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ARTICLE

The Shadow Carceral State and Racial Inequality in Turnout

Ted Enamorado[1], Anne McDonough[2] and Tali Mendelberg[3]

[1]Department of Political Science, Washington University in St. Louis, St. Louis, MO, USA, [2]Law School, Yale University, New Haven, CT, USA and [3]Politics Department, Princeton University, Princeton, NJ, USA
Corresponding author: Ted Enamorado; Email: ted@wustl.edu

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Abstract
Scholars have studied the carceral state extensively. However, little is known about the 'shadow' carceral state, coercive institutions lacking even the limited safeguards of the carceral state. Pretrial incarceration is one such institution. It often lasts months and causes large resource losses. Yet it is imposed in rushed hearings, with wide discretion for bail judges. These circumstances facilitate quick, heuristic judgments relying on racial stereotypes of marginalized populations. We merge court records from Miami-Dade with voter records to estimate the effect of this 'shadow' institution on turnout. We find that quasi-randomly assigned harsher bail judges depress voting by Black and Hispanic defendants. Consistent with heuristic processing, these racial disparities result only from inexperienced judges. Unlike judge experience, judge race does not matter; minority judges are as likely to impose detention and reduce turnout. The 'shadow' carceral state undermines democratic participation, exacerbating racial inequality.

Keywords: shadow carceral state; pretrial incarceration; turnout; racial inequities

Introduction

The 'carceral state' has become a well-documented feature of the American political system. Record numbers of Americans – especially poor people of colour – regularly encounter harsh treatment by police, and many have been imprisoned (Lerman and Weaver 2014; Soss and Weaver 2017). This carceral contact has increasingly been featured as a possible cause of depressed voting among disadvantaged groups in the US (Lerman and Weaver 2014; Morris 2021; White 2019).

Though voluminous, the literature on the carceral state has neglected the 'shadow' carceral state (Beckett and Murakawa 2012). The shadow carceral state consists of actors who draw on the government's power of coercion unconstrained by the procedures of the carceral state (Kohler-Hausmann 2018; Page, Piehowski, and Soss 2019). The carceral state must consider the evidence and decide guilt or innocence before imposing punishment. The shadow carceral state is not required to do so because it relies on administrative rather than judicial procedures. In the shadow carceral state, citizens lose civil liberties with degraded due process and equal protection.

A key institution of the shadow carceral state is pretrial incarceration (PI). PI confines defendants before the disposition of their case, to ensure they do not violate the law or fail to show up for court. In local jails, which hold far more than state and federal prisons combined, approximately two-thirds of inmates are held pretrial (Digard and Swavola 2019, see also Sawyer and Wagner 2022; U.S. Commission on Civil Rights 2022).

Typical of the shadow carceral state, PI lacks important elements of due process, with bail hearings often lasting under four minutes (Gonzalez Van Cleve 2022, 135; Scott-Hayward and Ottone 2018, 172–3).[1] Given this weak due process, it is not surprising that PI is punitive; five months is not uncommon, even though most of these cases are nonviolent and even though a plurality of cases are later dismissed or found not guilty (Rabuy and Kopf 2016; Sawyer and Wagner 2022; Stevenson 2018). Like other shadow institutions, PI is also highly selective by race and class (Arnold et al. 2018; Demuth and Steffensmeier 2004).

What are the consequences of this shadow carceral state for the individuals it punishes? Building on carceral state literature, we argue that pretrial incarceration reduces turnout for poor people of colour – even adjusting for prior vote propensity and case and defendant characteristics (Demuth and Steffensmeier 2004; Lerman and Weaver 2014; White 2019). Crucially, this racial disparity is associated with the distinctive features of the shadow carceral state and occurs regardless of the race of the person who administers it (Arnold et al. 2018). Bail judges must make quick judgments lacking relevant information – the type of decision-making most vulnerable to inaccurate stereotypes of poor people of colour (Arnold et al. 2018; Rachlinkski and Wistrich 2017). Because PI is administered with little time to weigh evidence, minimal accountability for incorrect decisions, and because stereotypes of stigmatized poor defendants are prevalent even among minority judges, the racial disparity in PI may be similar for minority judges. Thus, PI may be racially biased, with the bias largely impervious even to descriptive representation on the bench. Finally, bail literature shows that the cognitive challenges of the rushed decision situation are most severe for inexperienced judges; thus, we expect them, in particular, to produce biased decisions. That is, the PI system will disempower poor minority defendants facing inexperienced judges.

To test these propositions, we need a measure of pretrial incarceration, which has been missing in the literature (McDonough, Enamorado, and Mendelberg 2022). In addition, we need to avoid well-known pitfalls of studies of incarceration effects on turnout: omitted variable bias, inaccurate self-reports, and an under-representation of incarcerated people in surveys (Burch 2011; Gerber et al. 2017; White 2022). To that end, we use data from Miami-Dade County, where, on weekends, defendants are assigned to the bail judge on duty. Judges are assigned to shifts with approximately equal probability based on their last names, and judges vary in their propensity to set high bail conditions. We leverage the quasi-random assignment of bail judges who vary in their tendency to assign pretrial incarceration. This random assignment avoids the confounding effects of case or defendant characteristics (McDonough, Enamorado, and Mendelberg 2022; White 2019). We obtained full case records for the entire population of defendants in Miami-Dade County ($2008-2016$), which yielded a large sample of 42, 950 defendants. We merge these records with voter files to estimate the effect of pretrial incarceration on turnout.

A final important advantage of this data is the presence of many more judges than in previous carceral effects studies. Specifically, we have 156 judges (12 Black, 60 Hispanic, and 84 White), far more than the 6–15 judges in recent studies of incarceration effects (McDonough, Enamorado, and Mendelberg 2022; White 2019).[2] This large and racially varied sample of judges allows us to test two alternative mechanisms: the race of the judge and judicial experience. While we lack variation in due process safeguards, we leverage variation in judge experience to indirectly assess the consequences of weaker safeguards. This data can show whether the effect diminishes with same-race judges, who would not hold animus toward their racial group, or, alternatively, diminishes with experienced judges, who would be less susceptible to erroneous heuristic judgments triggered by the rushed decision situation.

We find that pretrial incarceration decreases voting by 7 percentage points. This is among the larger effects in the literature on voting interventions (Gerber, Huber, and Hill 2013). The effect is strong and precise only for Black and Hispanic defendants, and only for Black residents from

[1]The jurisdictions include Cook County, Illinois, and Los Angeles and Orange Counties, California.
[2]In White (2019), this is the number of courtrooms, the 'treatment' unit.

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poor zip codes. The effect is not due merely to physical incapacitation while incarcerated. It is not spuriously caused by a pre-existing vote propensity. Moreover, the race of the judge makes no difference, either to the likelihood of PI or to its effect on turnout. Black and Hispanic judges do not produce a smaller racial disparity, co-racial defendants are equally demobilized. By implication, then, harsh punishments and their disempowering effects are not explained by White judges' racial animus. Instead, these consequences emerge from a system of weak due process: quick, unchecked, stereotyped decisions about the risk posed by poor, stigmatized people of colour. We find that these decisions are most racially biased at the hands of inexperienced judges. Racialized disempowerment in the shadow carceral state seems to arise from weak due process administered by inexperienced judges. The pretrial incarceration system itself is demobilizing.

We use data from Miami-Dade because it meets the data requirements we noted above. These findings from Miami-Dade plausibly generalize to other large US cities. Many large urban counties require bail judges to reach quick decisions with little opportunity for a defence and frequently detain poor Black and Hispanic defendants for months (Arnold et al. 2018; Hood and Schneider 2019; Olson and Taheri 2012). The racial disparity in PI holds in the 75 largest counties in the United States (Demuth and Steffensmeier 2004).

The effect of incarceration on voting has been subjected to numerous tests, but the literature has focused on incarceration after a verdict (Burch 2011; Gerber et al. 2017; Lerman and Weaver 2014; White 2019). We advance the literature in three ways. First, we focus on incarceration before a verdict by measuring PI and estimating its causal effects with quasi-random assignment of judges to cases. Second, we test and reject the moderating role of judge race. Third, we test and find a role for judge experience. This research allows us to conclude that the shadow carceral state matters for turnout and that weak due process administered by inexperienced judges is an important aspect of American punitive institutions. Through pretrial incarceration, the carceral state permeates far deeper and in less formal ways than recognized to date in the literature on the carceral state. This informality promotes a decision-making process that bypasses even some of the limited protections of the official carceral state. These conclusions carry troubling implications for the democratic health of the American political system, which rests on the promise of equal voice (Verba, Schlozman, and Brady 1995), and for the fairness of its carceral systems.

Pretrial Incarceration

The broad reach and disparate racial impact of the carceral state have been well documented (Soss and Weaver 2017). Little explored, however, is the shadow carceral state. 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 is a significant element of the shadow carceral state. PI confines defendants before their case is decided to ensure they do not violate the law or fail to show up for court. PI has received little attention, but it is a major reason for the enormous size of the carceral state. As we noted, local jails have more inmates than state and federal prisons put together, and nearly two-thirds of jail inmates are being held pretrial (Sawyer and Wagner 2022). In addition, net jail growth in recent years is almost entirely driven by PI (Sawyer and Wagner 2022).[3]

PI is characterized by each element of the shadow carceral state. First, it contravenes notions of justice that undergird safeguards in the formal legal system. Though PI is justified by the

[3]In addition, over half of people with frequent police interactions also report very frequent contact with bail officials (Garcia-Rios et al. 2023).

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imperative of public safety, in many large jurisdictions, most PI cases are nonviolent (Circuit Court of Cook County, 2019; Scott-Hayward and Ottone 2018). For example, in one large urban county, 60 per cent of cases with three or more PI days had nonviolent charges (Stevenson 2018). PI is not a formal punishment commensurately fitting a specific crime, and yet it often results in months in jail. For example, in Philadelphia County, 40 per cent of people arrested were incarcerated for at least three days, and of those, the average detention period was almost 5 months (Stevenson 2018). In large jurisdictions across the US, most jail inmates are held pretrial for over a month on average (U.S. Commission on Civil Rights 2022, 28).

Second, PI is not subject to the usual protections of formal due process. It is often decided in a pro forma hearing too brief to allow arguments from the defence. For example, in Los Angeles and Orange Counties, defendants did not contest the decision in nearly 2/3 of the cases studied, and when they did, judges 'usually denied requests…without comment' (Scott-Hayward and Ottone 2018, 173). In these and other large jurisdictions, the hearing typically lasts less than 2–4 minutes (Scott-Hayward and Ottone 2018; Stevenson 2018). In this rushed process, judges have little opportunity to consider individual circumstances that may depart from heuristic, stereotyped judgments.

Third, pretrial incarceration does not conform to standard notions of equal protection. It is typically imposed on minority defendants who are too poor to post bail (Circuit Court of Cook County, 2019; Scott-Hayward and Ottone 2018).[4] The defendant's inability to pay is rarely taken into account during the hearing (Scott-Hayward and Ottone 2018; Stevenson 2018). The bail industry makes billions a year by extracting resources from poor communities (Page, Piehowski, and Soss 2019; Rabuy and Kopf 2016). Bond companies impose additional fees and seize homes and other property as collateral, even when the court later dismisses the charge, leaving many 'not-guilty' defendants thousands of dollars in debt (Page, Piehowski, and Soss 2019; U.S. Commission on Civil Rights, 2022). The vastly disproportionate burden placed on poor defendants is typical of the shadow carceral state.

Thus, the PI process lacks some important defendant safeguards, and PI is imposed much more often on racialized poor populations.

Bias, Heuristics, and Rushed Judgment

How would the PI process shape the PI decision? According to studies of street-level bureaucrats, the high degree of discretion afforded by bail judges can lead to biased decisions (Lipsky 1980). For example, in 'welfare' cases, where caseworkers have discretion, they are more likely to apply punitive sanctions against disadvantaged racial minorities (Keiser, Mueser, and Choi 2004). As Einstein and Glick note, 'in the absence of clear rules designed to preclude discrimination, bureaucrats with discretion can act according to their own biases' (Einstein and Glick 2017, 101).

This pattern holds in the case of bail decisions. Bail judges are given discretion to decide how likely the defendant is to pose a physical threat, and they tend to over-rely on race and underweight more proximal predictors (Arnold et al. 2018; Demuth and Steffensmeier 2004; Gonzalez Van Cleve 2022; Kleinberg et al. 2018). Machine learning algorithms that lack this human discretion, which excludes race, can generate more accurate predictions and reduce the racial disparity in PI decisions compared to human judges (Kleinberg et al. 2018).

How might this racially biased process work? Is it due to racial animus; that is, general stereotypes targeting all racial minorities? Or is it due instead to implicit cognitive heuristics applied to particular, stigmatized subgroups of minority populations in a rushed judgment? (Bordalo et al. 2016) We argue that the PI process may produce the latter: quick heuristic decisions driven by

[4]The typical local inmate is non-White and earns $16,000 a year (Gupta, Hansman, and Frenchman 2016; Rabuy and Kopf 2016). In a study of one large jurisdiction, most pretrial detainees could not afford even the $1,000 or less necessary to avoid detention (Stevenson 2018, 512).

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associations between specific racialized poor populations and crime. As Rachlinkski and Wistrich (2017) summarize, 'judges, like most adults, rely too heavily on intuition while making important decisions. This tendency leaves them vulnerable to using overly simplistic cognitive strategies to decide cases, which creates predictable, systematic errors in judgment' (Rachlinkski and Wistrich 2017, 211).

This argument builds on studies of street-level bureaucrats finding that racial disparities are caused not by outright racial hostility as much as workload and lack of accountability for biased outcomes (Andersen and Guul 2019; Christensen, Szmer, and Stritch 2012; Lipsky 1980). The situational factors present in the PI decision conform to the conditions identified in these studies. As noted above, the detention hearing is too brief to allow the judge to take complex individual circumstances into adequate account, or for a defendant to offer any defence. Decisions are made under severe time pressure and with limited individuating information about the defendant. In these circumstances, decisions tend to be based on 'System 1' processing: a quick, less deliberate, heuristic, and intuitive cognitive process (Rachlinkski and Wistrich 2017).

Judges' racially disparate decisions may be reinforced by a representativeness heuristic. This heuristic takes a small grain of truth and exaggerates it. In this setting, while the average Black and White defendants do not differ substantially in their risk of rearrest while released, Black defendants are slightly more likely than White defendants to be among the very small percentage of defendants with high risk (Arnold et al. 2018). This slight correlation of race with extreme and rare behaviour may lead to a representativeness heuristic based on an illusory correlation. This heuristic exaggerates the probability that the average member of a group will engage in a salient action that most differentiates their group from another (Arnold et al. (2018). In other words, the perceived probability of a behaviour by a group member becomes inflated when that behaviour has a higher *relative* frequency at the extreme, even if the *absolute* frequency of that behaviour is small.

Importantly, this process does not require generalized stereotypes against *all* Black or Hispanic defendants, and no racial *animus* (Bordalo et al. 2016). In a System 1 process, bias comes *not* from feelings of dislike for racial outgroups or from blanket stereotypes against an entire racial group, but from quick, heuristic judgments about a particularly stigmatized, salient *subgroup* of a minority population commonly associated with crime (Bordalo et al. 2016, footnote 9). For example, in implicit bias experiments, participants who more strongly associate weapons with Black people, during an implicit judgment task calling for a quick System 1 judgment, also exhibit more severe racial bias in a 'shooter' task, mimicking the quick decisions police officers must make in assessing a threat (Glaser and Knowles 2008). Importantly, these quick, punitive reactions are triggered *especially* by Black defendants with a more racially stereotypical presentation (Eberhardt et al. 2004; Kahn and Davies 2017). Furthermore, implicit associations are not correlated with general negative assessments of all Black people (Judd, Blair, and Chapleau 2004). That is, System 1 processing may promote implicit bias rather than categorical bias, specifically against the most stereotyped subgroups of racial minority groups.

These studies of System 1 processing particularly highlight how time pressure and the absence of deliberation allow such associations to dominate judgments. The rushed hearing, the high cognitive load, the lack of accountability to the defendant and their legal counsel, the lack of opportunity to consider the facts of the case, and the circumstances of the individual – all these features of the detention hearing are also well-known factors that exacerbate the biases from heuristics (Rachlinkski and Wistrich 2017, 111–118). As a result of these mechanisms, judges may systematically assess poor minority defendants as posing too high a risk.

The (Null) Impact of Judge Race and the Effect of Judge Experience

This System 1 process has important implications for judge race: judge race should have a null effect. The racial disparity in pretrial incarceration may have roots in heuristics that, with time

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pressure and lack of accountability, could produce racial disparity by judges of any race. Consistent with this possibility, a study of bail decisions in Miami-Dade found that racially disparate detention decisions were produced regardless of judge race (Arnold et al. 2018). Furthermore, Rachlinski et al. (2009) found that about half the Black judges they studied exhibited anti-Black bias on the Implicit Association Test (IAT), and these IAT bias scores predict racial disparities in hypothetical court cases. These effects are obtained only with implicit racial stimuli, consistent with a System 1 process (Rachlinkski and Wistrich 2017, 101). This finding is buttressed by evidence that many minority judges hold negative views of lower-status subgroups of their own racial group, as do many minority survey respondents (Jefferson 2023). These studies suggest that the judgment process produces racially disparate outcomes because of a situation that promotes reliance on heuristics about stigmatized subgroups of minority communities, not as a result of generalized animus or blanket negative stereotypes held by White judges. Put differently, the racial disparities may be built into the situation.

An additional reason why judges of any race might produce racial disparity is the requirements of their role. Theories of judicial organization posit that judges are selected, socialized, or incentivized to conform to the expectations of their organizational role as judges (Harris and Sen 2019; Steffensmeier and Britt 2002). Judges' individual identities may be overridden by a judicial culture or incentives that prioritize the avoidance of releasing defendants who may then commit harmful crimes (for example, Steffensmeier and Britt 2002).[5] Judges issue much more punitive decisions as their re-election date approaches, especially for violent crimes (Berdejó and Yuchtman 2013; Huber and Gordon 2004). That is, judges may seek to avoid releasing defendants who go on to commit further violent crimes, to avoid losing their seats. This institutional imperative may hold for judges of *any* race (Harris and Sen 2019). These judges are chosen by elites or voters who expect that judges will avoid releasing defendants who go on to commit violent crimes. As Harris notes, Black judges may adopt the expectations of the White-dominated carceral system (2024; see also Steffensmeier and Britt 2002).

For these reasons, we do not expect judge race to make much difference. As Harris put it, 'non-White judges' racial identities, alone, do not appear to lead to a decrease in the Black-White incarceration gap' (2024, 34). Our detailed review of the literature on judge race effects supports this conclusion (see Appendix A). That is, there is no consistent evidence that Black and Hispanic judges produce less racially disparate decisions.

Unlike judge race, we expect that the judge's experience does matter. If System 1 processing helps explain the racial disparity, then judges would make more inaccurate, racially disparate predictions if they are inexperienced. As Arnold et al. (2018) explain, defendants who violate the conditions of their bail and are consequently taken into custody undergo a hearing before a bail judge. These hearings allow bail judges to learn which factors often lead to unsafe releases. The more that judges see first-hand that defendants do and do not re-offend, the more their future decisions can become accurately informed.

This is in line with recent findings that people can learn to break mental habits that produce bias. In recent randomized studies by Devine and colleagues, treated participants practised a set of cognitive strategies, such as learning to rely on relevant information about an individual rather than illusory correlations about a group (Devine et al. 2012). Treated participants continued to apply these cognitive strategies up to two years later, suggesting that the right sort of experience can establish new decision habits (Forscher et al. 2017). Another example comes from 'shooter' experiments. In these studies, untrained participants tend to make racially biased, inaccurate shooting decisions while trained participants do not. Observational and experimental evidence from police officers and other populations shows that training and similar learning opportunities can greatly reduce this racial bias (Singh et al. 2020). As Singh et al. conclude, 'through practice,

[5]For example, judges in districts with competitive partisan elections issue more punitive sentences than judges in districts that use non-competitive retention elections (Gordon and Huber 2007).

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police officers and trained participants learn to more effectively identify and use information other than race' (Singh et al. 2020, 566).

The cognitive psychology literature finds that the more a decision task is repeated, the less cognitive effort it requires; by performing a decision task over and over again, and learning from their mistakes, practised decision-makers have available greater cognitive resources for taking into account detailed individuating information in any individual case (Tobin and Grondin 2015). Conversely, decision-makers unused to the challenge may be too cognitively depleted to apply the required mental resources. As Devine et al. put it, 'overcoming prejudice is a protracted process that requires considerable effort' (2012, 1,268). These studies support the notion that, as judges gain experience, they may make more individuating, accurate, and unbiased decisions.

Practice and experience are likely to matter especially when the decision task is difficult. Adding time pressure and other forms of 'cognitive load' – that is, making the decision situation more difficult – exacerbates heuristic thinking and decreases accuracy (Glaser and Knowles 2008; Govorun and Payne 2006; Kleider, Parrott, and King 2010). A primary mechanism for this cognitive depletion effect is a weakened ability to control one's thought process (Govorun and Payne 2006). Although judges have more time than police officers to confront possible danger on the street, they too must make decisions about whether the person before them poses a safety risk, and do so in a highly compressed time window. The time pressure and lack of ready information would be much more demanding for judges who lack experience in making decisions accurately.

Thus, one way to examine whether the heuristic-inducing situation matters is to compare experienced and inexperienced judges. Following this literature, we use judge experience to test whether the decision process creates cognitive distortions, as these would especially affect judges with little experience (Arnold et al. 2018).

Having developed an experience-based explanation of racial disparity in PI decisions, we turn to the effect of PI on voting. We ask whether the shadow carceral state may inhibit the democratic practice of voting.

The Demobilizing Effect of Pretrial Incarceration

Does PI reduce voting? The answer is not obvious. The literature on the formal carceral state finds mixed effects from post-conviction incarceration. Some studies find no decrease in participation (Burch 2011; Gerber et al. 2017; Walker 2020), while others find a large negative effect (Lerman and Weaver 2014), especially for Black defendants (White 2019). We hypothesize that the shadow carceral state reduces voting among Black and, perhaps, Hispanic defendants.

There are several reasons why PI would reduce voting.[6] We do not aim for definitive tests of these mechanisms. They are the reasons why we expect PI to reduce voting by poor minority defendants. Our main focus will be on the role of judges in mitigating this effect.

First, PI reduces concrete resources and imposes substantial costs (Dobbie, Goldin, and Yang 2018; Gupta, Hansman, and Frenchman 2016; Heaton, Mayson, and Stevenson 2017; Stevenson 2018). It increases unemployment by 9 percentage points for up to four years, causing substantial income loss (Dobbie, Goldin, and Yang 2018, 204). The political science literature on political participation has established that these resources are a significant antecedent of voter turnout (Verba, Schlozman, and Brady 1995). Moreover, even short stints of financial hardship reduce turnout for people already living in distressed economic circumstances (Schaub 2021). Thus, PI may reduce voting by decreasing the material resources needed to cast a vote (Schlozman, Verba, and Brady 2012).

[6]Some studies find that carceral contact increases political participation (Burch 2013; Walker 2020). For example, Garcia-Rios et al. (2023) find that people of colour who report personal racial discrimination and have high linked fate report more political participation if they have contact with authoritarian institutions. However, Garcia-Rios et al. do not find this mobilizing effect on voting (2023, Appendix Table A4).

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In addition, PI may work through a symbolic mechanism. The process violates common notions of fairness and basic dignity. A system that appears to ignore the principles of liberty enshrined in the Constitution may come to be regarded as hopelessly undemocratic (Lerman and Weaver 2014). For example, those detained pretrial are more likely to then plead guilty, often because they realize that, as their detention drags on, they may lose their job and incur other negative consequences (Dobbie, Goldin, and Yang 2018; Heaton, Mayson, and Stevenson 2017). Taking a plea is, in turn, associated with negative perceptions of the criminal justice system because detained defendants often plead guilty despite the lack of evidence against them (Lerman, Green, and Dominguez 2022). This abrogation of justice may alienate them from the government and symbolize its lack of accountability and responsiveness to its citizens (Lerman and Weaver 2014). Importantly, this process may enhance distrust of government in all its forms and functions (Weaver, Prowse, and Piston 2020). As Gimpel, Lay, and Schuknecht (2003) put it, 'evaluations of a variety of institutional authorities – teachers, police, judges – are positively associated' (145). In the political science literature on voting, trust in government responsiveness is a major symbolic antecedent of voting (Verba, Schlozman, and Brady 1995). PI may reduce voting by reducing that trust.

These resource and symbolic mechanisms would especially apply to Black and, perhaps, Hispanic defendants. Regarding the resources mechanism, Black and Hispanic defendants have fewer assets (even before arrest) (Page, Piehowski, and Soss 2019). They tend to reside in underserved areas with more entrenched poverty (Lerman and Weaver 2014). Encounters with the carceral state reduce Black defendants' earnings and pose a higher barrier to employment for them than they do for White defendants (Apel and Powell 2019; Harris and Harding 2019). These racial disparities in resources may mean that PI reduces turnout more for Black and Hispanic defendants. Consistent with this hypothesis, some studies of the impact of formal conviction find that incarceration reduces turnout among Black, not White, defendants (White 2019).

The symbolic mechanism would also hold especially for Black and, to some extent, Hispanic defendants. As we noted, Black defendants are much more likely to be detained pretrial even when accounting for their prior record and the nature of the charges. Consequently, in many large jurisdictions, those detained would be surrounded primarily by Black and Hispanic defendants (Page, Piehowski, and Soss 2019). It would be apparent to those detained – in a literal, visual sense – that the system is racially disparate. Black and Hispanic defendants would thus be especially likely to conclude that it is unjust.

These beliefs are in line with studies of procedural justice. The fairness of the process can be more consequential for people's attitudes about the criminal justice system than the favourability of the outcome (Tyler 2001). Black individuals are far more likely than White individuals to perceive the criminal justice system as unfair (Hurwitz and Peffley 2005). Furthermore, Black individuals are also more likely to apply that perception to assessments of specific events involving misdeeds by police (Hurwitz and Peffley 2005). These symbolic mechanisms may reduce voting, especially by Black and Hispanic detainees, who are more likely to bear the brunt of the system's injustice – and to generalize it to the government's view of their lack of worth as citizens (Lerman and Weaver 2014). Several recent studies find that perceptions of racial discrimination are associated with lower turnout for young Black individuals (Cohen 2010; Gimpel, Lay, and Schuknecht 2003). Black detainees would be especially likely to be affected by this symbolic mechanism.[7]

What is the role of judge race in this symbolic process of demobilization? On one hand, descriptive representatives may elicit greater trust (Bobo and Gilliam 1990). For example, a hypothetical news article reporting that the Black percentage of judges reflects the Black percentage of the population increases Black respondents' institutional trust (Scherer and Curry 2010). Black judges, then, may not trigger political alienation by Black defendants, or at least less so than

[7]We also test, and reject, the possibility that Black detainees are more strongly affected because of their higher prior voting propensity.

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White judges do. And, likewise, for Hispanic defendants facing Hispanic judges. In that case, the symbolic mechanism for reduced turnout by Black or Hispanic defendants may not hold when PI is assigned by a same-race judge.

However, as we noted, there is reason to expect that judge race will not substantially affect the racial disparity in PI. Defendants may not perceive minority judges as fairer. Consequently, judge race may not mute the effect of PI on turnout. That is, conditional on being detained, Black or Hispanic defendants would respond similarly to decisions by Black, Hispanic, and White judges. That would be consistent with studies finding a null impact of judge race on perceptions of judicial fairness. For example, the number of Black judges in the county has no effect on Black defendants' perception of the fairness of judges in Mississippi (Overby et al. 2005). This null effect is also found in the literature on officer race and community trust in the police (Brunson and Gau 2015), and is consistent with studies finding a null effect of a representative's race on political trust (Fowler, Merolla, and Sellers 2014). Thus, same-race judges may not mute the effect of PI. That would be consistent with the notion that the institution itself matters beyond the effect of individual judges (Harris 2024).

In sum, we expect a negative effect of PI on voting by Black and, perhaps, Hispanic defendants, especially among those living in poverty.[8] If the decision situation matters as we hypothesized, we would see the effect among judges of any race, but not among experienced judges.

Data

We obtained records from Miami-Dade County for all individuals arrested and charged with criminal offences. We analyzed those who were arrested and had a first appearance bail hearing during the period between the November 2008 and November 2016 general election days. The records provide detailed information about the defendant (name, date of birth, gender, race) and their case, including the charges at the time of arrest, the timing of arrest and release from custody, and the judge who set the conditions of pretrial release.[9] As discussed below, our identification strategy relies on defendants who had a first appearance bail hearing that occurred on a weekend.[10] Thus, we omit all other cases from our sample.[11]

We merge each defendant with the official state voter files from Florida after the 2008, 2012, and 2016 elections, by first name, last name, and date of birth, using probabilistic record linkage (Enamorado, Fifield, and Imai 2019).[12] To account for the possibility that defendants were residents of other states during or after their case, we repeat the merges with all other states' voter files, using probabilistic merge on the same fields.[13] Appendix C has further details.

[8]Hispanic defendants may be less likely than Black defendants but more likely than White defendants to experience the resource losses and hear the symbolic message (Page, Piehowski, and Soss 2019; Walker 2020).

[9]The court records include a person identifier. However, we found a non-negligible percentage of exact name and date of birth combinations associated with different IDs (9 per cent). We thus generated a new person identifier using probabilistic record linkage and clerical review (Appendix B.2). Results are almost identical to the courts' identifier (Appendix Table A8).

[10]Weekend and weekday cases are similar. The largest covariate difference between them is only 2 percentage points (see Appendix Table A4).

[11]We omit cases in which the defendant secured release before the first appearance. We also omit a small number of cases from judges with sparse data (replacement judges); cases involving serious charges that rarely result in pretrial release regardless of the assigned judge; defendants younger than 18 at the time of treatment; defendants released to U.S. immigration enforcement (non-citizens); and cases that suffer from other data limitations (see Appendix B.3).

[12]The official voter files from FL were provided by L2, Inc., a national non-partisan firm that collects and prepares voting records. The official Florida files we used were not altered by the company. We use the first snapshot of the voter file L2 collected following the general election. The turnout counts from the files we use match official counts closely, within 0.03–0.62 per cent.

[13]For these merges, we use the 'uniform' voter files prepared by L2, for which L2 incorporates additional data sources, like National Change of Address (NCOA), and standardizes formats across states.

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Following Dobbie, Goldin, and Yang (2018) and Stevenson (2018), we measure PI as being detained for more than three days after the bail hearing. This threshold has been used by advocacy groups and researchers based on evidence that a) the judge in the first bail hearing (our instrument) has the most influence over the defendant's pretrial incarceration status within the first three days before defendants can petition for and secure modified pretrial conditions, and b) the more severe collateral effects of pretrial incarceration typically begin after three days (Dobbie, Goldin, and Yang 2018; Stevenson 2018).[14] As we will show, the results are robust to alternative measures of PI.

After constructing the instrument, we make several additional modifications to the sample. First, we omit cases whose pretrial release decisions are outliers in relation to other decisions made by the same judge in the same year and violent charge level ($N = 3,\ 288$).[15] Second, we drop cases in which the defendant is likely already disenfranchised due to a prior felony conviction ($N = 6,\ 710$; Appendix B provides details). Third, because our outcome of interest (general election turnout) is at the defendant-level and observed only once per general cycle, if a defendant had multiple weekend cases, we select only the defendant's last weekend case for each election cycle (2008 − 2012 and 2012 − 2016).[16,17,18]

Our final sample includes 45, 107 cases, involving 42,950 unique defendants and only one case per defendant in each four-year election cycle (their last weekend case).[19] As shown in Appendix Table A3, 23 per cent of the defendants were detained pretrial. The average pretrial incarceration lasted twenty-one days and the average bail was approximately $9,500. Compared to those released, defendants incarcerated pretrial were more likely to be male, Black, reside in zip codes with incomes below the median, committed a drug-related offence, and have a prior case.

Methods

Quasi-random Assignment of Bail Judges

PI decisions may be endogenous to many defendant or case characteristics associated with turnout. And so, OLS regression could lead to biased estimates of the average treatment effect of PI on turnout for the population of defendants (White 2019).[20] We address this issue by taking advantage of the quasi-random assignment of bail judges to weekend cases (following Arnold et al. 2018; Dobbie, Goldin, and Yang 2018).

Specifically, in Miami-Dade, weekend bail cases are assigned to judges who spend weekdays as trial court judges. On weekends, they take turns serving as judges in felony and misdemeanour bail hearings. Within a few hours of arrest, the court system automatically assigns the defendant to the bail judge on duty. As a result, on weekends, defendants cannot select their bail judge. They

[14]See Appendix B.4.

[15]The inclusion of outliers does not affect our substantive findings (Appendix Table A8).

[16]If we had included all cases from a defendant in an election cycle, we would add variation in treatment assignment without the possibility for variation in the outcome, which occurs only every four years. Therefore, we focus on each defendant's last case before the election while controlling for prior cases. Selecting the first case in each election cycle without accounting for multiple later treatments would not be possible because controlling for post-treatment variables would introduce post-treatment bias. Selecting the last weekend case of a defendant does not result in a larger sample of individuals detained on election day.

[17]As we will explain below, less than 2 per cent of defendants in our sample are still detained on election day. The exclusion of these observations does not alter our conclusions.

[18]Omitted cases are included in the instrument construction and improve its precision.

[19]Some defendants (≈2.5 per cent) appear twice in the sample because they had a weekend case in both election periods and our data spans two election cycles (2008–2016). If a defendant was arrested on multiple cases or if their case was later consolidated or transferred to a new case number, we combine all such cases in one observation.

[20]Appendix Table A5 reports OLS estimates of the average treatment effect (ATE) of PI on turnout. While OLS estimates may be biased, as discussed below, Appendix Table A17 presents results for an unbiased estimate of the ATE of PI on turnout following Aronow and Carnegie (2013).

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are assigned by the court system to whichever judge happens to be assigned to that day. Importantly, judges are allocated to weekends in a quasi-random fashion – in alphabetical order by last name (see Appendix B.1 for details). Furthermore, there is significant variation in judges' tendencies to set incarceration-inducing bail amounts: as we will show, some judges are consistently more likely to set higher bail amounts that result in pretrial incarceration compared to judges deciding observably similar cases.

We use this quasi-random assignment of bail judges to construct an instrument to address endogeneity concerns between PI and turnout (see McDonough, Enamorado, and Mendelberg 2022). Using Two Stage Least Squares (2SLS), we identify the local average treatment effect (LATE) for defendants on the margin of incarceration and release. In other words, we identify the effect of PI on turnout for a defendant who would be released by a lenient judge but may have been detained pretrial had they been assigned to a harsher judge.[21]

Instrument

For the instrument, we construct a measure of judge punitiveness net of the focal defendant. We allow punitiveness to vary by time and case severity. Specifically, the instrument leaves out the defendant's case(s) and uses all other cases assigned to that judge in that year with that severity type (see Appendix B.4.2). To measure severity, we use an indicator variable for violent charge (see Appendix B.3.3). The instrument represents the proportion of other cases with the same violent charge indicator decided by that judge that year which resulted in PI (Aizer and Doyle 2015; Stevenson 2018).

There are 156 bail judges in our analysis sample, with a median number of 107 cases per judge-year-violent charge.[22] The average leave-out-case PI rate is 0.24 (s.d. = 0.12). As we go from the least to the most punitive judge, the likelihood of PI increases by 44 percentage points for defendants with non-violent charges, and 53 points for those with a violent charge.

Testing The Demobilizing Effects of Pretrial Incarceration

To estimate the effect of PI on voting, we rely on two-stage least squares (2SLS). The first stage is:

$$P_{dtjh} = \alpha_0 + \alpha_1 Z_{dtjh} + X_{dt}^{\top}\Omega + \epsilon_{dtjh} \tag{1}$$

and the second stage is:

$$T_{d,e} = \beta_0 + \beta_1 \widehat{P}_{dtjh} + X_{dt}^{\top}\Gamma + \varepsilon_{dtjh} \tag{2}$$

where $e \in \{2012, 2016\}$ indicates an election, d is for defendant, j is for judge, t is for year of bail, and $h \in \{\text{violent, non-violent}\}$ is the offense violent charge level. $T_{d,e}$ is an indicator for voting in election e, P_{dtjh} is a binary variable measuring PI (¿3 days), $\hat{P}_{dtjh}$ represents the predicted values for PI from the first stage, Z_{dtjh} is our instrument, and X_{dt} is a set of defendant- and case-level covariates and fixed effects. Defendant-level covariates are: age, age squared, gender, race, voting-age-ineligible, pretreatment turnout (previous election), and pretreatment registration. Case-level covariates are indicators for firearm, robbery, drug-related crime, and prior arrest. Fixed effects are hearing day, month, year, and violent charge. While the quasi-random assignment of judges allows us to omit many major predictors of turnout, we include variables potentially associated both with PI and with turnout (such as resources), in case judge assignment is

[21]Our results are not representative of defendants who would always be detained pretrial regardless of the type of bail judge (always-takers) or of defendants who would never be detained pretrial (never-takers).

[22]The judge-year median number of cases is 163.

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not fully random. This approach also increases statistical precision. In doing so, we follow other studies of quasi-randomly assigned judges (Dobbie, Goldin, and Yang 2018; McDonough, Enamorado, and Mendelberg 2022; Stevenson 2018). Because bail judge rotations are set by year, we follow Dobbie, Goldin, and Yang (2018) and Cameron and Miller (2015) and report bootstrap standard errors (based on 500 samples) clustered at the judge-by-year level. Additionally, Appendix Table A8 presents almost identical findings when using heteroskedastic-consistent standard errors and when clustering at the judge level.

Following McDonough, Enamorado, and Mendelberg (2022), to preview the connection between judge punitiveness and the outcomes of interest, Appendix Figure A1 displays the non-parametric fit between the residualized instrument and residualized pretrial incarceration (left panel), as well as residualized turnout (right panel). Residualizing involves removing the variation attributed to fixed effects. As expected, the figure reveals a positive (reduced form) correlation between the instrument and pretrial incarceration, along with a negative correlation between the instrument and voting. Additionally, the figure depicts the residualized distribution of the instrument, confirming that the extremes of the distribution are not driving these relationships. Our analysis indicates ample variation in the instrument, allowing us to predict both the endogenous variable (pretrial incarceration) and the outcome (turnout).

Instrument Validity

For valid inference, the instrument must meet the exclusion restriction, be sufficiently correlated with the endogenous variable (PI), and exhibit monotonicity. In this section, we evaluate each requirement.

First, as we explained above, judges are assigned quasi-randomly to cases, making our instrument exogenous. Furthermore, if the instrument is exogenous, pre-existing defendant and case covariates should be uncorrelated with the decision tendencies of the assigned judge. Following Dobbie, Goldin, and Yang (2018), Stevenson (2018), and McDonough, Enamorado, and Mendelberg (2022), in Appendix Table A6 we regress the instrument (punitiveness) on the covariates and the fixed effects described above. Though we detect statistically significant correlations between a few individual covariates and our instrument, the magnitude of the correlations is exceedingly small. In addition, to rule out the possibility that those small correlations may introduce bias, we construct a measure of predicted turnout such that all variation in it is coming from defendant- and case-level covariates. As shown in Appendix Figure A2, this measure and the residualized instrument are not correlated ($r = 0.002$).

The exclusion restriction states that bail judges cannot influence turnout through means other than the bail hearing itself. The fact that defendants cannot choose their bail judge, that bail hearings are brief, and that there are no further interactions between the judge and defendant after the hearing suggests that judge punitiveness affects turnout only through PI. While there is no direct test for the exclusion restriction, these reasons make the assumption plausible (Dobbie, Goldin, and Yang 2018; Stevenson 2018).

Second, we assess instrument strength. Appendix Table A7 presents the first stage estimates. The instrument is a significant predictor of pretrial incarceration. A one-unit increase in judge punitiveness is associated with a 0.74 to an 0.80 increase in the likelihood of being detained pretrial. These results make weak instrument bias unlikely.

Third, we assess monotonicity. In our setting, monotonicity requires that punitive judges are more punitive than judges in all cases of violent charge h in year t. In other words, assignment to a more punitive judge increases the likelihood of pretrial incarceration. In settings such as ours, Frandsen, Lefgren, and Leslie (2019) suggest testing for average monotonicity. Appendix Table A7 presents the first stage estimates across a variety of subsets, including race, gender, prior contact with the justice system, and charge types. Across subsets, assignment to harsher judges consistently increases the likelihood of pretrial incarceration.

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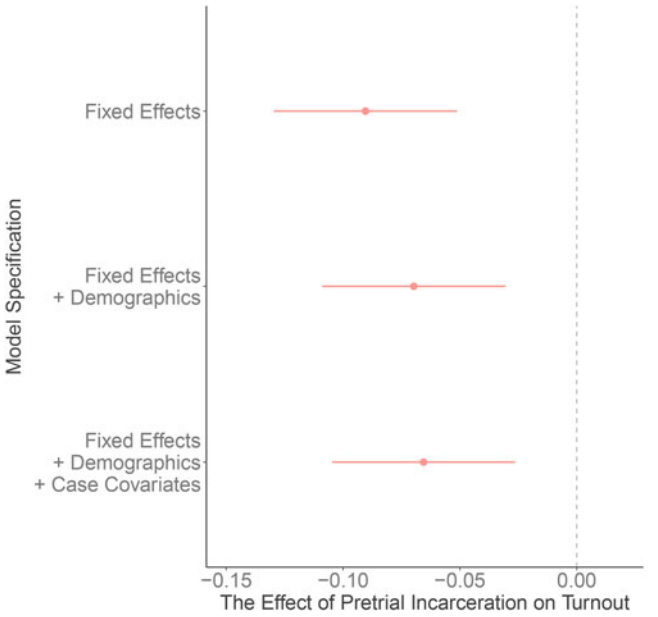

Figure 1. The Demobilizing Effect of PI. Marginal effects from 2SLS estimates with 95 per cent CI.

Results

Main Effect of Pretrial Incarceration on Turnout

Figure 1 presents the 2SLS estimates of the effect of pretrial incarceration on voting. The top estimate includes only fixed effects (day, month, year, and violent charge); the middle estimate includes those and defendant covariates (gender, age, age squared, race, voting-age-ineligible, pretreatment turnout and registration); the bottom estimate includes all those and case covariates (binary measures of prior case, firearm, drug, and property offense). Pretrial incarceration causes a 7–9 percentage point decrease in voting in the subsequent election.

This effect survives a large set of robustness checks and placebo tests. As we show in Appendix Table A8, we obtain similar results if we include outliers on judge punitiveness; use different cut points to define PI (7 and 14 days); use a continuous measure (the logarithm of the number of pretrial detention days);[23] code PI using three categories; use a residualized version of the instrument;[24] use a deterministic instead of probabilistic merge; use bivariate probit instead of two-stage least squares; or include additional controls for a felony charge and any prior conviction. Furthermore, PI does not predict turnout in the election prior to the case (Appendix Table A8) or beyond the first general election post-treatment (see Figure A5 and Table A18 in Appendix F). These tests confirm we are not measuring a spurious correlation with defendants who are less likely to vote. In addition, the effect is located almost entirely among prior voters (Panel A of Appendix Table A9), further evidence that the effect is not spuriously caused by a lower propensity to vote. Finally, because these are LATE effects for compliers, we use weighted 2SLS (with complier weights) to recover an estimate of the ATE from the LATE (Aronow and Carnegie 2013). The ATE is similar to the LATE (see Appendix Table A17).[25]

[23]See also Appendix Figure A4.

[24]Following Dobbie, Goldin, and Yang 2018, we obtain residuals from regressing pretrial incarceration on bail hearing year, month, and violent charge, and then calculate judge punitiveness in year t and violent charge h as the mean of the residuals.

[25]Table A16 in Appendix E shows that compliers and the average defendant are similar (less than 3 percentage points apart) on all demographics and electoral covariates and on having a prior case and the use of a firearm. Compliers are

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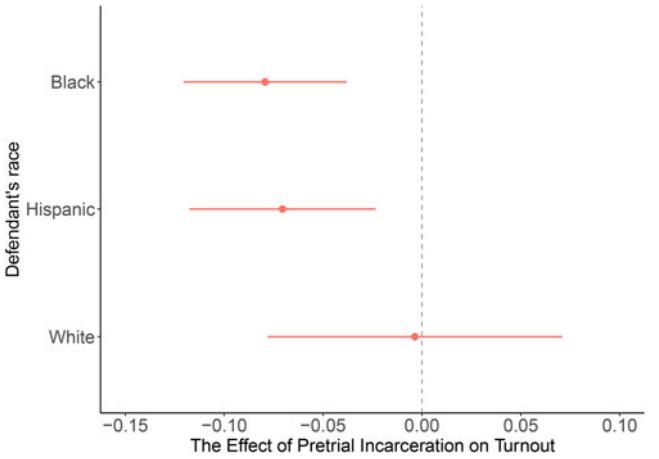

Figure 2. The Demobilizing Effect of PI by Defendant Race. 2SLS marginal effects and 95 per cent CI from a specification including defendant- and case-level covariates, fixed effects, and interactions between PI and race.

Before proceeding, we consider and reject a mechanical explanation: if defendants are incarcerated on election day, they may find it difficult or impossible to vote. That is, PI may demobilize by increasing the chances of post-conviction incarceration (when defendants are not allowed to vote), or because of the difficulty of voting while detained pretrial. However, less than 2 per cent of defendants are still detained on election day, and fewer than one-fifth of cases in the sample result in any form of incarceration post-conviction. Furthermore, as detailed in Appendix B.4.5 and Appendix Table A10, the effect is unchanged or larger if we remove cases more likely to be incarcerated on election day because of proximity to election day (cases that began within 2, 4, and 6 months before election day); if we examine cases with an offence that rarely results in a post-conviction incarceration sentence if convicted; and if we exclude cases likely incapacitated on election day due to the case's actual observed dates of PI and length of post-conviction sentence, regardless of when the case began.[26]

Racially Disparate Effect

As noted, we expect pretrial incarceration to especially affect defendants of colour. To test this hypothesis, we interact race with the instrument (judge punitiveness) and PI. The model includes all covariates and fixed effects. As shown in Fig. 2, pretrial incarceration reduces turnout by 8 percentage points for Black and Hispanic defendants. These effects are strong and precise. By contrast, the effect for non-Hispanic White defendants is nearly zero and statistically indistinguishable from null.[27] In the remainder of the paper, we focus on explaining the effect on Black and Hispanic defendants.[28]

PI is not only more likely to affect defendants of colour, it is more likely to affect poor defendants of colour. To measure poverty, we divide defendants into three groups according to zip code income: below the sample median per calendar year (between 28 and 32 thousand dollars

less likely to have a drug-related offence on file and to have been accused of a property offence, and more likely to have a violent charge.

[26]We caution that excluding observations based on post-treatment outcomes (detention length and sentence) may introduce bias. Nevertheless, our main finding is largely unchanged.

[27]Appendix Table A17 shows the same pattern of racially disparate effects with ATE.

[28]We cannot distinguish the effect on White defendants from the effect on Black and Hispanic defendants due to the very large confidence interval for White defendants. The difference in the effect of PI is indistinguishable from zero for Black vs. White (p-value: 0.18) and Hispanic vs. White defendants (p-value: 0.25).

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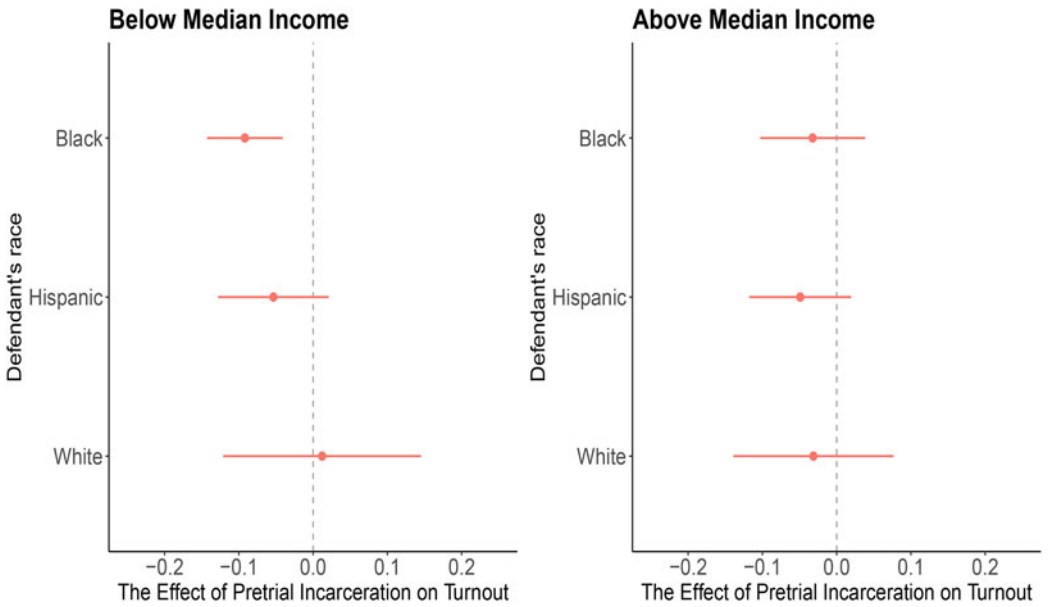

Figure 3. The Demobilizing Effect of PI by Zip Code Income and Defendant Race. 2SLS marginal effects and 95 per cent CI from a specification including defendant- and case-level covariates, fixed effects, and interactions between PI, race, and zip code income.

depending on the year), at or above the median, and unobserved income (see Appendix B.4 for more details). We interact these income groups with race to create race-class indicators and interact these indicators with punitiveness (in the first stage) and with predicted PI (second stage).

As Fig. 3 shows, poverty indeed matters, for Black defendants. The effect is large and statistically different from zero only for Black defendants below median zip code income. By contrast, the effect for White defendants below the median is 0 (with a wide confidence interval). Likewise, the effect for Black defendants above the median is small and highly imprecise.[29] In sum, the pattern is consistent with a race-class disparity. Black defendants living in poor circumstances are the population most clearly affected.[30]

Judge race

So far, we have shown that the shadow carceral state has a racially disparate effect on turnout, and that this process disempowers poor Black defendants. What is it about the shadow carceral state that leads to this racial disparity? It is impossible to randomize people to be 'treated' by shadow institutions versus non-shadow institutions. But we can exploit variation in judges within this shadow institution. As we noted, theories of descriptive representation suggest Black judges may be less likely than White judges to impose harsh measures on Black defendants. Perhaps, then, the dearth of Black judges explains the racial disparity. On the other hand, there are reasons to expect that judging race does not matter. The institution's pressures and expectations may prevent even Black judges from making fair decisions.

[29]We cannot statistically distinguish between any effects in the figure because all but one of them is highly imprecise, but the only precise large effect is for poor Black defendants.

[30]We considered another explanation for racial disparities. In White's (2019) study, racial disparities are partly accounted for by Black defendants' higher vote propensity. In our study, White prior voters are not much affected, however. The negative effect of PI is located almost entirely among Black and Hispanic prior voters (see Panel B of Appendix Table A9). Thus, the hypothesis that racial disparities in the PI effect are due to racial differences in prior voting is not supported, as White defendants are mostly unaffected even when they are prior voters.

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Figure 4. The Demobilizing Effect of PI by Judge and Defendant Race. 2SLS marginal effects and 95 per cent CI from a specification including defendant- and case-level covariates, fixed effects, and interactions between PI, defendant race, and judge race.

Miami-Dade offers sufficient numbers of judges for analysis: 12 Black, 60 Hispanic, and 84 White judges.[31] Each group has sufficient cases for analysis: 3, 604 cases assigned to a Black judge, 15, 956 cases to a Hispanic judge, and 25, 547 cases to a White judge. In addition, we find that judges are seeing the same types of cases and defendants. For example, case- and defendant-level covariates are balanced across judge race and the judge-defendant's race.[32]

We now investigate whether PI decisions vary by judge race. First, we find that the raw PI rates do not vary by judge race, consistent with a null effect of judge race.[33] Second, we find that punitive variance is the same – the distribution of residualized judge punitiveness is almost identical across the combinations of defendant and judge race (Appendix, Figure A3). Thus, we find that Black and Hispanic judges are as punitive as White judges, both with co-racial defendants and with any defendant.

To test the moderating effect of judge race on turnout, we created a binary variable that takes the value of one if the defendant's race matches the race of the judge and zero otherwise.[34] To keep the 2SLS model identified, we interact this race match indicator with the instrument (the judge's punitiveness) and with PI. The results are in Fig. 4. PI reduces turnout for Black defendants regardless of judge race. Black judges do not mute the PI effect for Black defendants. Hispanic defendants likewise do not benefit from Hispanic judges.[35] In addition, as shown in the appendix, judge race does not affect turnout even as a standalone predictor (not interacted), either for all defendants, Black defendants, or Hispanic defendants (Appendix Table A12).

No matter how we estimate the effect, judge race does not matter. The effect of PI is not due to White judges acting out of racial animus, and it is not alleviated by minority judges. In line with

[31]We obtained this data from Arnold et al. (2018) for judges up to 2014 and hand-coded the remaining judges using similar methods (see Appendix B.4.4).

[32]See Appendix Table A11.

[33]The PI rate is 0.22, 0.23, and 0.24 for Black, Hispanic, and White judges respectively.

[34]We use this race match indicator due to the smaller number of Black judges. Appendix Table A13 presents similar results using interactions between the defendant and judge race.

[35]The larger and more precise effect of Hispanic judges on Hispanic defendants is not because Hispanic judges are more punitive; Appendix Figure A3 already ruled that out. Possibly, the demobilizing process itself is more precise for Hispanic defendants facing Hispanic judges.

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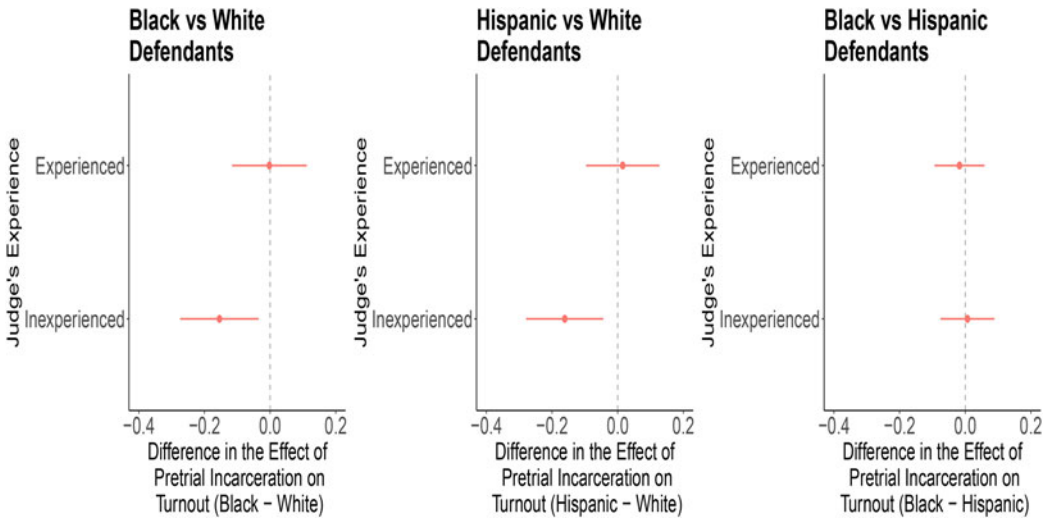

Figure 5. Judge Experience and Racial Disparities. The difference in the marginal effects and 95 per cent CI, from a 2SLS specification including defendant- and case-level covariates, fixed effects, and interactions between PI and judge experience.

much of the existing literature on judge race, our evidence points away from the racial identity of the judge and toward systemic explanations.

Judge Experience

What, then, explains the racial disparity? As noted, we hypothesize that features of the punitive institution may explain it. Rushed bail hearings lack the time necessary for a full consideration of the facts. This setting may foster an over-reliance on widespread associations between stereotypical defendants and threats. Racial bias may be further exacerbated by the institutional role these judges must inhabit. Judges are incentivized to avoid scandalous crimes committed by defendants released pretrial. These judges are accountable to crime-sensitive electorates. Together with the prevalence of racial stereotypes about poverty and crime, this role, and the time-pressured decision situation, may lead to biased cognitive processing and produce a racial disparity.

Crucially, this process would especially affect judges with little experience of bail decisions. As we theorized, these judges would be most vulnerable to the distortions of the situation. System 1 processing would produce biased decisions, especially for judges unused to overriding stereotypes and unpracticed in reaching accurate judgments under time pressure. As judges gain experience on the job, they would become less overwhelmed by the cognitive load and thus less likely to rely on stereotypes.

To measure judge experience, we take the difference (in years) between the bail hearing and the first time that judge appears in the court records. Judges above the median of 12 years are coded as experienced. We then verify that covariates are balanced across judge experience.[36] Next, we find that the raw PI rates of inexperienced judges vary substantially by defendant race: 0.21 for White and 0.29 for Black defendants. As expected, experienced judges' rates do not vary by defendant race, which is consistent with our hypothesis.[37]

[36]Appendix Table A14 shows no large and significant difference in defendant and case characteristics by judge experience, for Black or all defendants.

[37]The raw PI rate for experienced judges is approximately 0.20 across groups. In addition, on average, instrumented punitiveness does not vary across defendant race, for either experienced or inexperienced judges (0.21 and 0.27, respectively), lending credibility to the quasi-random assignment of a judge to the defendant as captured by the instrument.

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To test the hypothesis about judge experience, we follow Arnold et al. (2018). As demonstrated in that study, if there is a racial difference in the outcome for defendants at the margin of PI, that difference is the result of racial bias. In other words, it means that judges are providing Black and Hispanic defendants at the margin of PI with a punishment they withhold from White defendants at the margin of PI. If the judges are unbiased, no such differences should exist. The 2SLS estimation strategy allows us to test this mechanism because it identifies the impact of PI on turnout for defendants at the margin of PI.

Figure 5 presents the results. Inexperienced judges indeed produce disparities in turnout between White and Black or Hispanic defendants at the margin of PI.[38] By contrast, experienced judges do not.[39] In sum, experience explains the racial disparity.

This result is consistent with our theory that institutional features of the shadow carceral state give rise to racially biased cognitive shortcuts. In such a system, where street-level bureaucrats face intense time pressure and are incentivized to prevent re-offences, prevalent heuristics about marginalized subgroups may come into play, regardless of judge race. Also consistent with our theory, as judges are exposed to more accurate information about who actually violates the terms of their release, and gain practice in applying it, they are better able to override these heuristics and reduce racial disparities.

Conclusion

A Growing body of work documents the importance of the carceral state in American politics. Little noticed, however, is the importance of the 'shadow' carceral state. This article underscores the significance of this shadow carceral state by documenting the effects of one of its key institutions: pretrial incarceration. Pretrial incarceration is widespread. In fact, it accounts for many of the people jailed in the United States, the world leader in incarceration. Yet studies are only beginning to examine its impact.

To do so, we merged the population of defendants in a large, diverse county with voter records. We leveraged the quasi-random assignment to harsher bail judges to estimate the causal effect of pretrial incarceration on turnout for those on the margin of pretrial release. We found that pretrial incarceration reduces voting by Black and Hispanic defendants, especially by poor Black defendants. The effect passes a large set of robustness checks and placebo tests and holds only among defendants who had voted before, meaning we are not simply detecting the spurious impact of individual characteristics that predict voting. Moreover, this effect occurs regardless of the race of the judge, and holds only among judges with less experience, those most prone to inaccurate decisions resting on stereotypes. These findings are consistent with the distinguishing features of the shadow carceral state: weaker procedures for due process and equal protection.

How well do these findings generalize to other places? There is reason to expect these effects in large metropolitan areas, places with substantial numbers of people in poverty of whom a disproportionate percentage are Black or Hispanic, who are targeted for punitive interactions with the government (Hood and Schneider 2019). The findings may generalize to other places where many poor people of colour reside.

How well do these effects on voting generalize to other forms of political action? Studies are increasingly documenting the *positive* relationship between carceral contact and non-voting forms of participation in politics (Garcia-Rios et al. 2023; Owens and Walker 2018; Walker 2020; Weaver, Prowse, and Piston 2020). This poses a puzzle for future work to address. One possibility is that voting is different, perhaps because it is much more susceptible to a shock to concrete antecedents. Incarceration substantially reduces employment, income, and housing stability.

[38]Appendix Table A15 presents the results from the 2SLS regressions used to construct Fig. 5.
[39]As shown, Hispanic and Black defendants at the margin of PI are indistinguishable.

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These may be resources that particularly interfere with voting and may not interfere much with protesting, contacting a representative, wearing a campaign button, and other actions that do not require bureaucratic navigation and housing stability.

The study also has implications for policy reforms. Our results suggest that a rushed hearing with little opportunity for a defence and little accountability is part of the problem. In addition, leaving discretion in the hands of poorly trained judicial actors may be problematic. For example, programmes aiming to reduce PI often fail when they allow discretion by prosecutors or judges, while programmes without discretion succeed. As Albright (2021) writes in a study of a successful Kentucky release program, 'the…program…is distinctive in its avoidance of judicial discretion. Bail reform, like many policy reforms, is often at the mercy of the discretion of criminal justice actors, meaning effects are often weaker than expected' (5). This conclusion echoes studies of street-level bureaucrats, which emphasize that discretion, lack of accountability, and workload can explain racial and class disparities. To be sure, simply eliminating discretion may not be effective. The key may be training and accountability.

This study carries troubling implications for the American criminal justice system. The injustices of the shadow carceral system are perhaps even more insidious than those of the formal system. While these practices appear to many who are caught up in the system as violations of basic rights, they have not been so declared by official authorities.

The justice system is not only a backbone of law and order in society, but it also has downstream consequences for democracy, and, in particular, the ability of groups living at a structural disadvantage to exercise equal voice (Lerman and Weaver 2014; Soss and Weaver 2017). Pretrial incarceration is part of a powerful system that pervades the lives of marginalized groups. This system makes it more difficult for these groups to participate in politics and obtain fair representation.

We focused on pretrial incarceration, but the shadow carceral state includes other institutions too. Those include legal financial obligations from fees, debt owed to private actors, and many others (Beckett and Murakawa 2012; Harris 2024; Meredith and Morse 2017; Page, Piehowski, and Soss 2019). The shadow carceral state has been proliferating even as felony incarceration is decreasing. The full reach of these institutions into the political lives of Americans requires further study.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/S0007123424000358.

Data availability statement. Replication data for this article can be found in Harvard Dataverse (Enamorado, McDonough, and Mendelberg 2024) at: https://doi.org/10.7910/DVN/ECV72Y.

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