Jailed While Presumed Innocent: The Demobilizing Effects of Pretrial Incarceration

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Attention to the American carceral state has focused largely on its bookends: policing and sentencing. Between these bookends lies an underresearched but far-reaching "shadow" carceral state, a hybrid of criminal and commercial systems that often contravenes the principles of liberty, due process, and equal protection. Pretrial detention is an iconic example. It accounts for the majority of people in local jails on a given day. Up to half of detainees will not be convicted, yet detention often lasts months and triggers significant losses. Most are detained because they are too poor to pay bail, and they are disproportionately Black. How does this widespread punitive, arbitrary, and unequal experience affect political behavior? Using administrative records and as-if random assignment of bail magistrates, we find that pretrial incarceration substantially decreases voting among Black Americans. These results point to the neglected but important shadow carceral state.

Extensive research has documented the growth of the American "carceral state" (Gottschalk 2015; Lerman and Weaver 2014). The number of citizens stopped, arrested, or incarcerated has reached record numbers, exacerbating social and political inequalities (Lerman and Weaver 2014; Western 2006; White 2019).

However, little attention has been paid to the "shadow" carceral state, a set of administrative and market practices that exist outside the system of formal judicial procedure but rely on the coercive power of government (Beckett and Murakawa 2012; see also Kohler-Hausmann 2018; Page, Piehowski, and Soss 2019; Soss and Weaver 2017). These practices occur outside the official process whereby a court weighs evidence and metes out punishment. They can deny basic freedoms and extract resources with fewer safeguards of due process, the presumption of liberty, and equal protection.

Pretrial incarceration is one such practice. It is significant for three reasons. First, it is a major component of the American carceral state. Local jails process roughly 10 million more cases a year than state and federal prisons combined, and on a given day, nearly two-thirds of those in local jails are awaiting trial (Sawyer and Wagner 2020). Second, pretrial incarceration contravenes standard civil liberties protections and imposes harsh punishment (Meares and Rizer 2020). It can average five months in many jurisdictions, even though most cases involve nonviolent charges (Sawyer and Wagner 2020; Stevenson 2018). That is nearly half the minimum punishment for involuntary manslaughter (US Sentencing Commission 2018). Yet bail hearings often last less than two minutes (Meares and Rizer 2020, 19; Scott-Hayward and Ottone 2017, 172). Furthermore, as many as half are later found not guilty or have their charges dropped (Rabuy and

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Kopf 2016). Third, the pretrial system is upwardly redistributive, benefiting economically powerful private bail companies at the expense of the accused (Page et al. 2019). The median person in a local jail is nonwhite with a preincarceration income of $16,000 a year (Gupta, Hansman, and Frenchman 2016; Page et al. 2019, 156; Rabuy and Kopf 2016).

Such experiences likely carry significant consequences for political behavior. Pretrial incarceration triggers significant losses: employment, income, eligibility for social services, education, housing, and social relationships (e.g., Dobbie, Goldin, and Yang 2018; Stevenson 2018). These are not only economic and social resources; they are also antecedents of political participation (Schlozman et al. 2012). So is trust in government, which is undermined by the experience of arbitrary, harsh punishment, especially for African Americans (Soss and Weaver 2017). In addition, it is difficult to vote while detained, and furthermore, pretrial incarceration increases imprisonment on Election Day, which is disenfranchising depending on the charge. Whether through resources, alienation from government, or incapacitation, pretrial incarceration may decrease political participation.

Despite its importance, little is known about the political effects of pretrial incarceration or the shadow carceral state. Recent studies of the demobilizing effects of the carceral state have examined incarceration only after a verdict (Burch 2011; Gerber et al. 2017; Lerman and Weaver 2014; White 2019). Yet incarceration without a verdict—imposed by a pro forma hearing, frequently on those who are innocent, enforced by extractive private actors, and often lasting months—is likely to matter even more.

Estimating the effect of pretrial incarceration is challenging. Those who experience it differ from those who do not on important covariates. To identify causal estimates, we merge voter records with over 100,000 court cases from a large natural experiment in Philadelphia County. Two features of Philadelphia's pretrial process produce this natural experiment. First, after arrest, defendants are as-if randomly assigned to one of six bail magistrates who decide whether to release the defendant and on what conditions (see fig. A1). Assignment is as-if random because defendants are automatically assigned to the magistrate on duty, and all magistrates rotate through all shifts. The magistrates thus face the same types of defendants, cases, and circumstances, overall. Second, magistrates have discretion, and they vary in their tendency to set burdensome conditions for release and thus in their rate of pretrial incarceration (Dobbie et al. 2018; Stevenson 2018). By comparing similar defendants as-if randomly assigned to relatively harsh or lenient magistrates, we can isolate the causal effect of pretrial incarceration. The Philadelphia data offer additional advantages. They provide full case records, including covariates such as crime severity and prior offenses. This avoids error-prone self-reports and omitted-variable bias (White 2019). Finally, the Philadelphia case generalizes to other large jurisdictions in its use of money bail and deference to magistrate discretion, in the typical length of pretrial incarceration, and in the overrepresentation of Black Americans among pretrial detainees (Arnold, Dobbie, and Yang 2018; Chauhan et al. 2016; Olson 2012; Pretrial Justice Institute 2009).

We find that pretrial incarceration triggered by high bail amounts reduces turnout by 11 percentage points. The effect is higher for Black defendants and null for White and Hispanic defendants. It holds among prior voters, reassuring against bias from this covariate. Being incarcerated on Election Day may play a role, although we cannot test this hypothesis definitively. The heterogeneous treatment effects do not represent causal tests of mechanisms, but they suggest who is most affected.

Even with these limitations, there is clear evidence that pretrial incarceration in the months before the election reduces turnout for Black defendants. We are not aware of political science studies that focus on pretrial incarceration (or on other shadow carceral practices). These findings suggest that studies omitting the shadow carceral state underestimate the reach of the carceral state. The effects of overall incarceration extend to a much larger population than those convicted, who have been the focus of the literature. In addition, the study has implications for racial inequality. The expansion of the carceral state has disproportionately affected Black Americans (Baumgartner, Epp, and Shoub 2018; Burch 2011; Lerman and Weaver 2014; Western 2006). We find that this racially disparate impact extends to pretrial incarceration. Thus, pretrial incarceration makes a substantial negative difference for citizens who otherwise would have exercised an important right of citizenship and gained political representation.

THE SHADOW CARCERAL STATE AND PRETRIAL INCARCERATION

The carceral state is receiving a great deal of scholarly and public attention. In recent years, many more citizens were stopped, arrested, and incarcerated than in previous decades. The number of people incarcerated in the United States is much greater than in any other democratic country (Burch 2011; Gottschalk 2015; Lerman and Weaver 2014).

However, political scientists have largely omitted a significant set of practices from their study of the carceral state (Gottschalk 2015; Page et al. 2019; Soss and Weaver 2017). The focus has been on the bookends: policing or a formal finding of guilt. Yet the modal criminal justice contact results in no criminal conviction, and most cases do not result in a jail sentence (Kohler-Hausmann 2018; Lerman and Weaver 2014). Instead, a substantial number of contacts with law enforcement result in pretrial incarceration, "the act of keeping a defendant confined during the period between arrest and disposition for the purposes of ensuring their appearance in court and/or preventing them from committing another crime" (Stevenson 2018, 514). Pretrial incarceration accounts for nearly the entire growth of the jail population since 1997 and is a chief reason the United States leads the world in the number incarcerated.[1]

Pretrial incarceration fits within the concept of the shadow carceral state (Beckett and Murakawa 2012). The shadow carceral state uses "legally liminal authority, in which expansion of punitive power occurs through the blending of civil, administrative, and criminal legal authority. In institutional terms, the shadow carceral state includes institutional annexation of sites and actors beyond what is legally recognized as part of the criminal justice system. . . . These institutions . . . have nonetheless acquired the capacity to impose punitive sanctions—including detention—even in the absence of criminal conviction" (222). Further discussion of the shadow carceral state is in appendix B.

Pretrial incarceration exemplifies the shadow carceral state in several ways. First, the threat of pretrial incarceration allows extraction by the bail industry, "one of the most important yet least understood" links between punishment and social inequalities (Page et al. 2019, 150). Pretrial incarceration exists largely because the majority of jurisdictions use cash bail (Stevenson 2018, 514). Defendants must often choose between jail and paying a high-interest bond—often, thousands of dollars—which guarantees full bail to the government should the defendant fail to appear at court (Page et al. 2019). These bonds are set so high that the typical defendant is unable to pay even the 10% required (Page et al. 2019). From those who do pay, bail generates billions per year for large insurance companies, disproportionately from disadvantaged individuals (Page et al. 2019; Rabuy and Kopf 2016).

Second, the twinned institutions of bail and pretrial incarceration impose severe punishments without a formal process of assigning guilt or innocence. For example, of those arrested in Philadelphia County, 40% were detained for an average of nearly five months (Stevenson 2018). Bail is typically decided in a hearing too brief for judges to offer reasons for their bail decisions, and bail decisions take into account the defendant's ability to pay in less than 2% of the cases studied (Scott-Hayward and Ottone 2017, 172; see also Stevenson 2018, 514). In Philadelphia, the setting of our study, most pretrial detainees could avoid pretrial incarceration by paying less than $1,000, most of it reimbursable, yet are unable to post even this amount (512). A primary justification for bail is public safety, yet in our study, most were charged with nonviolent crimes (512). Thus, those detained pretrial may incur significant resource costs and view their experience—and the criminal justice system—as highly unjust.

THE IMPACT OF PRETRIAL INCARCERATION ON BEHAVIOR

Contact with the shadow carceral state represents among the most powerful negative interactions an individual could have with government. According to a well-established literature on policy feedback, government shapes individuals' participatory antecedents, including their views of government and their place in it. Jails and prisons are punitive, authoritarian institutions that impoverish inmates and function as agents of political socialization. As Lerman and Weaver put it, "antidemocratic" and stigmatizing criminal justice policies convey to those in the system that they are "not worthy of equal citizenship" (2014, 96; see also Soss and Weaver 2017). These experiences erode trust in political actors and the American political system. If the carceral state creates "custodial citizens" who are dispossessed and disempowered (Lerman and Weaver 2014), the shadow carceral state's arbitrary, racially disparate, and extractive practices would do so at least as much.

Existing studies have offered conflicting conclusions about carceral effects. A seminal longitudinal study by Lerman and Weaver (2014) found that self-reported encounters with the criminal justice system were associated with a decline in self-reported voting. While the panel design provides some assurances against biased estimates, it cannot address unobserved time-varying confounders (Gerber et al. 2017). In addition, self-reports are subject to error; people who were incarcerated may underreport voting. Most important for our purpose, the study was unable to investigate pretrial incarceration.

Burch (2011) arrived at a different conclusion, using voting and correctional records in five states. Comparing people who had been convicted before and after the 2008 election, Burch found that conviction increases turnout in three states. Burch explains that prison may spur a "revolutionary consciousness" among those who perceive their incarceration to be "harsh or unfair" (723). However, this finding could have resulted from the historic nature of the

1. The number of Americans detained pretrial is greater than the number convicted in prison in every other country except for China, Russia, and Brazil (Walmsley 2018).

2008 election and the grassroots organizing that targeted individuals with felony convictions. Moreover, those data exclude jails, where most cases of incarceration—and pretrial incarceration—occur.

A third conclusion emerges in White's (2019) study of misdemeanor convictions in Harris County, Texas. There, incarceration (compared to noncarceral punishment, e.g., community service) reduces turnout, but only among Black defendants. This study uses administrative and voting records as well as judge severity but does not measure pretrial incarceration.

A fourth answer comes from Gerber et al. (2017), who used Pennsylvania court and voting records and compared individuals convicted of low-level felonies and sentenced to prison with observably similar individuals sentenced to probation. That analysis found null effects of incarceration.[2]

The mixed results in this literature invite additional research. Accurately identifying the causal effect of incarceration requires an exogenous measure of incarceration and administrative records of incarceration and turnout. It also requires measures of pretrial incarceration, which is unmeasured in all existing studies. We contribute to this literature by measuring pretrial incarceration, using large administrative records, and leveraging as-if random assignment to incarceration.

THE NEGATIVE EFFECTS OF PRETRIAL INCARCERATION

H1. Main hypothesis: Pretrial incarceration decreases voter turnout

Our central hypothesis is that pretrial incarceration reduces voter turnout. Several possible mechanisms explain this prediction. Our data do not allow direct causal tests of mechanisms. We elaborate on the mechanisms to explain why pretrial incarceration may matter and offer suggestive evidence from heterogeneous treatment effects.

First, pretrial incarceration triggers real costs (Gupta et al. 2016; Heaton et al. 2017; Stevenson 2018). It increases job and housing instability and family disruption and causes substantial income drops. In Philadelphia, it is associated with an average loss of $40,000 in reported earnings and government benefits and an 11% lower chance of being employed (Dobbie et al. 2018). It may demobilize defendants because it diminishes the resources that are well known to facilitate political participation (Schlozman et al. 2012). Heterogeneous effects consistent with the resources mechanism are further described in hypotheses 2 and 3 below.

Second, pretrial incarceration may have a socializing effect. The unjust aspect of pretrial incarceration may foster intense estrangement from government, since one has been deprived of basic freedom without meaningful due process (Bell 2017). Consistent with this notion, the mental health consequences of incarceration may accrue more from pretrial rather than postconviction incarceration (Sugie and Turney 2017, 733). Pretrial detention transmits a particularly striking message about the shortcomings of American government and its poor view of the defendant's worth. These experiences may erode defendants' belief in government's commitment to rights, including the exercise of a citizen's voice in the political process. A government that does not value justice and voice may create distrust and alienation from all its functions, elections included. Heterogeneous effects consistent with this mechanism are further described in hypothesis 3.

Third, pretrial incarceration may affect voting through incapacitation. It directly increases the difficulty of voting while incarcerated during an election and indirectly increases the outright inability to do so because of felony imprisonment. This process is not a violation of assumptions or a source of bias; rather, it is caused by pretrial incarceration and follows it in the sequence of time. We elaborate on this mechanism in hypothesis 4, below.[3]

H2. Resource deprivation

If pretrial incarceration decreases voting through costly loss of resources, its effect will be greatest among lower income defendants, who have fewer resources with which to alleviate economic destabilization. For example, incurring a loss of $5,000 would represent a much larger cost for defendants earning $20,000 versus $100,000 a year. We measure income by the average income in the defendant's zip code.

H3. Racially disparate impact

Several of the mechanisms predict that pretrial incarceration will vary by race, and Blacks will be especially affected, although we are unable to causally test or adjudicate among them (White 2019).

2. Many of the jurisdictions in these studies used pretrial detention extensively (Natapoff 2018). Yet existing studies classify individuals incarcerated pretrial in the no-incarceration control group. For example, the pretrial incarceration rate in Harris County is 53% by one estimate (Heaton, Mayson, and Stevenson 2017). And in Philadelphia County, for example, approximately 26% of those who would be in Gerber et al.'s (2017) untreated group had been treated pretrial. By omitting pretrial incarceration, studies may underestimate the impact of incarceration or even conclude that it has no impact.

3. Other mechanisms include misinformation and system avoidance (see app. C).

H3a. If resource deprivation drives the treatment effect, the consequences will be more severe for Black than White defendants because Black defendants have fewer resources. Black defendants have lower prearrest wealth and much less access to family assets (Page et al. 2019). Many Black Americans live in disproportionately Black neighborhoods, and those areas are more resource poor (Massey and Denton 1993). The criminal justice system triggers larger decreases in Blacks' incomes and stigmatizes them more when seeking employment (Apel and Powell 2019; Harris and Harding 2019). That is, the resource effects of incarceration are likely worse for Blacks. If racial disparities are due to resources, and Blacks are more affected because they are more likely to live in poverty, racially disparate impacts will diminish when we condition on income. Incarceration effects will be equal for Black and White defendants at every income level.

H3b. A second reason why pretrial incarceration may especially affect Black defendants is vote propensity. Blacks are more likely than Whites to have voted before incarceration (White 2019). Blacks' turnout thus has more room to decline, and incarceration may make a bigger difference for them than for Whites. We examine this hypothesis by comparing treatment effects for Black and White prior voters and for Black and White nonvoters. If racial heterogeneity remains, then it is not due to prior voting.

H3c. If political socialization explains the effect of pretrial incarceration, it will likely manifest as racialized political socialization, and pretrial incarceration will especially affect Blacks (Lerman and Weaver 2014). Through racially targeted practices, law enforcement associates nonwhite identity with inferior citizenship (Soss and Weaver 2017; see also Baumgartner et al. 2018; Mummolo 2018). These practices influence Blacks' perceptions of fairness in the criminal justice system and government institutions more broadly (Cohen 2010). Survey data reveal that "when Blacks are treated unfairly because of their race they are likely to impugn the fairness of the wider system" (Peffley and Hurwitz 2010, 55).

This logic applies even more to pretrial incarceration, a particularly harsh and racially disparate feature of the carceral state (Page et al. 2019). Black defendants face much higher bail amounts than White defendants with similar charges and conviction histories and are therefore less likely to be released pretrial (Arnold et al. 2018; see also app. H). This racially disparate reality is not difficult for detainees to perceive. A defendant incarcerated pretrial in Philadelphia on a typical day would see mostly other Black defendants (Philadelphia Research Initiative 2011). Testimonials suggest that Blacks perceive the pretrial system specifically as a racial injustice. As a Black man held pretrial for four months expressed: "If they can make a dollar off of us, they will. . . . They are not in any big hurry to get you to a trial, to get you to a judge. They make money off of you sitting in there. . . . You need to let me go. They need to incriminate real criminals. Stop the discrimination based on race, and if this person dresses a certain way. Stop degrading people and tearing people down within the system. . . . The system has no heart. It's just a zombie going around killing people, destroying lives" (Gilbert, n.d.). In sum, Black detainees may interpret the pretrial experience as exploitive and discriminatory and generalize about the unfairness of government.

H3d. These mechanisms imply moderate demobilizing effects among Hispanic defendants. Hispanics occupy a liminal position. Hispanic defendants' prearrest incomes are typically between White and Black defendants' incomes (Page et al. 2019). Some Hispanics experience targeted policing and perceive bias in the legal system, although not as strongly as Blacks (Walker 2019). Therefore, they are less likely than Blacks to interpret punitive practices as a form of structural bias against their group. We analyze the effects of pretrial incarceration separately for Hispanic defendants. However, this test is tentative because of limitations in identifying Hispanics (as discussed later).

H4. Incapacitation

Aside from resources and race, incarceration may reduce voting through incapacitation. Pretrial incarceration increases the marginal likelihood of being convicted because its cascading effects on lost employment, income, relationships, and one's ability to build a defense can lead defendants to plead guilty (Dobbie et al. 2018; Stevenson 2018). Thus, pretrial incarceration may reduce turnout by increasing the likelihood of prison time, and those serving a felony sentence during the election are prohibited by Pennsylvania law from voting. In addition, while defendants awaiting trial and those serving a nonfelony sentence are allowed to vote, it may be difficult to do so (Lerman and Weaver 2014). Because only 5.2% of the sample are incarcerated pretrial on Election Day, we cannot test this latter pathway by itself, but we can combine it with postconviction incapacitation, maximizing our power to detect overall incapacitation effects (app. T offers further detail). As a suggestive test of incapacitation, we use a mediation analysis.

To summarize, the hypotheses are as follows. Pretrial incarceration reduces voter turnout (hypothesis 1). Those most affected are lower-income defendants (hypothesis 2: resource deprivation) and Black defendants (hypothesis 3: racially disparate impact). The higher impact on Blacks may be due to more severe resource impacts, meaning racial effects will diminish conditional on income (hypothesis 3a), or due to higher baseline turnout, implying racial effects will diminish conditional on prior voting (hypothesis 3b), or due to racial political socialization (hypothesis 3c). Hispanic defendants will have an intermediate effect (hypothesis 3d). The negative effect of pretrial incarceration may be partly explained by the difficulties of (or legal prohibitions against) voting from jail or prison (hypothesis 4).

DATA ON PRETRIAL INCARCERATION AND TURNOUT

This study requires access to comprehensive, detailed court records in order to measure pretrial incarceration and account for case and defendant covariates. The administrative court data allow us to estimate pretrial incarceration effects without error-prone self-reports and omitted-variable bias (see also White 2019). Philadelphia County meets these requirements.

We use all cases filed in Philadelphia County between the 2008 and 2012 general elections (Stevenson 2018). Each observation contains defendant information (name, birth date, race, gender, zip code, the number of prior cases and convictions in Pennsylvania) and case information (arrest date, date and time of the bail hearing, bail magistrate, offense charges, pretrial conditions such as released on recognizance or monetary bail, and pretrial incarceration release date). Appendix D provides details. These data provide a rich set of covariates and directly measure pretrial detention. Following Dobbie et al. (2018) and Stevenson (2018), we measure pretrial incarceration as being detained for more than three days after the bail hearing, unless otherwise noted.

We clean the data in the following ways. First, we drop the small number of cases missing the defendant's name or birth date (52), which precludes matching with voter records, and we drop defendants who were too young to vote in the 2012 election (368). Second, we drop cases without a named bail magistrate (15,187), preliminary arraignment date (48), or pretrial release date (22), necessary for calculating the instrument.

We then merge the court records with state voter files. First, we use raw Pennsylvania files from 2009 and 2013 to measure 2008 and 2012 turnout and registration, respectively.[4] However, using Pennsylvania voter records exclusively would bias our estimates if people detained pretrial are subsequently more likely to move out of state. Therefore, we also match defendants not in the Pennsylvania voter file with the L2 uniform 2014 voter files of all remaining states. This latter approach represents a small percentage of our matched cases. See appendix E for more details.

To merge these records, we use the probabilistic method developed by Enamorado, Fifield, and Imai (2019). Its main advantage is its flexibility to account for the uncertainty surrounding the merging process (by controlling error rates). Moreover, as shown by Enamorado et al. (2019), it is robust to typographical errors and missing data and outperforms deterministic (rule-based) approaches. We merge the records using name, gender, and birth date. The estimated match probability is reweighted to account for the frequency of names: matches on common (less common) names are down-weighted (up-weighted) according to the empirical distribution of each name. In postmerge analysis, we reweight turnout and registration status by the match probabilities, to account for the uncertainty surrounding the merge and produce consistent estimates (see app. F). Altogether, the match rate is 55%. Appendix N shows a similar rate with a deterministic approach.

In sum, our main sample includes all defendants with cases filed between the November 2008 and 2012 elections, except defendants whose name, birth date, bail magistrate, or release date is unavailable in the court records or who were too young to vote in 2012. Following Gerber et al. (2017) and White (2019), our unit of observation is the defendant. For defendants with multiple cases in the time period (33%), we consider only their last case before the election. This makes for a final sample of 100,821 defendants. Finally, for those defendants with valid zip codes in Pennsylvania (89%), we augment our data with a proxy measure of defendant resources (zip code average income) from the 2008 release of the IRS's Statistics of Income.

Table G1 describes the sample. In our sample, 36% of defendants were incarcerated pretrial (detained more than three days). Detainees' pretrial jail time averaged nearly five months, with a median of 2.6 months. Their median bail was $10,000, suggesting most were unable to secure $1,000 or less. Detained and released defendants share some similarities. Both tend to be male and Black, live in poor areas, face minor and nonviolent charges, have a prior case, and are unlikely to vote.[5] There are also some differences. Compared to released defendants, detainees are more likely to be Black, poor, and male. They face somewhat more serious charges, but their charges tend to be minor nonetheless, and most did

4. We obtained all voter files from L2, Inc., a national nonpartisan firm that collects voter files from states. L2 did not clean or alter the Pennsylvania files, which match vote counts accurately (within 1.2% and 0.2% of the official counts for the 2008 and 2012 general elections, respectively).

5. In Philadelphia County, the average zip code income was $46,562 (above the sample's middle tercile), and Blacks compose 42% of the population (Philadelphia Research Initiative 2011).

not face any violent charge. Detainees are less likely to have voted than released defendants, but this gap grows posttreatment, suggesting a pretrial incarceration effect.

NATURAL EXPERIMENT

Isolating the causal effect of incarceration on voting behavior is challenging because of the endogeneity of pretrial decisions. For example, bail magistrates are more likely to release defendants who have a consistent employment record, a stable housing history, and strong ties to their community (Gupta et al. 2016). These factors are also correlated with political participation (Schlozman et al. 2012). Ordinary least squares (OLS) regression, therefore, may produce biased estimates.

To overcome this challenge, we analyze a natural experiment in Philadelphia's court system. Philadelphia County uses an as-if random process to assign defendants to bail magistrates who differ in their propensity to incarcerate defendants pretrial. Specifically, assignment is as-if random because defendants are assigned to the magistrate on duty, and magistrates rotate through all shifts. One magistrate works a particular shift for five days, then takes five days off, then works a different shift for five days, and so on. This rotation proceeds throughout weekends and holidays. Studies of this jurisdiction find no evidence of strategic manipulation or substantial deviation from the assigned schedule (Stevenson 2018, 516–19). Some magistrates are consistently more likely to set higher bail amounts that result in pretrial incarceration compared to magistrates deciding observably similar cases, as studies of this jurisdiction show (Dobbie et al. 2018; Stevenson 2018).

The decision tendencies of randomly assigned magistrates present an exogenous source of variation in pretrial incarceration. A defendant who was released by one magistrate may have been detained pretrial had that person been assigned to a magistrate with more punitive tendencies. This design identifies the local average treatment effect for defendants on the margin of incarceration and release. For these defendants, the likelihood of experiencing incarceration is not driven by confounding, preexisting characteristics. Instead, these defendants are incarcerated because of an exogenous source of variation: the randomly assigned magistrate's decision tendencies.

CONSTRUCTING THE INSTRUMENT

Following Aizer and Doyle (2015) and Dobbie et al. (2018), we construct an instrument using the as-if random assignment of bail magistrates to cases. The instrument leaves out the focal case and uses all the other cases seen by the same magistrate in the same time period. Similarly to Aizer and Doyle (2015) and Stevenson (2018), we allow our instrument to vary by case severity and year. For example, a magistrate who is more lenient than others on low-level offenses may be harsh on severe offenses. Such heterogeneity in magistrate tendencies has been documented in our setting (Stevenson 2018).[6] To code case severity, we sum the Pennsylvania Offense Gravity Scores across the offenses in the case and bin them into terciles (see app. D). Thus, for a given defendant, the instrument represents the proportion of other cases of a similar severity level decided by the same magistrate in the same year that resulted in pretrial incarceration. Specifically, as in Aizer and Doyle (2015) and Dobbie et al. (2018), we construct our instrument using the following equation:

$$Z_{dtjh} = \frac{\left(\sum_{k=1}^{N_{tjh}} \sum_{c=1}^{N_{ktjh}} P_{kctjh}\right) - \sum_{c=1}^{N_{dtjh}} P_{dctjh}}{N_{tjh} - N_{dtjh}}, \qquad (1)$$

where N_{tjh} is the number of cases seen by magistrate j at year t and case severity h, N_{dtjh} is the number of cases where defendant d was involved and seen by magistrate j at year t and case severity h, and $P_{dctjh} \in \{0, 1\}$ represents the decision (detained $= 1$ or released $= 0$) made by magistrate j in case c for defendant d at year t and case severity h. Our final sample includes six magistrates per year, except 2008 and 2009 when there were two vacancies (see app. M). The median number of cases per magistrate by year is 6,569.5, and the median number of cases per magistrate by year by Offense Gravity Score tercile is 2,031. The leave-out-case pretrial detention rate ranges from 0.06 to 0.71, with an average of 0.35 and a standard deviation of 0.20. Moving from the most to the least lenient magistrate increases the likelihood of pretrial detention by 14 percentage points for defendants in the lowest tercile of offense severity, almost 16 points for those in the middle tercile, and 18 points for those with the most serious offenses.

To illustrate the relationship between magistrate leniency and our variables of interest, figure H1 presents the nonparametric fit between the residualized instrument and the residualized pretrial detention (*left panel*) and residualized turnout (*right panel*). As expected, the figure shows a strong and positive relationship between our instrument and pretrial incarceration and a negative relationship (via reduced form) between our instrument and turnout. The figure also shows the residualized distribution of the instrument, confirming that the tails of the distribution do not drive the main

6. We find similar results when constructing instruments separately for Black and White defendants. This accounts for the possibility that magistrate leniency depends on race. We do not rely on this specification because of its limitations (see app. H).

effect. We find sufficient variation in the instrument to predict the endogenous variable (pretrial incarceration) and the outcome (turnout), motivating further analysis.

EMPIRICAL STRATEGY

As described above, we use two-stage least squares (2SLS) to estimate the impact of pretrial detention on turnout. Specifically, the first stage is

$$P_{dtjh} = \alpha_0 + \alpha_1 Z_{dtjh} + X_{dt}^{\top}\Omega + \varepsilon_{dtjh}, \tag{2}$$

and the second stage is

$$T_{d,2012} = \beta_0 + \beta_1 \widehat{P_{dtjh}} + X_{dt}^{\top}\Lambda + \varepsilon_{dtjh}, \tag{3}$$

where d indicates defendant, j is for magistrate, h is the offense severity level, $T_{d,2012}$ is an indicator for voting in 2012, P_{dtjh} is an indicator for being detained pretrial more than three days, Z_{dtjh} is the instrument, and X_{dt} is a set of defendant and case covariates and fixed effects.[7]

ASSESSING THE INSTRUMENT'S VALIDITY

We now present evidence that our instrumental variable approach satisfies the required assumptions for a valid instrument: exogeneity, relevance, monotonicity, and exclusion. Violations of these assumptions can lead to biased estimates.

We discuss each assumption in turn. First, our instrument must be exogenous, meaning that the likelihood that a defendant faces a harsh or lenient judge is uncorrelated with preexisting characteristics. If the instrument is not exogenous, observed associations between pretrial incarceration and turnout may reflect, for example, defendant or case-related confounders. As noted by Stevenson (2018), if our instrument is exogenous, we should observe that case and defendant characteristics are distributed evenly across magistrates with different decision tendencies and should not be predictive of the instrument. We find that most covariates are not significantly related to the instrument (see table H2). While some covariates on their own are significant predictors of the instrument and the covariates are jointly correlated with the instrument (joint $F = 3.320$), the magnitude of those correlations is close to zero. To further test for potential violations of randomization, we construct a measure of predicted turnout using only the demographic and case-level covariates, so that all the variation explaining turnout is coming from these covariates. As the flat red line in figure H2 shows, there is no sizable correlation between this measure and the residualized instrument, as expected ($r = -0.003$). Finally, to check that omitting interactions between fixed effects in the randomization process does not lead to any biases, table H3 replicates the results with interacted time fixed effects, following Dobbie et al. (2018).

Second, our instrument should be a strong predictor of pretrial detention (relevance). Otherwise, the analyses could suffer from weak instrument bias. Table I1, row 1, presents the first-stage estimate, from three models with different sets of covariates. There is a strong positive relationship, nearly one to one, between pretrial detention and the instrument: a 1 percentage point increase in the instrument translates into a 0.80 point or more increase in the likelihood of pretrial detention. The first-stage F-statistic ranges from 422 (in the model with fixed effects only) to 448 (in the model including fixed effects, demographic and case covariates).

Third, our instrument must be monotonic, meaning assignment to a more punitive magistrate should increase defendants' probability of pretrial incarceration regardless of their characteristics. If our instrument does not meet the monotonicity assumption, then it is not possible to identify the effect on compliers (Angrist and Pischke 2008). While no direct test of the monotonicity assumption exists, we can at least provide evidence that the instrument satisfies "average monotonicity" (Frandsen, Lefgren, and Leslie 2019). Tables I1 and I2 present the first-stage coefficients across a variety of subsamples. Assignment to stricter magistrates substantially increases the likelihood of pretrial detention across a wide variety of characteristics. To alleviate concerns about monotonicity violations of the instrument (that the instruments work differently for some subgroups), we follow Aizer and Doyle (2015) and Stevenson (2018) in allowing the leniency of the judge to vary by time and case severity.

Finally, our instrument must meet the exclusion restriction, which requires that the treatment—assignment to a magistrate with a particular tendency—affects the outcome (turnout) only through the pretrial decision (release or incarceration pretrial). If the magistrate affects a defendant's political behavior through other channels, this assumption would be violated, and other unobserved confounders could drive our result. As noted by Stevenson (2018, 516–19), several factors lend support for the validity of the exclusion restriction. First, arrestees have no agency in selecting their bail magistrate. Second, bail magistrates' interactions with defendants are brief (less than two minutes on average), leaving minimal time for comments aside from release conditions. Finally, bail magistrates have no further interaction with defendants or jurisdiction over cases after the preliminary

7. Defendant covariates are age, age^2, gender, race, pretreatment turnout in 2008, voting age ineligible in 2008, and pretreatment registration. Case covariates are drug, DUI (driving under the influence), violent, firearm, and property charge and had prior case. Fixed effects (not combined) are bail hearing year, month, day of the week, shift, and case severity tercile.

pretrial decision, meaning their pretrial decision is the only plausible way that they could affect the defendant.

MAIN EFFECT

Figure 1 presents the second-stage coefficients from equation (3), that is, the effect of instrumented pretrial incarceration on 2012 turnout. This represents the local ("complier") average treatment effect. Pretrial incarceration leads to an 11 percentage point decrease in the probability of voting, with full controls (the baseline turnout rate for our complete sample is 28 percentage points). The effect is similar with fewer controls (see fig. 1 and table J1) and with bivariate probit (biprobit; see table K1).[8]

Appendixes M and N present robustness checks. We find similar results with an alternative (residualized) instrument specification; a continuous measure of pretrial incarceration (logged number of days) or alternative cut points (5, 7, 10, or 14 days); alternative case covariates; a deterministic (rather than probabilistic) merge; and only in the period without magistrate vacancies, when the number of magistrates in the rotation is constant and the percentage of cases with missing magistrate data is too small to introduce selection bias. In addition, to address the possibility that heteroskedasticity-robust standard errors are not sufficiently conservative (and results are due to chance), we conduct several robustness checks. We conduct a permutation test that nonparametrically accounts for potential within-magistrate clustering of cases across time. We also cluster at other levels (see tables M6 and M7). In all of these analyses, we find consistent results. Finally, we conduct a placebo test of reverse timing. Being detained after the 2008 election should not predict voting in the 2008 election. We regress voting in 2008 (an outcome measured pretreatment) on the treatment instrument (table M4). As expected, the treatment does not predict the pretreatment outcome.[9]

As discussed above, the estimated effects are for *compliers*, that is, those individuals at the margin of being incarcerated pretrial. In appendix O, we compare compliers to the overall sample (Dahl, Kostol, and Mogstad 2014). We find that compliers are within 3 percentage points of the sample average on gender, race, turnout, registration, prior case status, and each charge. Thus, compliers are not markedly different, supporting the generalizability of the effects. Tables O2–O4 show a similar result when we compare the covariates of compliers within each covariate subset, and thus, results among subgroup compliers are likely to be representative of subgroups.

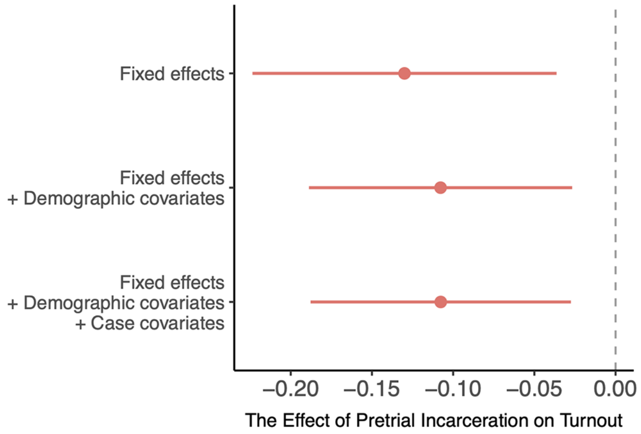

Figure 1. Effect of pretrial incarceration on 2012 turnout. Second-stage 2SLS estimates with 95% confidence intervals from models that include fixed effects only (*top*), fixed effects and demographic covariates (*middle*), and fixed effects, demographic, and case-level covariates (*bottom*). Heteroskedastic-consistent standard errors are used to construct the confidence intervals.

RESOURCE DEPRIVATION

If pretrial incarceration affects voting through resource deprivation, the magnitude may be greater among lower-income defendants, who have few resources to mitigate the socioeconomic repercussions. This requires a test for heterogeneous treatment effects by income. We code defendants with zip codes in Pennsylvania into terciles of average zip code income: bottom (<$25,888), middle ($25,888–$34,090), or top (>$34,090).[10] To keep the 2SLS model identified, we interact income terciles with leniency in the first stage and with predicted pretrial incarceration in the second stage (see app. J). We conduct two-tailed significance tests on these estimates (in table J1) and also use them to calculate marginal effects (in fig. 2). Figure 2*A* presents the marginal effect of pretrial incarceration on turnout for each income tercile, derived from the interaction model in the third column of table J1 (the specification that controls for fixed effects and case and demographic characteristics). The demobilizing effect of pretrial incarceration is acute among defendants in the bottom and middle terciles. For such defendants, pretrial

8. We do not use biprobit extensively because it can be sensitive to heteroskedasticity and computationally expensive because of the joint distributional assumptions (Chiburis, Das, and Lokshin 2012). In practice, biprobit produces similar results to 2SLS but is not as robust to misspecification of the first stage (Angrist and Pischke 2008). OLS estimates show a 7–11 point effect (table L1).

9. As an additional robustness check, we assess a related outcome—registering to vote posttreatment—among those not registered pretreatment (39,751 defendants). The negative effect on registration is of similar magnitude (15 percentage points), although imprecise (table M5).

10. Even upper-tercile defendants are lower-income compared to local and national income.

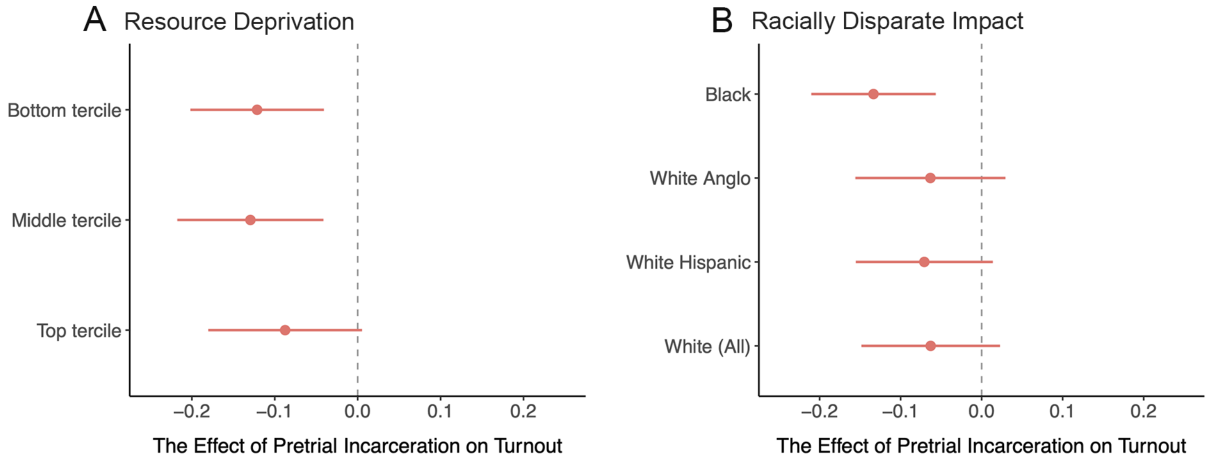

Figure 2. Effect of pretrial incarceration on 2012 turnout by income (*A*) and race (*B*). Marginal effects based on the second-stage 2SLS estimates of the models with interactions between pretrial incarceration and income (*A*) and between pretrial incarceration and race (*B*). 95% confidence intervals from models that include fixed effects, demographic, and case-level covariates. Heteroskedastic-consistent standard errors are used to construct the confidence intervals.

incarceration reduces turnout by 12 percentage points. In the top tercile, the effect is −9 percentage points but cannot be statistically distinguished from zero. The effects in the bottom and middle terciles are not statistically distinguishable from one another ($p < .54$), whereas the effects on top-tercile defendants are statistically distinguishable from both middle- ($p < .002$) and bottom-tercile defendants ($p < .02$). These results are consistent with the resource mechanism: pretrial incarceration especially affects defendants less able to withstand socioeconomic destabilization. To be sure, we cannot adjudicate whether income itself or factors correlated with income explain the income heterogeneity, and average zip code income is a noisy measure of individual resources.

RACIALLY DISPARATE IMPACT

Pretrial incarceration may especially demobilize Black defendants, for reasons detailed above. Again, we estimate an interaction model (table J1) and display marginal effects in figure 2.[11] Pretrial incarceration clearly has a racially disparate impact. As table J1 and figure 2*B* show, pretrial incarceration decreases turnout by 13 percentage points among Black defendants. This effect passes conventional thresholds of statistical significance. By contrast, the effects for White, White Hispanic, and Anglo defendants are smaller by about half and, as shown in figure 2, indistinguishable from 0. The effect for Blacks is significantly different from the effects for Whites, White Anglos, and White Hispanics (see table J1). Black defendants are especially affected by pretrial incarceration.

Are Black defendants especially affected because they tend to be poor? If so, pretrial incarceration would affect poor Blacks and Whites equally, and income would matter for each racial group. To test this racialized resources hypothesis, we examine heterogeneity in treatment effects for Blacks and Whites at each income tercile, using a triple interaction model (see table P1). Contrary to the racialized resource hypothesis, the racial disparity persists at each income level. The effect for Blacks in the lowest income group is large and statistically significant (−14 percentage points; SE = 4 percentage points). That effect drops by a statistically detectable magnitude among Whites (−7 percentage points; SE = 2 percentage points), representing a 50% decrease by race. In other words, the lowest-income defendants are doubly affected if they are Black. Moreover, in this analysis, income has no effect within racial groups: all the interaction terms for middle and high income are small and nonsignificant. Figure 3 illustrates this finding more clearly. In sum, the effect of income previously seen in figure 2 may be mostly the result of race. Pretrial incarceration may not affect Black defendants because of their lower income, and Black defendants may be more affected than White defendants whether or not they are poor.[12] We refrain from drawing stronger conclusions from this test, as our average zip code income measure is more prone to measurement error.

The strong effect on Black defendants may be driven by Blacks' higher baseline turnout (White 2019). Such higher turnout is evident in our data (table Q1). If Blacks are affected

11. We rely on surname prediction to identify Hispanic defendants (see app. D).

12. The effect among Blacks and Whites is significantly different in the bottom and top terciles ($p < .01$) but not in the middle tercile ($p < .14$; see table P1). We caution against inferring that income is entirely irrelevant. This sample has much lower income than the Philadelphia population. In a sample with population levels of income variation, income may matter much more.

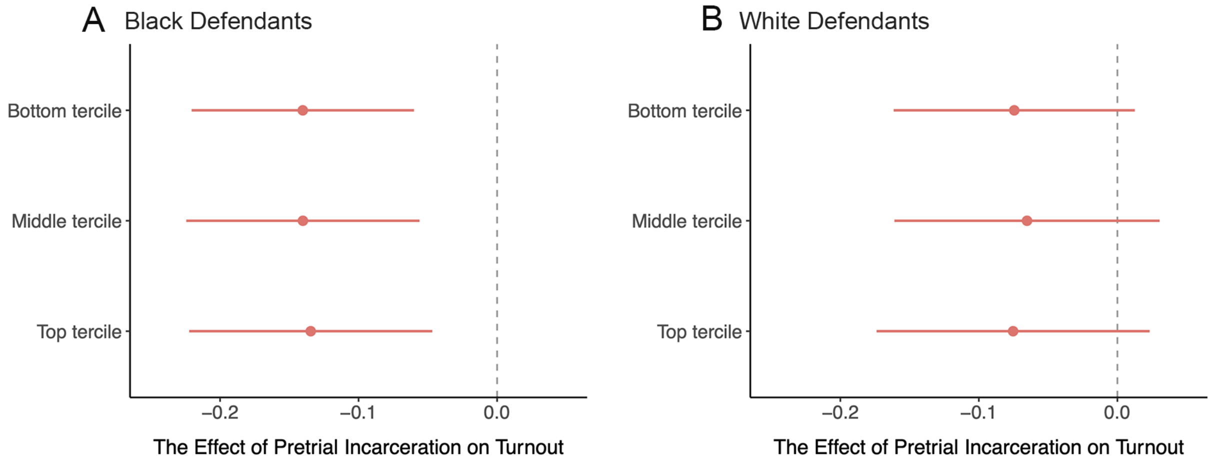

Figure 3. Effect of pretrial incarceration on 2012 turnout by race-income groups. Marginal effects for Black (*A*) and White defendants (*B*) based on the second-stage 2SLS estimates of models with interactions between pretrial incarceration, income, and race. 95% confidence intervals from models that include fixed effects, demographic, and case-level covariates. Heteroskedastic-consistent standard errors are used to construct the confidence intervals.

because they are more likely to be voters, we would find equally large treatment effects for White and Black prior voters. However, a triple interaction model disconfirms this prediction (table Q2). The effects are significantly more negative on Black than White prior voters ($p < .01$). They are also statistically different for Black than White nonvoters ($p < .01$). Thus, the racially disparate effect exists among those with the same voting history.[13] In sum, race substantially conditions the effect even when accounting for prior voting.[14]

TIMING AND THE ROLE OF INCAPACITATION

Next, we examine the temporal dynamics of the effect, as a preliminary exploration of incapacitation. As discussed above in hypothesis 4, in Philadelphia, while pretrial detention and incarceration for a nonfelony conviction do not automatically disenfranchise a defendant, they likely make it more difficult to cast a ballot. In addition, those incarcerated postconviction for a felony conviction during the election are not allowed to vote.[15] If incapacitation explains the effect, then we may see a time gradient. We interact our instrument with year-quarter fixed effects and plot the coefficients in figure 4. As shown, the negative impact of pretrial incarceration is concentrated in the four quarters before Election Day.[16] This is consistent with the possibility that the impact of pretrial incarceration does not operate through long-term resource losses or constant, fixed socialization. It is consistent with shorter-term factors, such as short-lived resource losses, decaying socialization, election salience, or incapacitation. That said, this analysis confounds various time-varying and defendant-level covariates. It cannot isolate any one of them, determine whether long-term factors indeed decay, or choose among potential shorter-term factors. It merely describes how the effect varies with case timing.

To more directly explore whether the effect of pretrial incarceration could be driven in part by incapacitation, we use causal mediation analysis. Our goal is to separate the direct and indirect effect (through incapacitation) of pretrial incarceration on turnout. We use the framework of Dippel et al. (2020) for mediation analysis with one instrument. We combine pretrial and postconviction incapacitation, to maximize our power to detect the incapacitation effect. Using this measure, 8.7% of the sample is incapacitated (see app. T for details).

As shown in table 1 (panel A), the indirect effect of pretrial incarceration through incapacitation is large but not statistically significant. Moreover, pretrial incarceration continues to exercise a substantial, precisely estimated direct effect (not accounted for by incapacitation). In other words, if incapacitation matters, we cannot detect that, and pretrial

13. In addition, while the −8 percentage point effect among Black nonvoters is close to being distinguishable from zero ($p < .055$), the −4 percentage point effect among White nonvoters is not ($p < .40$).

14. In app. R, we test this explanation using prior registration. We similarly find larger effects on registered Black than White defendants, and the difference passes significance thresholds.

15. Pretrial incapacitation is more prevalent than postconviction incapacitation (5.2% vs. 3.5%).

16. Table S1 confirms this result with a prospective analysis that drops cases close to Election Day. Here again, we caution that case timing is confounded with many factors.

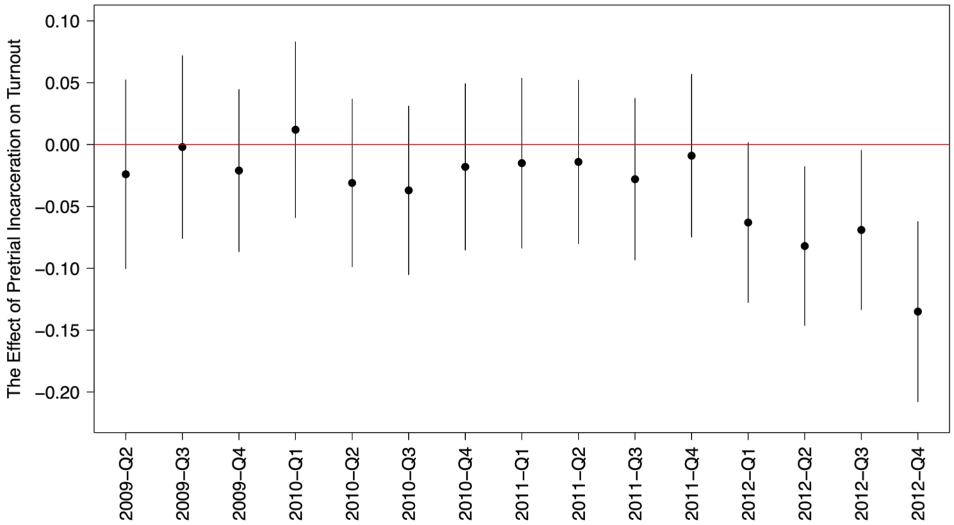

Figure 4. Dynamics of the effect of pretrial detention on turnout. Coefficients and the corresponding 95% confidence interval associated with the interaction between quarter-year of the bail hearing and pretrial incarceration. Quarter 1 of 2009 is the reference category. Model includes fixed effects, demographic, and case-level covariates. Heteroskedastic-consistent standard errors are used to construct the confidence intervals.

incarceration also matters in other ways. For cases closer to Election Day, table 1 (panel B) shows roughly similar magnitudes, but the smaller sample reduces precision, and selecting on timing introduces confounds, as noted above. In sum, the data do not offer definitive evidence about time-varying mechanisms or the role of incapacitation but suggest future research might examine each.

CONCLUSION

The American carceral state has received growing scholarly attention in recent years. Yet some of its most distinctive and consequential facets have received little notice in political science. These practices and rules function outside the formal system of criminal justice. They involve links between private economic actors and public bureaucrats that diminish the system's accountability, administrative rules that circumvent robust constitutional protection, and subjects who are disproportionately poor and nonwhite.

In this article, we focused on two intertwined aspects of this shadow carceral state: the bail system and pretrial incarceration. Pretrial incarceration is a prevalent and iconic feature of the shadow carceral state. Like much of the shadow carceral state, pretrial incarceration carries implications for social inequalities (Soss and Weaver 2017). Does this racially

Table 1. Incapacitation Mechanism: The Mediated Effect of Pretrial Incarceration on 2012 Turnout

	Direct Effect (Pretrial Incarceration)	Indirect Effect (Incapacitation)	Total Effect
A. November 2008–November 2012	−.059	−.049	−.108
	(.011)	(.042)	(.041)
% Incapacitated	8.7		
% Pretrial incarcerated	35.5		
N	100,821		
B. November 2011–November 2012	−.052	−.071	−.124
	(.119)	(.137)	(.068)
% Incapacitated	15.7		
% Pretrial incarcerated	40.7		
N	33,500		

Note. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Incapacitation takes 1 if (*a*) the individual's case reached a disposition before the election, there was a minimum sentence of 1 day or more, and the estimated sentence release is after Election Day or (*b*) their estimated pretrial release date after the preliminary arraignment hearing is past Election Day. Specifications include the instrument, case- and demographic-level covariates, and fixed effects. Heteroskedasticity-consistent standard errors are presented within parentheses.

disparate experience affect political behavior? To date, studies have not examined this question.

Using a large administrative data set, we found that defendants as-if randomly assigned to harsher magistrates in Philadelphia are more likely to be incarcerated pretrial and emerge with a lower voting propensity, especially if they are Black. Moreover, pretrial incarceration makes a difference by reducing voting even by people who had voted before. The findings likely generalize to many other large jurisdictions with substantial inequalities, where poor Black residents tend to experience harsh contact with the shadow carceral state (Hood and Schneider 2019). The demobilizing effect of pretrial incarceration raises difficult questions for a democracy whose core value is to deny liberty and disenfranchise only with due process and equal justice. The findings suggest that research on the justice system should explicitly account for pretrial incarceration.

Pretrial incarceration is only one aspect of the shadow carceral state. Other aspects include the collection of consumer debt, legal financial obligations, and child support payments and the operation of parole systems. For example, in some jurisdictions, private debt collectors coerce repayment by leveraging civil contempt of court orders against debtors, and county clerk offices enforce payment plans by garnishing assets and requesting court-issued arrest warrants. Both can result in forms of incarceration (Beckett and Murakawa 2012). Such practices are increasingly common, and they reach far and deep into the lives of Americans. They operate with weaker evidentiary standards and protections of democratic principles, from due process and entitlement to legal representation to the public's ability to hold actors accountable. They likely carry significant effects for political engagement and representation.

While pretrial incarceration—and perhaps other aspects of the shadow carceral state—have a demobilizing effect on voting, they may have mobilizing effects on other civic behaviors or attitudes (Walker 2019). If declining to vote is an act of active avoidance or even resistance, it may go hand in hand with oppositional collective consciousness (Weaver, Prowse, and Piston 2020, 616). Whether the shadow carceral state mobilizes in some ways while it demobilizes in others is a question for further research.

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