Jailed While Presumed Innocent: The Demobilizing Effects of Pretrial Incarceration[1]

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Abstract

Attention to the American carceral state has focused on its bookends: policing and sentencing. Between these bookends lies an under-researched 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 jail 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 and poor Americans. These results point to the neglected but important "shadow" carceral state.

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It is well-established that the carceral state has become a central element of the American political system (Gottschalk 2015; Lerman and Weaver 2014). The number of citizens stopped, arrested, or incarcerated has reached record numbers (Lerman and Weaver 2014). The unparalleled reach of the police and the prison has significant consequences for social and political inequalities (Lerman and Weaver 2014; Western 2006; White 2019).

However, much of the literature has yet to grapple with 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 the state (Beckett and Murakawa 2012; see also Kohler-Hausmann 2018; Page et al. 2019; Soss and Weaver 2017). These practices occur outside the official process whereby a court weighs evidence and metes out punishment. They can be highly punitive and authoritarian, denying basic freedoms and extracting resources. The defining character of the shadow carceral state is that it contravenes the principles of due process, the presumption of liberty, and equal protection.

Pretrial incarceration is one such practice. It is significant for several 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 prison combined, and on a given day, nearly two-thirds of those jailed are awaiting trial (Sawyer and Wagner 2020). Pretrial incarceration also accounts for nearly the entire growth of the jail population since 1997 and is a chief reason the US leads the world in the number incarcerated.[1] It accounts for much of the massive size of the carceral state.

[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).

Second, pretrial incarceration conflicts with the standard criminal justice protections of due process and prohibitions against excessive punishment (Meares and Rizer 2020). It can average five months in many jurisdictions, though most cases involve nonviolent charges (Sawyer and Wagner 2020; Stevenson 2018). That is nearly half the minimum punishment for involuntary manslaughter (U.S. Sentencing Commission 2018). Yet individuals can be detained based on a "minimal amount of evidence" presented in a bail hearing that often lasts less than 2 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 Kopf 2016). In these ways, pretrial incarceration violates notions of fairness and justice governing the *formal* carceral state.

Third, the pretrial system is extractive, relying on cash bail. It uses the state's monopoly of force to the benefit of publicly unaccountable, economically powerful, private bail companies, at the direct expense of the accused (Page et al. 2019). It allows private actors to share in the coercive power of the state for upward redistribution.

Finally, pretrial incarceration is typically imposed on poor or nonwhite defendants. The median person in local jail is nonwhite and has a pre-incarceration income of $16,000 per year (Gupta et al. 2016; Page et al. 2019, 156; Rabuy and Kopf 2016). The bail system mostly incarcerates those too poor to pay bail.

Such experiences likely carry significant consequences for political behavior. Pretrial incarceration triggers a cascade of losses: employment, income, eligibility for social services, education, housing, and social relationships (e.g., Dobbie et al. 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 (Soss and Weaver 2017). Whether through diminished resources or alienation from government, the shadow carceral state may undermine political participation.

Despite its importance and likely impact, little is known about the political effects of pretrial incarceration, or the shadow carceral state generally. The particularly anti-democratic character of pretrial incarceration may undermine participation in civic life. Yet to date, no studies have asked whether pretrial incarceration depresses political engagement. Recent studies of the demobilizing effects of the carceral state have found mixed results, but they examine 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, often lasting months, frequently while innocent, and enforced by extractive private actors – is likely to matter even more.

To directly measure pretrial incarceration, we use a large administrative dataset of 90,590 cases from a natural experiment in Philadelphia. Philadelphia County uses an as-if-random process to assign defendants to bail magistrates who differ in their propensity to incarcerate defendants pretrial. It also provides full case records, including covariates such as crime severity and prior offenses (Dobbie et al. 2018; Stevenson 2018). The data thus allow us to estimate pretrial incarceration effects without error-prone self-reports and omitted-variable bias (see also White 2019). The Philadelphia case generalizes to the many other large jurisdictions that disproportionately incarcerate poor people of color pretrial.

We find that pretrial incarceration triggered by high bail amounts reduces turnout by 10.5 percentage points. The effect is higher for Black and low-income defendants, and null for White and Hispanic defendants. It holds among first-time defendants and prior voters, reassuring against bias from these covariates. It is not explained by being in jail or serving a sentence during the election. These findings suggest that studies omitting the shadow carceral state under-estimate the reach of the carceral state.

This paper also helps adjudicate among mixed findings on the impact of punitive practices on turnout (Burch 2011; Gerber et al. 2017; Lerman and Weaver 2014; White 2019). Some of the inconsistency may be due to the misclassification of pretrial incarceration as non-incarceration. Causal estimates from studies of posttrial incarceration under-estimate the overall impact of incarceration, since they rely on a baseline that experiences significant incarceration pretrial. In addition, the effects of overall incarceration extend to a much larger population than those convicted, who have been the focus of the literature.

Finally, the study has implications for racial inequality. The expansion of the carceral state has disproportionately disadvantaged Blacks (Baumgartner et al. 2018; Burch 2011; Lerman and Weaver 2014; Western 2006). This racially disparate impact extends to voting. Pretrial incarceration especially reduces voting by Blacks who previously voted. It thus 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 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. The rise of the American carceral state poses substantial barriers to equality, democracy, and human rights (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). As noted above, pretrial incarceration accounts for much of the prevalence and growth of incarceration in the United States.

Pretrial incarceration exists largely because "the vast majority of jurisdictions use a money bail system" (Stevenson 2018, 514). Crucially, most individuals assessed bail are unable to pay the full amount. They must choose between jail and a high-interest, private bail bond which guarantees the full bail to the government should the defendant fail to appear at trial (Page et al. 2019). These bonds are often set so high that the typical defendant is unable to pay even the 10% required by bail bonds companies (Page et al. 2019). 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 an average of nearly five months (Stevenson 2018).

The rise of pretrial incarceration is part of the general turn by the courts away from an "adjudicative" model toward a "managerial" model (Kohler-Hausmann 2018, 4-5). In the adjudicative model, courts apply established procedure to decide whether an individual committed a behavior defined by law as a crime, the level of blame the accused deserves, and the legally prescribed punishment. By contrast, in the managerial model, courts use more informal tools of social control: constructing records that handicap individuals for years and are difficult to amend; imposing procedural hassles with heavy costs; or assessing a person's criminal character by whether they comply with these procedural requirements. In the managerial model, punishment is often triggered by behavior that does not meet the legal definition of a crime. The person is not found guilty nor sentenced to jail. Yet the courts nevertheless impose substantial penalties, through mandatory fees, stigma, or the chain reaction of missed work, lost employment and income, relationship friction, and eviction.[2]

This managerial model 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." (Beckett and Murakawa 2012, 222).

Pretrial incarceration exemplifies the shadow carceral state. The threat of pretrial incarceration allows predation by the bail industry, "one of the most important yet least understood" links between punishment and social inequalities (Page et al. 2019, 150). Bail is a distinctive and economically consequential feature of the American carceral state. It generates billions per year for large insurance companies, profits disproportionately extracted from communities and individuals disadvantaged by race, class, and gender (Page et al. 2019; Rabuy

[2] The Eighth Amendment explicitly prohibits "excessive" bail and fines. However, the Supreme Court has ruled that "inability to afford bail does not make it 'excessive' under the Eighth Amendment" (Natapoff 2018). Nor has the Supreme Court yet invalidated unaffordable bail as a violation of the 14th Amendment's equal protection clause (Scott-Hayward and Ottone 2017).

and Kopf 2016). Bail is typically decided in a hearing lasting less than 2 minutes, where judges rarely offer reasons for bail decisions and only 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). Bail is often thousands of dollars, an amount out of reach for low-income individuals even with bail bonds (Rabuy and Kopf 2016). 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 (Stevenson 2018, 512). A primary justification for bail is public safety, yet in our study, most were charged with nonviolent crimes (Stevenson 2018, 512).

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, discriminatory, and extractive practices would do so at least as much.

However, studies to date have offered conflicting conclusions about the effects of the carceral state. Incarceration may – or may not – reduce political engagement. Here we argue that these inconsistent findings are partly due to the omission of pretrial incarceration. By counting people who were detained pretrial as non-incarcerated, studies may under-estimate the overall impact of incarceration.

Some studies find that the carceral state demobilizes citizens. 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, people who were incarcerated may under-report voting. Finally, the study is unable to investigate pretrial incarceration.

Burch 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, she 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" (2011, 723). However, this finding could have resulted from the historic nature of the 2008 election and the grassroots organizing that targeted former felons. Moreover, that data excludes jails, where most cases of incarceration – and pretrial incarceration – occur.

A third conclusion emerges in White's study of misdemeanor convictions in Harris County, Texas. There, incarceration (compared to non-carceral 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. Because the pretrial incarceration rate in Harris County is high (53% by one estimate), omitting pretrial incarceration will under-estimate the effect of incarceration (Heaton et al. 2017).

A fourth answer comes from Gerber et al. (2017), who found null effects using Pennsylvania court and voting records. They compared low-level felons sentenced to prison with observably similar felons sentenced to probation. They concluded that incarceration did not affect turnout. However, this estimate is likely downwardly biased by pretrial incarceration. In Philadelphia County, for example, 26% of those who would be in Gerber et al.'s untreated group had been treated pretrial (2018, 29).

We address the conflicting findings by measuring pretrial incarceration, and using large administrative datasets that do not depend on self-reports or small or unusual samples. 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. By omitting pretrial incarceration, studies may under-estimate the impact of incarceration, or even conclude that it has no impact.

The Negative Effects of Pretrial Incarceration

Our argument is that pretrial incarceration is a costly and alienating experience that reduces voting. Specifically, we test the following hypotheses.

H1. *Main Hypothesis: Pretrial Incarceration Decreases Voter Turnout*

Our central hypothesis is that pretrial incarceration reduces post-release voter turnout. Several possible mechanisms explain this prediction. 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 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). We offer partial tests of the resources mechanism as described further in H2 and H3 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 post-conviction 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. We offer a partial test of this mechanism, as discussed in H3.

Third, pretrial incarceration may affect voting through downstream conviction. Pretrial incarceration increases the marginal likelihood of being convicted (Dobbie et al. 2018; Stevenson 2018). Its cascading effects on lost employment, income, relationships, and ability to build a defense can lead defendants to plead guilty. Pretrial incarceration may reduce turnout by triggering prison time, which in turn may reduce turnout. 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 offer a partial test of this mechanism in H4, below.[3]

[3] Three other mechanisms are also possible: misinformation, system avoidance, and mechanical effects (detained pretrial during the election). Pretrial detention during the election is unlikely to explain the effect, as it only affects 5% of the sample. We discuss these in Appendix A.

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. While we are unable to adjudicate among most of these mechanisms, all nonetheless imply that race will condition the impact of pretrial incarceration, with Blacks especially affected (White 2019).

H3a: If resource deprivation drives the treatment effect, the consequences will be more severe for Black than White defendants. The criminal justice system triggers larger decreases in Blacks' resources, stigmatizes them more when seeking employment, and interferes to a greater extent with their transition to adult roles that facilitate political participation (Apel and Powell 2019; Harris and Harding 2019). Black defendants also have lower pre-arrest wealth and access to family assets (Page et al. 2019). That is, the resource effects of incarceration are likely worse for Blacks, and Blacks may be more politically demobilized as a result. If pretrial incarceration reduces voting because of its racialized resource impact, it would especially affect Blacks in poor neighborhoods.

H3b: 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 reveals 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 are less likely to be released pretrial, and face much higher bail amounts than White defendants with similar charges and conviction histories (Arnold et al. 2018).[4] Moreover, the pretrial process signals that race is salient. For example, a defendant incarcerated pretrial in Philadelphia on a typical day would see mostly other Black defendants (Philadelphia Research Initiative 2011). Testimonials further suggest that Blacks perceive the pretrial system specifically as a racial injustice. As a Black man held pretrial for four months put it:

> "... If they can make a dollar off of us, they will. I had a bond of $25,000. It was ridiculous. I couldn't afford it. 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... I have a life outside of these walls. 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.).

That is, Black detainees may interpret the pretrial experience as exploitive and discriminatory, and generalize about the unfairness of government.

[4] Approximately $9,923 higher (Arnold et al 2018). See also Appendix Q.

H3c: Prior voting is a final explanation for racially disparate effects. Because Blacks face a lower bar for pretrial incarceration, Black and White defendants likely differ on unobserved covariates. Among these covariates, the literature points especially to pretreatment turnout: 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 test whether prior turnout explains the larger incarceration impact on Blacks, by comparing treatment effects for prior voters and nonvoters.

H3d: The resources and socialization mechanisms imply demobilizing effects among Hispanic defendants, though attenuated relative to Black defendants. Hispanic defendants' pre-arrest 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, though not as strongly as Blacks (Walker 2019). These factors imply Hispanics will be moderately affected. We analyze the effects of pretrial incarceration separately for Hispanic defendants. However, our test is tentative because of limitations in identifying Hispanics (as discussed later on).

H4. *Incarceration Sentence*

The last mechanism is post-conviction incarceration (White 2019). In Pennsylvania, those serving a felony sentence during the election are prohibited by law from voting, unlike defendants awaiting trial. In addition, post-conviction incarceration leads to long-term costs and political alienation, which in turn may reduce turnout (Lerman and Weaver 2014; Western 2006). We test this mechanism with a mediation analysis.

H5. *First-Time Defendants*

Finally, we also test a corollary hypothesis: the effect of pretrial incarceration may be most pronounced among first-time defendants (Gerber et al. 2017). By contrast, defendants with prior cases may have already been detained pretrial and already experienced its repercussions. To summarize, our hypotheses are as follows:

H1: Pretrial incarceration reduces voter turnout.

H2: Resource deprivation: The impact of pretrial incarceration is greater for low-income defendants.

H3: Racially disparate impact: The negative effect of pretrial incarceration is greater among Black defendants, because of: more severe resource impacts, reflected in larger effects on poor Blacks (H3a); racial political socialization (H3b); or higher baseline turnout, reflected in larger treatment effects on Blacks who had voted (H3c). Pretrial incarceration also moderately affects Hispanic defendants (H3d).

H4: Incarceration sentence: The negative effect of pretrial incarceration is partly explained by an increased likelihood of conviction and an incarceration sentence.

H5: First-time defendants: The effect of pretrial incarceration is greater among those not previously detained.

Data on Pretrial Incarceration and Turnout

This study requires access to comprehensive, detailed court records, and as-if random assignment to pretrial incarceration. Philadelphia County meets these requirements. The county is coterminous with the city of Philadelphia, the sixth-largest city in the United States. Philadelphia's pretrial system resembles most jurisdictions in its use of money bail, deference to officials' discretion to set bail, and other characteristics (Pretrial Justice Institute 2009). The average length of pretrial detention, and the overrepresentation of Black Americans among pretrial detainees, are similar to other large metropolitan areas (Olson 2012, Chauhan et al. 2017; on race, see Arnold et al. 2018). The case can thus generalize to large cities.

We use all cases filed in Philadelphia County between the 2008 and 2012 general elections (Stevenson 2018). Each observation contains defendant information (name, birthdate, 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 B 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 birthdate, which precludes matching with voter records, and we drop defendants who were too young to vote in the 2012 election. Second, we exclude defendants whose zip code at the time of arrest is unknown or located in other states. Third, we drop cases without a release date or a named bail magistrate, necessary for calculating the instrument.

We then merge the court records with state voter files. First, we use raw Pennsylvania (PA) files from 2009 and 2013 to measure 2008 and 2012 turnout and registration, respectively.[5] However, using PA voter records exclusively would bias our estimates, if people detained pretrial

[5] We obtained all voter files in the paper from L2, Inc., a national non-partisan firm that collects voter files from states. L2 did not clean or alter the PA files, which match vote counts accurately (within 1.2% and 0.2% of the official counts for the 2008 and 2012 general elections, respectively).

are subsequently more likely to move out of state. Therefore, we also match defendants not in the PA voter file with the L2 uniform 2014 voter files of all remaining states. This latter match represents a small percent of our matched cases. See Appendix C for more details.

To merge these records, we use the probabilistic method developed by Enamorado et al. (2019). Its main advantage is the 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 birthdate. 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 post-merge analysis, we reweight turnout and registration status by the match probabilities to account for the uncertainty surrounding the merge and produce consistent estimates (see Appendix D). Altogether, the match rate is 55%.

In sum, our main sample includes all defendants with cases filed between the 2008 and 2012 elections, except defendants whose name, birthdate, bail magistrate or release date is unavailable in the court records, or who were too young to vote in 2012, or whose zip code is invalid or outside of Pennsylvania. 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 90,589 defendants. Finally, we augment our data with a proxy measure of defendant resources (zip code average income) from the 2008 release of the IRS' Statistics of Income.

Table E1 describes the sample. In our sample, 36% of defendants were incarcerated pretrial (detained more than 3 days). Detainees' pretrial jail time averaged nearly 5 months, with a median of 2.5 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, Black, live in poor areas, face minor and nonviolent charges, have a prior case, and are unlikely to vote.[6] 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 not face any violent charge.[7] Detainees are less likely to have voted than released defendants, but this gap grows post-treatment, 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). OLS regression, therefore, may produce biased estimates.

To overcome this challenge, we analyze a natural experiment in Philadelphia's court system. Two features of Philadelphia's pretrial process are particularly helpful for our design. First, after arrest, defendants are randomly assigned to a bail magistrate who determines the pretrial conditions of release at a bail hearing (see Figure F1). Assignment is as-if random because defendants are automatically assigned to the magistrate on duty, and the six bail magistrates rotate

[6] 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)

[7] The percentage with any violent offense increases from 22% to 41% when including sexual assault and simple assault (a misdemeanor).

through all three possible shifts. Specifically, 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).

The second key feature is that bail magistrates have discretion in imposing pretrial conditions and vary in their harshness. Prior studies of this jurisdiction show that some magistrates are consistently more likely to set higher bail amounts that result in pretrial incarceration compared to magistrates deciding observably similar cases (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 they been assigned to a magistrate with more punitive tendencies. This design identifies the local average treatment effect (LATE) 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 due to 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).[8] To code case severity, we sum the Pennsylvania Offense Gravity Scores (OGS) across the offenses in the case, and bin that into terciles (see Appendix B). Thus, for a given case, 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. We construct our instrument using the following equation:

$$Z_{dtjh} = \frac{(\sum_{k=0}^{N_{tjh}} P_{ktjh}) - P_{dtjh}}{N_{tjh} - 1} \quad (1)$$

where N_{tjh} is the number of cases seen by magistrate j at year t and case severity h, and $P_{dtjh} \in \{0,1\}$ represents the decision (detained = 1 or released = 0) made by magistrate j for defendant d at year t and case severity h. Our final sample includes 6 magistrates per year, with the exception of 2009 when there was one vacancy. The median number of cases per magistrate-by-year is 6,072, and the median number of cases per magistrate-by-year-by-OGS-tercile is 1,357. The leave-out-case pretrial detention rate ranges from 0.06 to 0.71, with an average of 0.36 and a standard deviation of 0.20. Moving from the most to the least lenient magistrate increases the likelihood of pretrial detention by 13 percentage points for defendants in the lowest tercile of offense severity, almost 20 points for those in the middle tercile, and 11 points for those with the most serious offenses.

Empirical Strategy

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

$$P_{dtjh} = \alpha_0 + \alpha_1 Z_{dtjh} + X_{dt}^{\top}\Omega + \epsilon_{dtjh} \quad (2)$$

[8] We find similar results when constructing instruments separately for Black and White defendants, which accounts for the possibility that magistrate leniency depends on race. We do not rely on this specification due to its limitations (see Appendix Q).

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, and 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 3 days, Z_{dtjh} is the instrument, and X_{dt} is a set of defendant and case covariates.[9]

Assessing the Instrument's Validity

We now present evidence that our instrumental variable approach satisfies the required assumptions: exogeneity, monotonicity, and exclusion. First, if our instrument is exogenous, case and defendant characteristics should be distributed evenly across magistrates with different decision tendencies and should not be predictive of the instrument. We find that with a few minor exceptions, these covariates are not significantly related to the instrument (see Table G1).

Second, our instrument should be a strong predictor of pretrial detention. Table H1, row 1, presents the first stage estimate for the sample, 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 0.8 point or more increase in the likelihood of pretrial detention.

Third, if our instrument is monotonic, assignment to a more punitive magistrate should increase defendants' probability of pretrial incarceration regardless of their characteristics. While

9 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, violent, firearm, and property charge; case severity tercile; had prior case; and the year, month, day of the week-shift of bail hearing.

no direct test of the monotonicity assumption exists, we can at least provide evidence that the instrument satisfies “average monotonicity” (Frandsen et al. 2019). Tables H1-H2 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.

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 bail magistrate affects a defendant’s political behavior through other channels, this assumption would be violated. Several factors lend support for the validity of the exclusion restriction. Bail magistrates’ interactions with defendants are brief (less than two minutes on average), leaving minimal time for comments aside from release conditions (Stevenson 2018). Moreover, bail magistrates have no further interaction with defendants or jurisdiction over cases after the preliminary pretrial decision, meaning that their pretrial decision is the only plausible way that they could affect the defendant (Stevenson 2018).

Main Results

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 a 10.5 percentage point decrease in the probability of voting, with full controls. The effect is similar with fewer controls (see Figure 1 and Table I1) and with bivariate probit (biprobit) (see Table J1).[10]

[10] We do not use biprobit extensively because it can be sensitive to heteroscedasticity and computationally expensive due to the joint distributional assumptions (Chiburis et al. 2012). In practice, biprobit produces similar results to 2SLS but is not as robust to misspecification of the

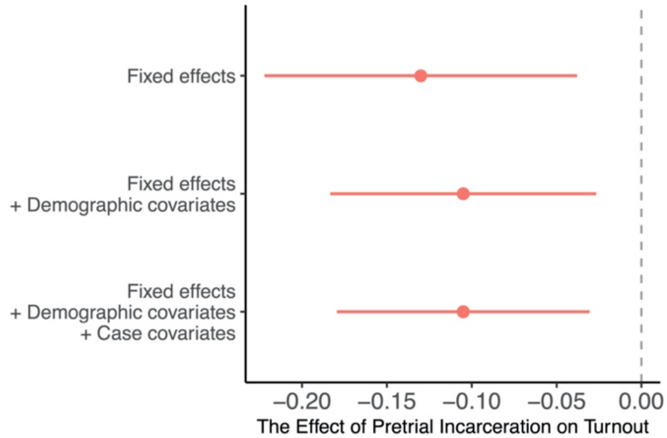

Figure 1. **The Effect of Pretrial Incarceration on 2012 Turnout.** Second-stage 2SLS estimates with 95% confidence intervals from models that include fixed effects only (top), fixed effects and demographic covariates (middle) and fixed effects, demographic and case-level covariates (bottom).

Appendix L presents robustness checks. We find similar results with an alternative (residualized) instrument specification; a continuous measure of pretrial incarceration (logged number of days detained); alternative case covariates; and in a period when the percentage of missing magistrate data is too small to introduce selection bias. In addition, to address the possibility that our results are due to random chance, we conduct a permutation test which non-parametrically accounts for potential within-magistrate clustering of cases across time. Finally, we conduct two additional checks. First, 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. As expected, the treatment does not predict the pretreatment outcome. Second, we replicate the main result for a related outcome: registering to vote. The effect on registration is -16 percentage points and statistically significant.

first stage model (Angrist and Pischke 2008). Using OLS, pretrial incarceration has a 7 to 11 point effect (Table K1).

Resource Deprivation

Pretrial incarceration may depress voting because it undermines defendants' livelihoods and relationships, which are resources that facilitate turnout. If pretrial incarceration affects voting through resource deprivation, the magnitude may be greater among the lowest-income defendants, who have few resources to mitigate the socioeconomic repercussions. As a partial test of this mechanism, we split our data into three groups based on whether the defendant lives in areas where the average income falls in the bottom (< $25,888), middle ($25,888-$34,090) or top (> $34,090) tercile of the sample. [11] Rather than an interaction model, we estimate the effect for each subset and use a t-test for the difference between these effects. Figure 2 (left panel) presents the results. The demobilizing effect of pretrial incarceration is acute among defendants in the bottom and middle terciles. For such defendants, pretrial incarceration reduces turnout by 13–17 percentage points.[12] In contrast, in the top tercile, the effect is only 1 percentage point (significantly smaller than the middle tercile, $p < 0.05$, one-tailed; see Table M1, row 1). These results are consistent with the resource mechanism: pretrial incarceration affects those in zip codes earning less than $34,000 a year, who are less able to withstand socioeconomic destabilization.[13]

Racially Disparate Impact

Pretrial incarceration may especially demobilize Black defendants. Black defendants on average may face greater disadvantage in withstanding resource losses, and pretrial incarceration

[11] Even upper-tercile defendants are lower-income compared to local and national income.

[12] Table I1 presents full results. Results are similar without covariates.

[13] While we cannot statistically distinguish the bottom and top terciles, their magnitudes are quite different (Table I1, row 2).

is likely to be especially unjust to Blacks, who may feel alienated from government institutions as a result. In this respect, Hispanics may occupy a middle ground between Blacks and Whites. We rely on surname prediction to identify White Hispanic defendants, and caution that the results for them may be affected by measurement error (see Appendix B).

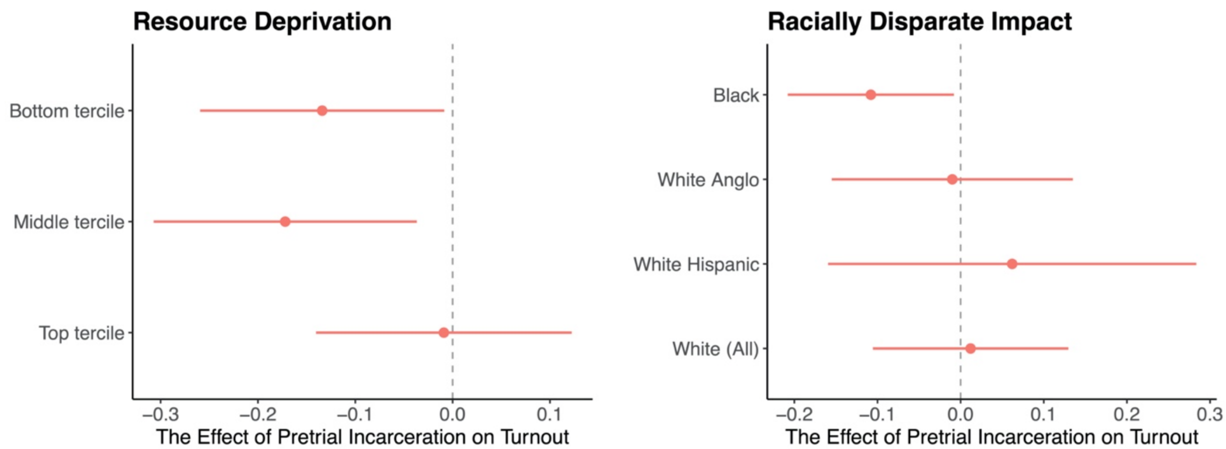

Figure 2. The Effect of Pretrial Incarceration on 2012 Turnout by Income (left) and Race (right). Second-stage 2SLS estimates with 95% confidence intervals from models that include fixed effects, demographic and case-level covariates.

Figure 2 (right panel) shows that pretrial incarceration has a racially disparate impact: while it has no significant effect on White Anglo or White Hispanic defendants, it decreases turnout by 11 percentage points among Black defendants.[14] The effects on White Hispanic and Anglo defendants are small, highly imprecise, and change sign across specifications (Table I1, row 3). By contrast, the effect on Black defendants is large, and similar with and without controls (Table I1, row 3). In addition, the effects on Black defendants and pooled White defendants differ substantially (by 12 percentage points, $p = 0.065$, one-tailed; Table M1).[15] This evidence supports the hypothesis that Blacks are especially affected.

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[14] Table I1 presents full results. Other racial groups are too few to analyze separately.

[15] The effect on Black defendants persists in the robustness checks discussed above (Table L1).

We can also test the racialized resources hypothesis, by examining treatment effects for Blacks and Whites at each income tercile (see Table N1). As predicted, resources condition the pretrial incarceration effect for Black but not White defendants. Specifically, we find large (though somewhat imprecise) treatment effects only for Blacks in low and middle income terciles. There are no effects for Whites in any tercile. Pretrial incarceration reduces voting among Black defendants from poor neighborhoods.

While the strong effect on Black defendants is consistent with resource and socialization mechanisms, it may also be driven by Blacks' higher baseline turnout (White 2019). Such higher turnout is evident in our data (Table O1). If pretrial detention only affects those who voted in prior elections, then we would see a spurious larger effect among Black defendants even if the treatment has the same effect across race. To test this possibility, we divide the sample by both race and 2008 turnout.[16] If Blacks are affected simply because they are more likely to be voters, we would see null effects among prior non-voters of either race and find an equally large treatment effect for both White and Black prior voters. [17] Results are presented in Tables O2-O3. As this hypothesis expects, both Black and White *non*-voters are *not* affected (Table O2, row 1). And both Black and White voters *are* affected (Table O2, row 2). However, Black and White voters are not affected equally. Specifically, the effect on voters shows a racial gradient: -17 points among Blacks, -12

As further evidence of racial disparity, Table L4 shows large pretrial incarceration effects on *registration* only for Black defendants.

[16] We do not examine White Hispanics in these analyses because there are too few observations.

[17] Conversely, the impact on White Hispanics may appear muted because of likely lower rates of citizenship.

points among Whites, and -6.5 points among White Anglos. These effects are not statistically distinguishable from each other because of the paucity of White Anglo prior voters (Table O3). However, the magnitude for Black voters is more than double that for Anglo voters, and it is the only statistically significant effect in the table. In addition, the difference between voters and non-voters is only significant for Black defendants (Table O3, row 1). Race remains a factor even when accounting for prior voting.[18]

Incarceration Sentence

Pretrial incarceration may have a negative effect on turnout because it increases the likelihood of being convicted and sentenced to incarceration. If defendants are serving an incarceration sentence for a felony in Pennsylvania, they are not allowed to vote. Even afterwards, the effects of having served an incarceration sentence may follow them. This is not a violation of the exclusion restriction, but rather a way that pretrial detention may affect turnout. To test this mechanism, we use mediation analysis in an instrumental variable setting (Pinto et al. 2019). The mediator is a binary variable coded 1 if the defendant's case received a disposition before the election and resulted in a minimum incarceration sentence of 1 day, and 0 otherwise (16% of defendants).[19] In Table 1, we find no evidence that the effect of pretrial incarceration is explained by post-conviction incarceration sentencing. Table R1 replicates this null result for the subsets we

[18] In Appendix P, we test this explanation using prior registration. We similarly find larger effects on registered Black than White defendants. Moreover, the effect on registered Whites is 0 (Table P1).

[19] For those sentenced after the election, the mediator is coded 0 (7% of the sample). Appendix R discusses this measure.

focus on (by income, race, and prior case). Overall, the null mediation effects are suggestive evidence that sentencing is not a major factor in our results.[20]

	Mediation Analysis		
	Direct Effect (Pretrial Incarceration)	Indirect Effect (Incarceration Sentence)	Total Effect
	−0.078	−0.027	−0.105
	(0.010)	(0.040)	(0.038)
N: 90,589			

Table 1: Sentencing Mechanism: The Mediated Effect of Pretrial Incarceration on 2012 Turnout. Pretrial incarceration is coded as 1 if an individual was in jail for three days or more and 0 otherwise. Incarceration sentence takes 1 if an individual was sentenced to a minimum 1 day or more of incarceration and their case reached a disposition before the election, and 0 otherwise. Full controls included. Heteroscedasticity-consistent standard errors are presented within parentheses.

First-Time Defendants

Finally, we examine whether the effects of pretrial incarceration are strongest among those undergoing their first arrest (no prior cases) in Pennsylvania, as those with prior cases may have already been demobilized. As Figure 3 shows, the results are consistent with this prediction. Pretrial incarceration reduces turnout by 9 and 15 points for those with and without prior cases, respectively. Thus, the effect is strongest on first-time defendants, although imprecise due to the small sample.[21] This suggests that the overall treatment estimate is not an artifact of covariates correlated with having prior cases.

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[20] The specific mechanism of imprisonment *during* the election is unlikely. The percentage of defendants who might have been imprisoned post-conviction *during* the election is small: for example, only 4% of defendants with cases in the 6 months before the election had a disposition date before the election and an incarceration sentence.

[21] The imprecision also explains why the two are not statistically distinguishable (Table I1).

Generalizing the Results

The estimated effects are for *compliers,* that is, those individuals at the margin of being incarcerated pretrial. In Appendix S, we compare the sample of compliers to the overall sample (Dahl et al. 2014). We find that compliers are within 6 percentage points of the sample average on age, gender, race, turnout, registration, prior case status, and each charge. Thus, compliers are not markedly different, supporting the generalizability of the effects.[22]

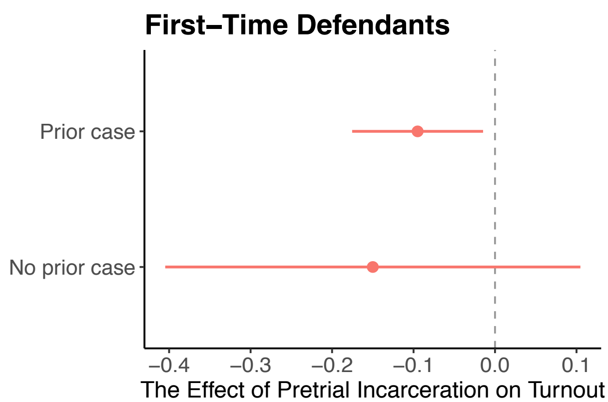

Figure 3. The Effect of Pretrial Incarceration on 2012 Turnout With and Without a Prior Case. Second-stage 2SLS estimates with 95% confidence intervals (with full controls).

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.

[22] Finally, pretrial incarceration matters less for cases far from the election, especially those more than 6 months out (Appendix T). We interpret this as a possible decay of the effect.

In this paper, 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 is a racial class system of social control (Soss and Weaver 2017). Does this racially-targeted experience of being jailed while presumed innocent affect political behavior?

Using a large administrative dataset, we found that defendants as-if randomly assigned to harsher bail magistrates in Philadelphia are jailed pretrial for much longer, and emerge from the experience with a much lower voting propensity, especially if they are poor and Black. Pretrial incarceration makes a difference, reducing voting by people who had voted before. The findings likely generalize to many other large jurisdictions with substantial inequalities, where poor individuals of color 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 the franchise only with due process and equal justice.

The findings suggest that research on the carceral state should explicitly account for pretrial incarceration. To date, studies have not done so. Studies that do not distinguish pretrial from no incarceration may under-estimate the full impact of incarceration.

Pretrial incarceration is only one aspect of the shadow carceral state. The coercive power of the state is exercised through a host of private, civil and administrative practices and institutions. Those 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 bedrock democratic principles, from due process and entitlement to legal representation to the ability to hold actors accountable to the public. They likely carry significant effects on political engagement, accountability, and representation. A fuller understanding of the impact of institutions on behavior requires attention to the interplay of private and public actors.

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 et al. 2019, 39). Whether the shadow carceral state mobilizes in some ways while it demobilizes in others is a question for further research.

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