) and places where more work is needed (such as nonvoting participation and community spillovers). Then, I discuss policy proposals that seek to mitigate the political impacts of the carceral state, and what is and is not yet known about what they might accomplish.

## 1. INTRODUCTION

In the United States, contact with the criminal legal system is extraordinarily common. Shannon et al. (2017) estimate that 8% of adults have a felony conviction, with that percentage rising to 33% for Black men. Nearly half of those people have spent time in prison. Millions more have experienced arrests or charges that did not yield a felony conviction: For example, recent estimates put the number of misdemeanor cases filed in the United States annually at 13.2 million (Stevenson & Mayson 2018).

These experiences with the punitive face of government are both widespread and life-altering. They can mean the loss of a job, the weakening of family relationships, and the social stigma of a criminal conviction (Middlemass 2017). And they are a powerful site of learning about what to expect from government (Justice & Meares 2014). But such learning experiences are not evenly distributed across society; they are especially concentrated in what Soss & Weaver (2017) term “race-class subjugated communities” (Western 2006).

In a country where carceral contact is both frequent and racially disparate, it seems natural that political scientists should ask what this carceral state means for democratic life. Do such experiences with the state prevent people from participating in politics, or mobilize people? Are there spillover effects among their loved ones or neighbors? Could changes to election practices or the legal system shift these patterns? But as Burch (2009) points out, political science, and particularly the subfield of American politics, was slow to study the punitive face of government. Even as the number of people with convictions or past experiences of incarceration grew dramatically, political scientists left it to sociologists and economists to think about the implications of these shifts.

Over the last two decades, however, political scientists focused on the United States have turned their attention to the carceral state (Harris et al. 2020). Their work has illuminated the political processes that yielded our current system of mass incarceration, as well as some of its political effects. In this article, I review the part of that literature focused on mass political participation: How does contact with the criminal legal system shape people’s ability or willingness to take part in political life?

The review proceeds as follows. Section 2 begins with a discussion of the various types of data that underlie scholarship on this topic, and their utility for answering different types of questions. Section 2.2 presents a series of stylized facts that we have learned from the last decade of scholarship, highlighting recent progress as well as opportunities for future contributions:

1. Descriptively, carceral contact is associated with less voting.
2. Short periods in jail have a causal effect on voting.
3. There is less clear evidence that long prison terms themselves reduce voting.
4. There is mixed evidence of household or community spillover effects.
5. Scholars know less about nonvoting participation than about voting behavior.

Section 3 discusses the theoretical mechanisms that could underpin some of these empirical regularities. Section 4 asks what could change, within either the political system or the criminal legal system, that could disrupt the patterns that this literature has documented. Section 5 concludes.

## 2. WHAT WE KNOW AND HOW WE KNOW IT

### 2.1. Data Sources

One of the strengths of the carceral state literature is its reliance on a range of data sources and methodological approaches. These varied approaches give us different insights into how carceral contact shapes behavior. Before diving into what we know, this section reviews how we know it.

**2.1.1. Qualitative data.** Qualitative data, including personal narratives and interviews, can give a deep understanding of people’s experiences with the carceral state. Work like the interviews in Weaver & Lerman (2014) and Middlemass (2017) can show the breadth of experiences that people have with the criminal legal system, as well as how people describe and make sense of those experiences in their own words. And qualitative data may not only come from researcher-led interviews or archival work. The Portals project, discussed further in Section 2.2, uses an immersive camera technology to facilitate conversations between ordinary people in heavily policed neighborhoods all over the country (Weaver et al. 2020).

These kinds of qualitative data are key for building theory about how people experience the carceral state. They provide an understanding of the types of experiences that people can have, which is crucial for thinking about what other data collection efforts (like surveys) should ask about. Interviews and other qualitative approaches rarely give direct measures of prevalence: We learn about people’s experiences, but we may be limited in our ability to characterize how widespread those experiences are.

**2.1.2. Survey data.** Surveys allow us to learn about the experiences of many people and to provide high-level descriptive statistics about important phenomena. Surveys such as the National Longitudinal Study of Adolescent Health (“Add Health”) and the Fragile Families and Child Well-Being Study allowed Weaver & Lerman (2010) to estimate the association between carceral contact and political behavior for the population covered by those surveys. Nationally representative surveys, where available, can tell us whether a phenomenon that has been observed in a given city or social group is widespread elsewhere.

Surveys are a powerful tool for descriptive inference, but there are limits to their usefulness for measuring causal relationships. Perhaps the most obvious concern is that of omitted variable bias, as a survey may not include information on possible confounders. But studies of the carceral state also face less obvious measurement challenges. Some people may not appear in survey samples for reasons related to their past convictions (Pettit 2012). Even if a survey asks about someone’s criminal conviction history, they may not report it out of embarrassment or worries about privacy. Or they may not remember details of when and how many times they have been arrested or convicted. These challenges are magnified when surveys ask respondents whether people in their life have been arrested or convicted: Respondents may be reluctant to admit to this, or they may simply not know. Section 2.2.4 discusses the inferential problems caused by such response issues.

**2.1.3. Administrative data.** Some recent work on the carceral state has relied on government records produced by courts or boards of elections. These fine-grained government records enable observation of people who might be missed by survey recruitment, and their measures of conviction or participation do not rely on people’s memories or their willingness to report those experiences to a researcher. Thus, these data sets can provide a very thorough look at conviction or incarceration within a given jurisdiction. As described below, they can sometimes be used for very precise causal inference that relies on natural experiments or even field experiments.

But new sources of administrative data come with new challenges. Given the decentralized nature of carceral institutions in the United States, there is immense variation in how willing jurisdictions are to share data (Goerger et al. 2020). Researchers using this kind of data must think carefully about how findings from their chosen jurisdictions would generalize to other (unobservable) places, as well as being sure they understand the data-generating process that underlies the data sets they collect and use. Most of these data sets are collected by governments and reflect the perspectives and priorities (as well as the biases) of government actors (Knox et al. 2020, Johnson 2021). Further, there are complex ethical questions about the collection, use, and

storage of personally identifying government data, particularly in an era of research transparency and data sharing.

## 2.2. Stylized Facts

The growth of research on the carceral state over the last decade has yielded many conclusions, some of which seem to be in conflict. In this section, I draw some stylized conclusions from this literature, highlighting findings that have been supported by multiple papers as well as explaining how some apparently contradictory findings fit together.

**2.2.1. Descriptively, carceral contact is associated with less voting.** Across a range of experiences on the spectrum of carceral contact—police stops, arrest, conviction, incarceration—and a range of measurement strategies, we have seen that people targeted by the carceral state are less likely to vote than people who do not have these experiences.<sup>1</sup> Weaver & Lerman (2010) analyze multiple surveys, finding that people who reported criminal justice contact, ranging from police questioning to long prison terms, were less likely to report voting or other political activity. Analyses that link public records of conviction or incarceration to state voting data confirm this overall pattern (Haselswerdt 2009, Burch 2011). Burch (2011) examines voting rates among men with past felony convictions in five states and finds that even with higher turnout in 2008 than in previous years, only about one in five members of this group voted in 2008. This rate is low even compared to people with similar life histories but no convictions: Uggen & Manza (2002) extrapolate from nondisfranchised people matched on age, race, gender, education, income, and marital status to predict that 35% of people with felony convictions would have voted in 2004 if allowed, which Burch (2011) points out is likely a large overestimate.

On average, people who encounter the punitive face of the state are less likely to vote than those who do not. This is a descriptive statement, not a causal one. The next few stylized conclusions take on the question of causal inference, and Section 3 discusses some theories of how (and for whom) carceral contact could reduce participation. But this is a crucial descriptive finding, regardless of the underlying mechanisms. Millions of Americans carry with them an intimate understanding of one of the most profoundly invasive and life-altering functions of the state, because they have experienced conviction or incarceration (Shannon et al. 2017). Most of them retain the legal right to vote. In large part, they do not vote. The people who are most affected by carceral policies are almost uniquely unlikely to participate in the electoral system that maintains (or could change) those policies. I return to this point in Section 4.

One form of punitive contact with the state that bears special mention is that of legal financial obligations, the fines and fees associated with criminal cases. These kinds of burdens have been documented in specific states, but there are very limited national data available on their extent or how people experience them (Harris 2016). There is room for political scientists to follow the lead of Meredith & Morse (2017) in examining what these financial obligations mean both for people's legal eligibility to vote and for their tendency to do so.

**2.2.2. Short periods of incarceration have a causal effect on voting.** Spending a few days or weeks in jail, either in pretrial detention or as a misdemeanor sentence, appears to prevent people from voting in the future. White (2019b) measures the causal effect of jail time on future voting, using administrative data from a county that randomly assigns cases to more- or

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<sup>1</sup>For a detailed overview of various studies' data sources and findings in this area, see table 1 of Gerber et al. (2017).

less-lenient courtrooms. This random assignment allows the comparison of otherwise-similar people charged with misdemeanor crimes who are or are not sentenced to jail time essentially by chance. In Harris County, Texas, White (2019b) finds that jail sentences reduced voting by several percentage points, with particularly large demobilizing effects among Black residents. Section 3 discusses possible reasons for these racial disparities.

Similarly, McDonough et al. (2020) examines pretrial incarceration, in which people are held in local jails while their cases are pending. The authors use the as-if-random assignment of cases to bail magistrates (whose decision of whether and how high to set monetary bail amounts can mean that people remain in jail awaiting trial) to measure the effects of pretrial detention on future voting. The authors find that pretrial incarceration reduces voting, especially among people who are Black and/or low-income. This study focuses on a different jurisdiction (Philadelphia) and a different stage of the criminal process than White (2019b), but finds a similar pattern of racially disparate demobilization from jail time.

Both these papers use administrative data and quasi-experimental designs to measure the causal effects of a form of legal contact that is often thought of as minor. Though millions of people face misdemeanor charges or are held in pretrial detention each year, the phenomenon has not received as much attention as felony convictions and prison sentences, perhaps partly because it is difficult to observe. This recent work suggests that these short-term interactions with the carceral state can have substantial, lasting, and racially disparate effects on political behavior even though they do not lead to formal disenfranchisement. There is more work to be done on how these estimates generalize beyond the specific places studied by White (2019b) and McDonough et al. (2020), how they vary with other personal or system characteristics, and what mechanisms underlie these effects.

**2.2.3. There is less clear evidence that long prison terms themselves reduce voting.** The first stylized fact (Section 2.2.1) notes that carceral contact is associated with less voting (Weaver & Lerman 2010), which is an important feature of political life in the United States. And the second stylized fact (Section 2.2.2) notes that in the case of short jail stints, that association appears to result from a causal relationship: Jail time itself makes people less likely to vote than they would have been if they had never been to jail. In the case of longer prison terms resulting from felony convictions, though, there is less clear evidence of such a causal effect. People who have been imprisoned are less likely to register and vote than people who have not, but they were also less likely to vote before being sentenced (Burch 2011). Studies that have sought to measure the effect of incarceration on voting have yielded mixed conclusions.

As described above, Weaver & Lerman (2010) analyze several survey data sets, finding that people who report having been to prison or jail were less likely to report being registered or voting, even when controlling for a variety of other factors. They then present several other specifications designed to address concerns about bias, including matching and a placebo test relying on the timing of cases. Similarly, Weaver & Lerman (2014) use a panel survey with multiple waves to account for previous (preincarceration) voting.

Gerber et al. (2017) point out the risks of selection bias and measurement error in survey-based approaches, noting that even panel studies will struggle to identify the effects of incarceration in the face of misreporting or time-varying confounders. Using records from Pennsylvania's court system (and, in a supplemental analysis, Connecticut's), the authors present three different approaches to measuring the causal effect of incarceration on voting. First, they examine 2008 and 2012 turnout rates for a set of people who were admitted to prison for the first time after the 2008 election and were released by the 2012 election. They note that 2012 (postincarceration) turnout was, if anything, higher than 2008 (preincarceration) turnout for this group, though they

acknowledge that this approach does not capture other reasons that turnout could have increased over time for this group, such as aging. Second, they compare registered voters who were sentenced to prison between the 2008 and 2012 elections to other registrants in the state, adding in pretreatment covariates such as age, gender, and previous participation. This approach finds that the estimated effect of prison time on voting shrinks dramatically as more covariates are included, with the fullest set of covariates yielding an estimate of about five percentage points' drop in voting due to incarceration. Though this estimate still suggests a substantial reduction in voting, the authors note that that estimate might shrink even further if other unobservable characteristics could be included in the model.

In their final approach, Gerber et al. (2017) examine 2012 turnout among people convicted of felonies between 2008 and 2010, comparing people who were sentenced to prison to those who were sentenced to something other than incarceration and finding very small estimated effects (the largest estimate is 0.8 percentage points' reduction in voting for people sent to prison). Gerber et al. (2017, p. 1143) describe this approach as "the most credible evidence that spending time in prison does not cause a large reduction in turnout." While noting that sentences are not assigned at random and that people sentenced to prison could differ from those receiving other sentences, the authors point out that any such differences should likely have the effect of biasing the estimates to overstate the effect of prison, meaning that the effect of prison on voting could be even smaller than the estimates presented.

However, one recent paper raises the possibility of a different issue with this comparison. McDonough et al. (2020), as described above, find that being held in pretrial detention can reduce future voting substantially and note that such a pattern could help explain some null effects in this literature. If many of the people counted as nonincarcerated in the study nevertheless spent some time incarcerated pretrial, the estimates presented would carry a different interpretation. Rather than estimating the effect of a prison sentence relative to no incarceration, the study would estimate the additional effect of a long prison sentence on the voting behavior of someone who had already experienced some incarceration while their case was being handled.

What should we take from this debate? First, I note that all of the work discussed in this section underscores the *descriptive* pattern mentioned above: People with carceral contact vote less often, though there is disagreement about why. Then, Gerber et al. (2017) demonstrate convincingly that selection bias plagues many of our estimates of incarceration's effects on voting and may yield estimates that overstate the true causal effect. But the estimates presented by Gerber et al. (2017) do not yet allow the conclusion that prison time has *no* causal effect on voting. In a supplemental analysis of administrative data from Connecticut, Gerber et al. (2017) again find that including covariates reduces the size of the estimated "incarceration effect," but the estimates even under the most complete model are substantially larger than those found in Pennsylvania (nearly 15 percentage points in Connecticut, compared to 5 in Pennsylvania). And in the most credible estimates presented in Pennsylvania, comparing imprisoned to nonimprisoned people, it is possible that many of the so-called nonimprisoned people nevertheless experienced some incarceration during this time period, making it harder to interpret the estimates as a comparison between incarceration and freedom. Gerber et al. (2017) provide a valuable statement of the problems encountered by previous work but leave room for additional research on this causal question.

Further, there is room to theorize more precisely about what types of incarceration might have the largest effects, and among whom. White (2019b) speculates that first-time misdemeanor defendants sentenced to jail time might differ (in their underlying propensity to vote) from people facing longer prison sentences for felony convictions. It is possible that some differences in the findings of studies based on administrative data and on surveys might be due not only to

bias in survey approaches but also to differences in who is included in the “incarcerated” group in such studies.

**2.2.4. There is mixed evidence of household or community spillovers.** In addition to the effects of carceral contact on an individual person’s behavior, we might wonder about possible spillover effects. Do these experiences also shape the behavior of one’s family members or neighbors? Learning about such topics means first defining what kinds of extended or “proximal contact” (Walker 2014) could be occurring and how frequent such contacts are, and then developing approaches to measure their effects on people’s political behavior. Both tasks are challenging.

Scholars have used creative approaches to characterize secondhand carceral experiences. Interviews with people visiting prisons or keeping in touch with incarcerated people highlight how these ties restructure people’s lives and interactions with government (Comfort 2008), while surveys and administrative data help illustrate how widespread and racially disparate such connections are (Wildeman 2009, Bobo & Thompson 2010). Lee et al. (2015), for example, use the General Social Survey to estimate rates of connectedness to imprisoned people, finding that 12% of White women and 44% of Black women report having at least one family member imprisoned. Broadening the focus to the neighborhood level, Burch (2013) wrangles an enormous volume of administrative data to describe the geography of incarceration in North Carolina and Georgia, highlighting the intense spatial concentration of incarceration and supervision in already-disadvantaged neighborhoods.

All of these projects underscore how prevalent such forms of proximal contact are in the United States, and they prompt the question of how these experiences shape people’s political behavior. I begin with the effects of personal connections and then turn to neighborhood effects.

**Table 1** briefly summarizes recent work on how proximal contact shapes participation. It focuses on studies of individuals with personal connections to someone with carceral contact. The table gives a sense of how varied the approaches to studying this question have been: The studies rely on definitions of proximal contact ranging from “someone you know has been arrested” to “having a romantic partner incarcerated,” and they examine a range of participatory outcomes across various geographic locations and data sources.

Even when they examine seemingly similar questions, they reach strikingly different conclusions. Studies relying on surveys to measure the effect of proximal contact on voting reach the conclusion that such experiences demobilize family members (Lee et al. 2014, Sugie 2015), that they have no effect on close friends’ or family members’ voting (Walker 2014, Walker & García-Castanón 2017), or that they have a mobilizing effect for people who know someone convicted of a felony in a state with felony disenfranchisement (Anoll & Israel-Trummel 2019). Similar variation emerges on nonelectoral forms of participation.

These contradictory findings could be driven by differences in data sources or definition, or by some of the inferential challenges faced by researchers on this topic. Just as survey estimates of the effects of personal carceral contact are subject to misreporting of people’s experiences or participation, studies of the effect of proximal contact often rely on respondents both to be aware of and to accurately report their experiences of proximal contact. If some people are hesitant to disclose such experiences to survey takers, that could bias the estimated effects of proximal contact (especially if people who are reluctant to report proximal contact are also more/less likely to misreport their levels of political participation). In addition to this form of measurement error, such studies must contend with selection bias: People who experience proximal contact may differ from people who do not on many dimensions, so observed differences in their behavior might be driven by something other than proximal contact.

**Table 1 Papers on proximal contact**

Reference	Contact type	Relationship	Data type	Geography	Outcome measures	Effects	Notes
Walker (2014)	Arrested, charged, or questioned by police	Close friend or family member	Survey (Washington Poll)	Washington State	Voting, nonelectoral participation	Voting: 0 Nonvoting participation: +	Larger effects for POC, though limited sample given Washington demographics
Lee et al. (2014)	Incarcerated	Parent	Survey (Add Health)	US	Voting, other political participation	Voting: – Nonvoting participation: –/0	Also includes interview data on attitudes among women with incarcerated partners
Sugie (2015)	Incarcerated	Romantic partner	Survey (Fragile Families)	Large US cities	Voting, registration, belief in importance of voting	Voting: – Registration: – Importance of voting: –	Panel data, with pretreatment placebo test
Walker & García-Castanón (2017)	Arrested, charged, or questioned by police	Varies (continuous measure of closeness, from someone you know to family member)	Survey (National Crime and Politics Survey)	US	Voting, nonelectoral participation	Voting: 0 Nonelectoral participation: +	Focuses on women of color
White (2019a)	Arrested, convicted, incarcerated	Household member	Administrative	Harris County, TX	Voting	Voting: 0/– (short-term negative effect, long-term null)	Focuses on registered voters
Anoll & Israel-Trummel (2019)	Convicted of a felony	Varies (count of people in “closest network of friends and family”)	Survey (Latino Decisions poll of Black respondents)	US	Voting, index of other political activities	Voting: + (in FD context) Participation index: + (in FD context)	Focuses on interaction between proximal contact and state FD laws
Walker (2020)	Arrested, charged, or questioned by police	Varies	Survey (National Crime and Politics Survey, ANES)	US	Participation index	Participation: +	Includes mediation analysis of sense of injustice

Abbreviations: ANES, American National Election Studies; FD, felony disenfranchisement; POC, people of color.



Some of these problems can be addressed through the use of administrative records of conviction or incarceration and of voting. White (2019a), for example, uses court records in combination with address data from the voter file to identify households affected by criminal cases without relying on self-reports, and uses the timing of cases (before or after election day) to measure the effects of arrest, charging, or conviction on household members' participation. Morris's (2021) study of household participation in Florida during the period when Amendment 4 (proposing changes to felony disenfranchisement that would have restored the right to vote to many Floridians) was on the ballot relies on release addresses from the state corrections department to match formerly incarcerated people to their household members. But such approaches introduce their own interpretation challenges. White's (2019a) study, for example, restricts the analysis to household members who are already registered to vote at the time of contact, and to one specific jurisdiction for which court and voting data sets are readily available. Both White's and Morris's (2021) studies focus only on voting, rather than other forms of political participation, since voting is easily observed through public records (Section 2.2.5 addresses this issue in more detail). Administrative records solve certain problems, especially in the realm of causal inference, but they are not a panacea.

There is room for progress to be made both through the introduction of new designs and through the construction of new data sets. Surveys may remain the most straightforward way to observe certain types of proximal contact and certain behaviors, but future survey-based work could make new contributions. First, **Table 1** indicates that some existing studies have attempted to run interaction models to focus on effects among specific subgroups: people in states with certain voting laws in place, or members of specific racial or ethnic groups. These estimates have often relied on very small samples, and future surveys could be designed to collect enough observations to put such analyses on firmer statistical footing. Second, future research could potentially include multiple measures of different kinds of proximal contact within the same survey, allowing for some analysis of how definitional choices shape effect estimates and potentially resolving some of the apparent contradictions in current work. And finally, this is an area where repeated measurement (panel surveys) could be very useful in estimating causal effects. It is worth keeping in mind Gerber et al.'s (2017) cautions about the limitations of such panel approaches, but they nevertheless seem worth trying in the realm of proximal contact, particularly as some of their specific concerns about criminality as an unobserved confounder may be less relevant when survey respondents are asked about other people's convictions rather than their own.

This subsection has discussed the effects of proximal contact on individual people who saw their friends or loved ones arrested or incarcerated. Perhaps even more difficult than estimating such individual-level effects of proximal contact is estimating the effect on entire *neighborhoods* of experiences like incarceration or police surveillance. As noted in Section 1, such experiences are not distributed across neighborhoods evenly or at random (Kurgan 2013, Eckhouse 2017). Instead, they are intertwined with other forms of social and economic inequality, with race-class subjugated communities experiencing both quantitatively and qualitatively different policing and incarceration outcomes (Western 2006, Clear 2009). Such patterns are descriptively important; they also pose a methodological challenge to researchers who seek to compare, for example, political participation in high- and low-incarceration neighborhoods. Such neighborhoods are different in myriad ways, and it is hard to even define the counterfactual being considered: What does it mean to picture a neighborhood with all other features intact but much lower incarceration rates than it currently faces?

Researchers occasionally seek out quasi-random variation in the experience being studied, such as the exact timing of killings by police or of individual residents returning home from prison. Such approaches can be particularly useful for learning about the short-term effects

of such “treatments,” since they usually rely on small, fleeting variations in dosage between otherwise-similar neighborhoods. For example, Burch (2013) uses the timing of residents’ prison admissions as a source of short-term variation in neighborhoods’ experiences of incarceration. But such approaches rarely yield long-term or large-scale differences in neighborhood experiences, so they must be interpreted carefully when researchers think about the scale of neighborhoods’ exposure to policing or incarceration, especially over time.

**2.2.5. Scholars know less about nonvoting participation than about voting behavior.** Voting is a widely studied political behavior, for good reason: It is a common and accessible form of participation, central to much democratic theory, and it is easy to observe through public records. Other forms of participation, such as protest or working with community organizations, are harder to systematically observe. Some surveys ask about these behaviors alongside questions about carceral contact (Weaver & Lerman 2010, Owens & Walker 2018), but they are few and variable in their geographic coverage and sample sizes.

It may be tempting for quantitative researchers to yield to these data limitations and restrict our analyses to voting only, perhaps imagining that other political behaviors will follow similar trends. But there are many theoretical reasons to expect that voting may respond differently to carceral contact than other forms of political participation (Owens & Walker 2018). Narrowing our analyses to voting risks painting “a decidedly incomplete portrait of political life in . . . race-class subjugated (RCS) communities” (Soss & Weaver 2017, p. 567). We risk overstating the disempowerment of people targeted by the carceral state and missing the “insurgent political resistance” they bring to their daily interactions with government (Soss & Weaver 2017, p. 583; Michener 2018).

There is a lot of opportunity in this literature for creative approaches, both to defining and measuring nonvoting participation and to measuring how carceral contact shapes these behaviors. Some contributions could come from running larger and more frequent surveys, as discussed in the previous section. But in this section I also highlight promising nonsurvey approaches to understanding these behaviors.

The Portals project (Bakshi et al. 2017) provided a dramatically different look at people’s experiences of policing and the state. Rather than a survey approach or a researcher-facilitated interview, the project used immersive audiovisual technology mounted inside repurposed shipping containers to link together people in heavily policed neighborhoods for informal conversations with each other about how they view police. People walking past the containers were invited to go inside them, where a camera setup brought them face-to-face with someone in another container in another city for a brief conversation. This hands-off approach to facilitating conversations allowed neighborhood residents to “speak, unscripted, about their experiences and to convey their narrative” (Bakshi et al. 2017, p. 3) and yielded insights that might not otherwise have emerged from more structured approaches, including patterns of “collective autonomy,” or of both retreating from state contact but also advocating for deeper community engagement (Weaver et al. 2020, p. 606). These unmediated discussions of people’s personal experiences with the state help to form the basis of “a theory of the state from below” (Prowse et al. 2020, p. 1424; Michener et al. 2020).

Other approaches rely on administrative records of contact with government: Lerman & Weaver (2014) measure neighborhoods’ rates of contacting 311, while several recent papers examine 911 calling behavior in the wake of police violence (Desmond et al. 2016, Cohen et al. 2019). These approaches carry with them thorny questions of how to interpret such contacting behavior (Bell 2016, White & Trump 2018) as well as how to measure causal effects (Zoorob 2020), but they can allow for more complete coverage than many other data collection approaches.

Similarly, some papers outside of the carceral state literature have sought to measure protest participation via social media data (see, for example, Hsuan et al. 2017 and Larson et al. 2019). As these papers acknowledge, the representativeness of such social media data sets depends on context. There may be room to use such digital-trace approaches to study the participatory behavior of people with past carceral contact, but their success may depend on how digitally connected this population is (and will raise important questions of privacy).

### 3. MECHANISMS

In contexts where carceral contact seems to reduce political participation (especially voting), how does that change come about? In this section, I highlight common explanations for why people would be less likely to vote in the wake of police contact, convictions, or incarceration. It is difficult to pinpoint causal mechanisms (Bullock et al. 2010), and in this realm of research, such challenges are compounded by the data limitations described above. Nevertheless, a substantial body of research has discussed and sought to measure possible mechanisms, and I introduce it briefly here.

The broader literature on voter participation has highlighted factors that make people more or less likely to vote. Verba et al. (1995) pithily summarized the reasons people may not vote: They can't, they don't want to, or nobody asked. Carceral experiences could plausibly shape any of these three factors, though I focus mainly on people's willingness and ability to vote (less is known about mobilization, and I discuss available evidence in Section 4). In this sense, carceral systems resemble many other types of government policy: As the policy feedbacks literature has observed, policies can make politics by shaping people's attitudes and behavior (Campbell 2002). But unlike the benefits-granting programs most often examined in this literature, convictions and incarceration confer negative experiences, social stigma, and material costs on the people subject to them.

Although this section considers negative policy feedbacks rather than the more commonly studied positive ones, the policy feedbacks literature provides a useful framework for thinking about how experiences with government policy can shape political participation. Pierson (1993) distinguishes "interpretive" or "learning" effects, where people's experiences with policy shape their values or attitudes, from simpler "resource" effects, in which policies provide material or civic resources that help people participate. I consider both these possibilities below, beginning with the ways that interpretive effects could reduce people's willingness to vote and then turning to the ways that resource effects could make it hard or impossible for them to do so.

#### 3.1. Willingness to Participate: Learning/Socialization Effects

The policy feedbacks literature teaches us that people learn about government from their interactions with state programs (Bruch et al. 2010, Campbell 2012). It is hard to imagine a more instructive set of experiences with government than the all-encompassing punitiveness of jail or prison time, or even of police questioning. A rich literature describes how the structure and content of those carceral experiences shape people's views of government and of their own role in society (Lerman 2013, Justice & Meares 2014, Weaver & Lerman 2014).

Weaver & Lerman (2014, pp. 9, 15), as described above, draw on both survey data and interviews to develop a theory of "custodial citizenship," describing how people's experiences with criminal justice authorities become the closest reference point for understanding government and yield a view of government that focuses on "control, authority, and dominance." Such experiences teach that the state is unresponsive and that custodial citizens are not full or equal citizens in the eyes of the law, both conclusions that do not encourage political participation.

### 3.2. Ability to Participate: Resource Effects

Beyond the attitudinal changes wrought by conviction or incarceration, these experiences can produce serious life disruptions. A large literature in sociology and other disciplines highlights possible consequences of a conviction or a period of incarceration: job loss, housing instability, and loss of access to certain public benefits (Chesney-Lind & Mauer 2003, Kirk & Wakefield 2018). Both classic and recent works in political science underscore how such material deprivation can make it hard for people to participate in politics (Rosenstone 1982, Verba et al. 1995, Hall et al. 2021), and these resource effects could be another pathway through which carceral contact reduces voting. Some recent work has attempted to distinguish between socialization and resource effects obliquely, such as by focusing on homeowners or people living in high-income neighborhoods (with the assumption that they should be less vulnerable to extreme resource shocks even after a conviction), but future research could directly measure such material changes (White 2019b, McDonough et al. 2020).

Beyond the immediate material costs of conviction or incarceration, these experiences can yield other life changes that might make it difficult or impossible for people to vote, as I consider in the next two subsections.

**3.2.1. Mechanical/eligibility effects.** In some cases, a conviction may leave people legally ineligible to vote, or it may mean that they are practically unable to vote because they are being held in jail on election day.<sup>2</sup> This kind of incapacitation is important, but it is theoretically distinct from the kind of political socialization process described above, and it could have different implications for long-term behavior. Someone who is not voting because they are presently not allowed to might return to voting when they regain their rights, but someone who is not voting because they no longer think it matters may not. And these pathways can be difficult to distinguish in certain data sets: Weaver & Lerman (2010) note that some surveys ask questions about voting eligibility, while others do not, making it difficult to distinguish between formal and informal exclusion. Other approaches (interviews, administrative data) sometimes allow researchers to observe whether people were legally ineligible or in custody at the time of a given election.

**3.2.2. Misinformation.** Distinct from legal disenfranchisement is the possibility that people might mistakenly think they are not allowed to vote. Available survey and interview evidence finds that many people with past arrests or convictions believe they are ineligible to vote even in contexts where they are not legally disenfranchised (Drucker & Barreras 2005, Manza & Uggen 2006, McCahon 2016). Such beliefs could come from confusion about the law, from rumors, or even from interactions with official sources: Studies have found that many elections officials are also misinformed about voting eligibility for people with past convictions (Ewald 2005, Allen 2011). And people with past convictions have good reason to be cautious about voting when they are uncertain about eligibility. High-profile cases of people facing criminal charges after voting while ineligible, such as the sentencing of Crystal Mason to five years in prison for attempting to vote while on supervised release, underscore the risks (Ura 2021).

It is difficult to know just how much misinformation could contribute to the low participation rates among people with past convictions [though see Meredith & Morse (2015) for a valuable discussion of this possibility], nor how much a given study's estimates could be driven by misinformation effects. Some research designs, such as Gerber et al.'s (2017) comparison among people

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<sup>2</sup>While people being held pretrial or serving misdemeanor sentences are technically eligible to vote in most cases, they may find it difficult or impossible to actually exercise that right (Paikowsky 2019).

with recent convictions, seem likely to hold rates of misinformation constant within their sample, while designs comparing people with very different levels of carceral contact might be especially likely to capture the effects of misinformation along with other pathways to demobilization.

**3.2.3. Effect heterogeneity.** In addition to highlighting specific pathways through which carceral contact could reduce voting, the literature has made some specific predictions about who could be most affected by these experiences. Some work has found that the demobilizing effects of jail time are concentrated among African Americans (White 2019b, McDonough et al. 2020). Other research highlights the possibility that some people might be mobilized (rather than demobilized) by experiences with the criminal legal system, particularly proximal contact (Walker & García-Castanón 2017, Walker 2020).

Thus far, efforts to explain such heterogeneity have been fairly informal. White (2019b), for example, theorizes that jail time could be particularly demobilizing for Black residents because racially disparate policing yields a population of Black arrestees who were relatively likely voters, as compared to white arrestees, who were unlikely to vote in the first place. White's (2019b) analysis corroborates this theoretical claim with evidence from past elections showing that Black arrestees had higher prearrest turnout rates than other groups. But as McDonough et al. (2020) aptly points out, other pathways could also lead to racially disparate effects of jail: Different treatment in jail by race, or different exposure to resource shocks, could also yield such different effects. Collecting as much information as possible on these specific pathways (such as jail experiences) will not allow future research to address all the methodological challenges of distinguishing between different causal stories, but it will allow for some basic tests to rule out certain pathways.

## 4. WHAT CAN BE DONE?

The previous sections have described the still incomplete but growing body of work on how the carceral state shapes political participation. Even with caveats about data limitations and open questions, the patterns are striking: People who have had many kinds of personal (Weaver & Lerman 2010, Gerber et al. 2017) or proximal (White 2019a, Morris 2021) contact with the carceral state are dramatically less likely to participate in formal electoral politics than other people. Voting is a common and constitutive form of democratic participation and one where we see dramatic participatory gaps. This pattern means that the people who are most personally knowledgeable about the carceral state, the individuals and neighborhoods with the most intimate understanding of the system, are the least likely to be involved in electing policy makers who will determine what that system looks like in future. Earlier sections of this article have highlighted methodological debates about the causes of this descriptive pattern, as well as cautions about ignoring other forms of nonvoting political behavior, both of which are important points. Nevertheless, I believe this state of affairs—dramatic underrepresentation of people with carceral contact in the electorate—poses a normative problem for our representative democracy. For readers who share my concern, this section discusses what could be done about the democratic problem posed by mass incarceration.

### 4.1. Broadening Eligibility

One high-profile proposal being considered in many states is broadening voter eligibility by changing or removing felony disenfranchisement laws. All but two states currently prevent people from voting while they are incarcerated for felony convictions, and many states continue to exclude people from voting after release—either while on supervision, for some years after a

conviction, or in a few cases for life (Chung 2019).<sup>3</sup> Nationwide, these laws mean that over six million people—one in every 40 US adults—are legally ineligible to vote due to convictions, with one in 13 voting-age African Americans subject to felony disenfranchisement (Uggen et al. 2016). What if states got rid of these laws? There are important normative discussions to be had about felony disenfranchisement laws (Katzenstein et al. 2010), but here I focus on empirical claims about what could happen if they were removed.

There is little evidence that removing felony disenfranchisement laws would yield a wave of new voters. Analyses that calculate counterfactual voter participation without felony disenfranchisement laws (e.g., Manza & Uggen 2006) often assume that people with felony convictions would vote at the same rates as demographically similar people without felony convictions. But as discussed above, people with felony convictions do not vote at the same rates even when they have the right to do so (Burch 2011). Often, they were voting at low rates even before they were convicted (Gerber et al. 2017). Even in Maine and Vermont, the two states that do not disenfranchise people while they are incarcerated, currently and formerly incarcerated people vote at very low rates (Meredith & Morse 2015, White & Nguyen 2022). We should be very cautious about predicting large turnout increases after felony disenfranchisement laws are removed.<sup>4</sup>

That said, proponents of ending felony disenfranchisement articulate a broader set of possible effects than increasing individuals' voter turnout. Perhaps removing such laws could have spillover effects on the family members of people with past convictions or on high-incarceration-rate neighborhoods. Right now, there is limited evidence for such effects, and they present thorny research design challenges (as discussed in Section 2.2). There is a need for better research into these laws' spillover effects on affected families and communities.

There is also work to be done on the experience of regaining (or fighting for) the right to vote and what that means for people's sense of citizenship and social belonging. Owens (2014) discusses the work of previously incarcerated activists in Rhode Island who organized to win the right to vote, highlighting the ways in which this kind of group-based activism can combat "civic degradation." For some people, getting involved in campaigns for rights restoration may serve as a path to deeper integration into their community and longer-term political involvement.

## 4.2. Infrastructure and Outreach

Other possible changes include making sure people are aware of their right to vote and are given the resources they need to exercise it. This could include practices to enable voting from local jails, introducing rules that ensure people are notified when they become eligible to vote again after a previous conviction, or other practices intended to help people participate in political life after carceral contact.

There is very limited evidence measuring the effectiveness of specific jail-voting initiatives, such as providing information or help requesting ballots, or even siting polling places in jails (Paikowsky 2019). As more places and organizations begin such programs in the years to come, there will be room for more research on what works best in this realm.

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<sup>3</sup>For historical discussion of the racial and partisan politics of the introduction and removal of these laws, see Behrens et al. (2003) and Yoshinaka & Grose (2005).

<sup>4</sup>Other discussions focus on the partisan implications of removing felony disenfranchisement laws. Again, available empirical evidence suggests that the partisan impact of these changes has been overstated. People with felony convictions have heterogeneous political preferences and are not a Democratic monolith (Burch 2010, Meredith & Morse 2018, Morse 2021).

Careful studies have been conducted on state notification policies that require officials to tell people when they have regained the right to vote. In 2005, Iowa removed its application process for rights restoration and automatically began restoring the right to vote when people left custody, but the state notified only some people of that change (Meredith & Morse 2015). Meredith & Morse (2015) find that people who were mailed a certificate notifying them that their rights had been restored were nearly 10 percentage points more likely to vote than otherwise-similar people who (because they were released at slightly different times) were not so notified. However, a study of newly implemented notification requirements in New Mexico, New York, and North Carolina found no such effects of notification laws on registration or turnout (Meredith & Morse 2014). The authors suggest that these effect differences could be driven by differences in how states implement notification. In contrast to Iowa's clear and explicit mailing stating that one's rights had been restored, New York tucked a brochure into a release packet that obliquely mentioned the requirements for registering to vote. These findings highlight the importance of policy design for the effectiveness of informational interventions.

Beyond notifying people at the time they regain the right to vote, as in the previous studies, states and other actors could potentially undertake a range of other activities to make sure people know about their rights. One study found that a letter telling people they had the right to vote increased registration and turnout by several percentage points (Gerber et al. 2015). This effort focused on people who had just completed their sentences, and consisted of a letter from the Connecticut Secretary of State's office. Several questions about such outreach efforts remain. Can they be as effective (or perhaps even more effective) when the outreach is done by nongovernmental organizations rather than state agencies? Can they reach people whose convictions occurred some time ago and who may be hard to find or hard to convince of the importance of politics? And can they move people to undertake political actions beyond voting in high-turnout elections? As organizations like the Florida Rights Restoration Coalition and other nonprofit groups led by formerly incarcerated people ramp up their efforts to engage others with past convictions in political advocacy, these questions will only become more pressing.

### 4.3. Shrinking the Carceral State

All the changes I have discussed so far assume a constant flow of people through the machinery of the carceral state, with changes happening after carceral contact to try to clean up the damage done. But it is also natural to ask whether we could instead reduce people's exposure to the system or change the experiences they have with it. Detailed plans for reform or abolition are beyond the scope of this article, but both scholars and activists have considered them as well as the political challenges they face (Davis 2011, Gottschalk 2016, Pfaff 2017, Kaba 2021).

A general challenge for the academic study of the carceral state has been the remarkable fragmentation of the object of study. As noted above, there are thousands of individual police departments, courts, and jail and prison systems across the United States, and consistent data from these many systems can be hard to come by. This fragmentation poses difficulties for researchers who want to get a high-level view of the system, and it yields concerns about generalizability if researchers rely on administrative data from just one or a few jurisdictions. But it also yields possibilities for local policy experimentation and variation (Anzia 2019), and that local experimentation may allow us to learn about what would happen if the United States were to become a lower-incarceration country.

For example, one change in recent years has been the election of so-called reform prosecutors in major US cities including Philadelphia and Boston. These candidates campaigned for district



attorney positions on platforms of using official discretion to reduce incarceration through changes like reducing the use of cash bail or the prosecution of minor offenses. Academic researchers have begun evaluating how these changes have shaped incarceration, as well as their effects on future recidivism and crime (Ouss & Stevenson 2019, Agan et al. 2021). But these changes could also yield an opportunity to see how reducing the use of prosecution and incarceration could affect democratic processes. Similarly, as cities experiment with police restructuring, they may open up opportunities to learn about how different policing approaches yield different politics for affected neighborhoods.

## 5. CONCLUSION

This article has reviewed what we know about political participation in the shadow of mass incarceration in the United States, as well as remaining questions and empirical challenges.

Some caveats are in order, chief among them the limits of the questions examined here. US politics scholarship has often focused on electoral participation to the exclusion of other features of political life (Soss & Weaver 2017). We ought not to restrict our curiosity solely to carceral state effects on voting or other political participation when there are many other open questions about how punitive interactions with the state are experienced and strategically avoided or navigated by race-class subjugated communities. Further, major structural questions persist about how our systems of policing and incarceration came to be, and how they could plausibly change in the future, to which the literature reviewed here offers only partial answers (Miller 2008, Thorpe 2015, Gottschalk 2016, Hinton & Cook 2020). It is my hope that this review will entice readers not only to read and contribute to the specific literature I lay out here, but also to consider and potentially work on these related questions.

I have restricted this review to research on the United States, despite the existence of excellent work on the political importance of policing and incarceration in other contexts. The United States is such an outlier on many dimensions of the carceral state that I have been reluctant to lump other countries' experiences in haphazardly, but I note that there is important comparative work to be done in understanding how these systems operate in other places as well as systematically examining how they differ from patterns in the United States.

At the time of this writing, the United States is experiencing a crucial historical moment. We continue to convict and incarcerate near-record numbers of people, with incarceration rates in some states increasing even as others' decrease. We are in the midst of a reckoning with police violence and racial bias, with millions of Americans having joined protests in the wake of the killings of Michael Brown in 2014, George Floyd in 2020, and many others in between. Even as I observe intense polarization and disagreement about how these systems could or should change, I am struck by the level of public attention they currently draw. It is hard to imagine how the United States could have reached a point of such intense public scrutiny of the criminal legal system without the ongoing work of grassroots organizations and activists, many of whom are from communities heavily burdened by our current systems of policing and incarceration. So I close this article by drawing readers' attention to the political knowledge and activities of people whose lives have been shaped by policing and incarceration. It is important to understand how carceral contact can make some people less likely to participate, as I have discussed throughout this review. But it is also crucial to understand the forms of political participation that nevertheless emerge in affected communities and their implications for the future (Soss & Weaver 2017, Weaver et al. 2020). Continuing to build this understanding will require varied perspectives and empirical approaches; I look forward to learning from the results of such efforts.



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