The Shadow Carceral State and Racial Inequality in Turnout

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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 judgements 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.

Introduction

The "carceral state" has become a well-documented feature of the American political system. Record numbers of Americans – especially poor people of color – regularly encounter harsh treatment by police, and many have been imprisoned (Lerman and Weaver 2014; Soss and Weaver 2017). This carceral contact has increasingly featured as a possible cause of depressed voting among disadvantaged groups in the United States (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 government's power of coercion unconstrained by the procedures of the carceral state (Kohler-Hausmann 2018; Page et al. 2019). The carceral state must consider 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 procedure. 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). Pretrial incarceration 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 pre-trial (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 4 minutes (Gonzalez Van Cleve 2022, 135; Scott-Hayward and Ottone 2018, 172–173).[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

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[1]The jurisdictions include Cook County, Illinois, and Los Angeles and Orange Counties, California.

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 the carceral state literature, we argue that pretrial incarceration reduces turnout for poor people of color – 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 judgements lacking relevant information – the type of decision-making most vulnerable to inaccurate stereotypes of poor people of color (Arnold et al. 2018; Rachlinkski and Wistrich 2017). Because PI is administered with little time to weigh evidence and 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, the 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 et al. 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. 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 et al. 2022; White 2019). We obtained full case records for the entire population of defendants in Miami-Dade County (2008 – 2016), yielding 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 et al. 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 judgements 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 et al. 2013). The effect is strong and precise only for Black and Hispanic defendants, and only for Black residents of poor zipcodes. The effect is not due merely to physical incapacitation while incarcerated. It is not spuriously

[2]In White (2019), this is the number of courtrooms, the "treatment" unit.

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, and 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 color. 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 defense, 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; Lerman and Weaver 2014; White 2019; Gerber et al. 2017). 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 et al. 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 pre-trial (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 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 percent 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, forty percent 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 defense. 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 judgements.

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 (Rabuy and Kopf 2016; Page et al. 2019). 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 (U.S. Commission on Civil Rights 2022; Page et al. 2019). 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 judgement

How would the PI process shape the PI decision? According to studies of street-level bureaucrats, the high degree of discretion afforded bail judges can lead to biased decisions (Lipsky 1980). For example, in "welfare" cases, where case workers have discretion, they are more likely to apply punitive sanctions against disadvantaged racial minorities (Keiser et al. 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).

[3]In addition, over half of people with frequent police interactions also report very frequent contact with bail officials (Garcia-Rios et al. 2021).

[4]The typical local inmate is non-White and earns $16,000 a year (Gupta et al. 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).

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 under-weight 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 and that exclude 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 judgement (Bordalo et al. 2016)? We argue the PI process may produce the latter: quick heuristic decisions driven by 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" (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 et al. 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 defense. 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 behavior 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, 1992). In other words, the perceived probability of a behavior by a group member becomes inflated when that behavior has a higher *relative* frequency at the extreme, even if the *absolute* frequency of that behavior 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 judgements 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 judgement task calling for quick System 1 judgements also exhibit more severe racial bias in a "shooter" task mimicking the quick decisions police offers must make in assessing 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 et al. 2004). That is, System 1 processing may promote implicit bias rather than categorical bias, and 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 judgement. 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 judges' race: judge race should have a null effect. The racial disparity in pretrial incarceration may have roots in heuristics that, with time 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 the judge's 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 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 judgement 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 the 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 (e.g., 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 violent crime, to avoid losing their seat. 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 (2023; 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" (2023, 34). Our detailed review of the literature on judge race effects supports

[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).

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 judge 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. explain, defendants who violate the conditions of their bail and are consequently taken into custody undergo a hearing before a bail judge (2018). These hearings give bail judges an opportunity to learn which factors often lead to unsafe releases. The more that judges see first-hand which 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 practiced 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, police officers and trained participants learn to more effectively identify and use information other than race" (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, practiced 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 and colleagues put it, "overcoming prejudice is a protracted process that requires considerable effort" (2012, 1268). 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 et al. 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 confronting 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.

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 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 et al. 2018; Gupta et al. 2016; Heaton et al. 2017; Stevenson 2018). It increases unemployment by 9 percentage points for up to four years, causing substantial income loss (Dobbie et al. 2018, 204). The political science literature on political participation has established that these resources are a significant antecedent of voter turnout (Verba et al. 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 and Brady 2012).

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 et al. 2018; Heaton et al. 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 et al. 2022). This abrogation of justice may alienate them from 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 et al. 2020). As Gimpel et al. (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 et al. 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 et al. 2019). They tend to reside in under-served areas with more entrenched poverty (Lerman and Weaver 2014). Encounters with the carceral state reduce Black defendants' earnings more, 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 and not White defendants (White 2019).

[6]Some studies find that carceral contact increases political participation (Burch 2013; Walker 2020). For example, Garcia-Rios et al. (2021) find that people of color 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 (2021, Appendix Table A4).

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 et al. 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 favorability 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 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 et al. 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 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 more fair. 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 et al. 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 2023).

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.

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

[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 et al. 2019; Walker 2020).

Data

We obtained records from Miami-Dade County for all individuals arrested and charged with criminal offenses. We analyze 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, on first name, last name, and date of birth, using probabilistic record linkage (Enamorado et al. 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.

Following Dobbie et al. (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 are able to petition for and secure modified pretrial conditions; and b) the more severe collateral effects of pretrial incarceration typically begin after 3 days (Dobbie et al. 2018; Stevenson 2018).[14] As we will show, 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 decision are outliers in relation to other decisions by the same judge in the same year and violent charge level (N = 3288).[15] Second, we drop cases in which the defendant is likely already disenfranchised due to a prior felony conviction (N = 6710; 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

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

[10]Weekend and weekdays 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%.

[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.

[14]See Appendix B.4.

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

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% of the defendants were detained pretrial. The average pretrial incarceration lasted 21 days and the average bail was approximately $9,500. Compared to those released, defendants incarcerated pretrial are more likely to be male, Black, reside in zipcodes with incomes below the median, have a drug-related offense and 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 et al. 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 misdemeanor 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 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 et al. 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

[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% 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 ($\approx$ 2.5%) 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).

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 and 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 that 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), $\widehat{P}_{dtjh}$ 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 not fully random. This approach also increases statistical precision. In doing so, we follow other studies of quasi-randomly assigned judges (Dobbie et al. 2018; McDonough et al. 2022; Stevenson 2018). Because bail judge rotations are set by year, we follow Dobbie et al. (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 et al. (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

[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.

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, preexisting defendant and case covariates should be uncorrelated with the decision tendencies of the assigned judge. Following Dobbie et al. (2018), Stevenson (2018), and McDonough et al. (2022), in Appendix Table A6 we regress the instrument (punitiveness) on the covariates and 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 et al. 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 a 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 et al. (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.

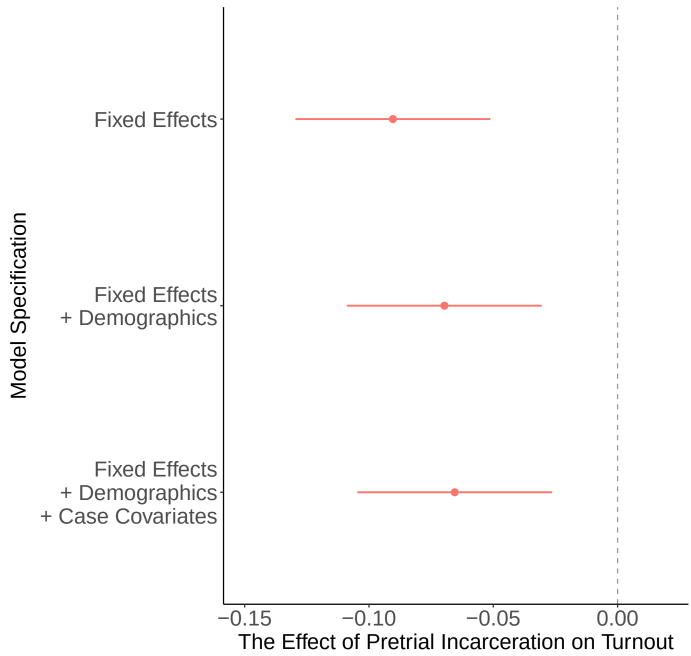

Figure 1: **The Demobilizing Effect of PI.** Marginal effects from 2SLS estimates with 95% CI.

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 cutpoints 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).

[23]See also Appendix Figure A4.

[24]Following Dobbie et al. (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.

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]

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% 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 offense 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 color. To test this hypothesis, we interact race with the instrument (judge punitiveness) and with PI. The model includes all covariates and fixed effects. As shown in Figure 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 color; it is more likely to affect poor defendants of color. 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 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 Figure 3 shows, poverty indeed matters, for Black defendants. The effect is large and statistically different from zero only for Black defendants below median zipcode income. By contrast, the effect for White defendants below the median is 0 (with a wide confidence interval).

[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 a firearm. Compliers are less likely to have a drug-related offense on file and to have been accused of a property offense, 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).

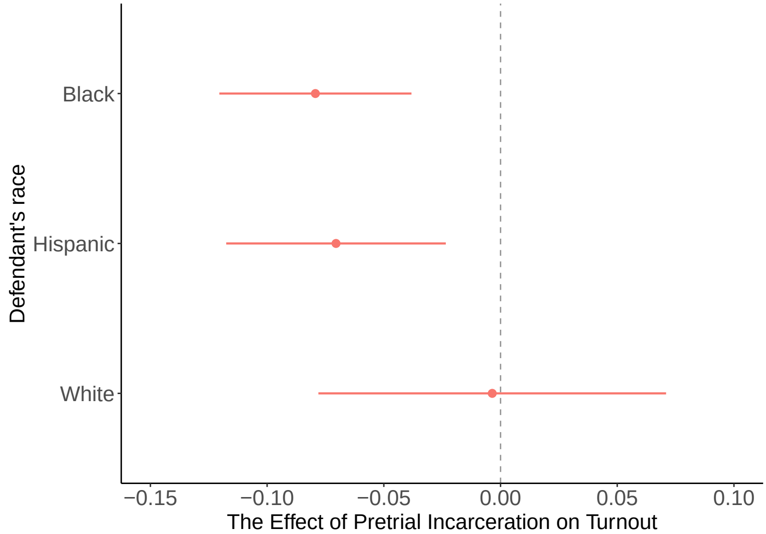

Figure 2: **The Demobilizing Effect of PI by Defendant Race.** 2SLS marginal effects and 95% CI from a specification including defendant- and case-level covariates, fixed effects, and interactions between PI and race.

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 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 judge 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.

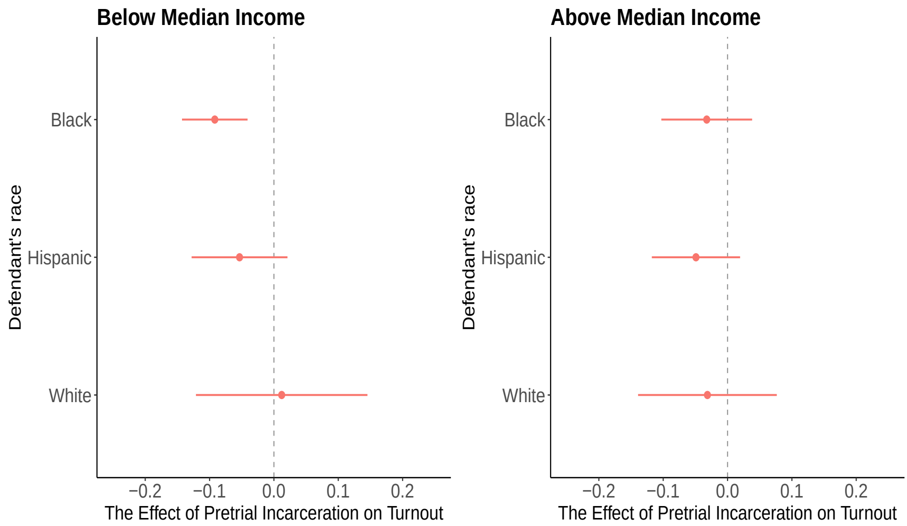

Figure 3: **The Demobilizing Effect of PI by Zip Code Income and Defendant Race.** 2SLS marginal effects and 95% CI from a specification including defendant- and case-level covariates, fixed effects, and interactions between PI, race, and zip code income.

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 judge-defendant 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 (judge punitiveness) and with PI. The results are in Figure 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,

[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 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.

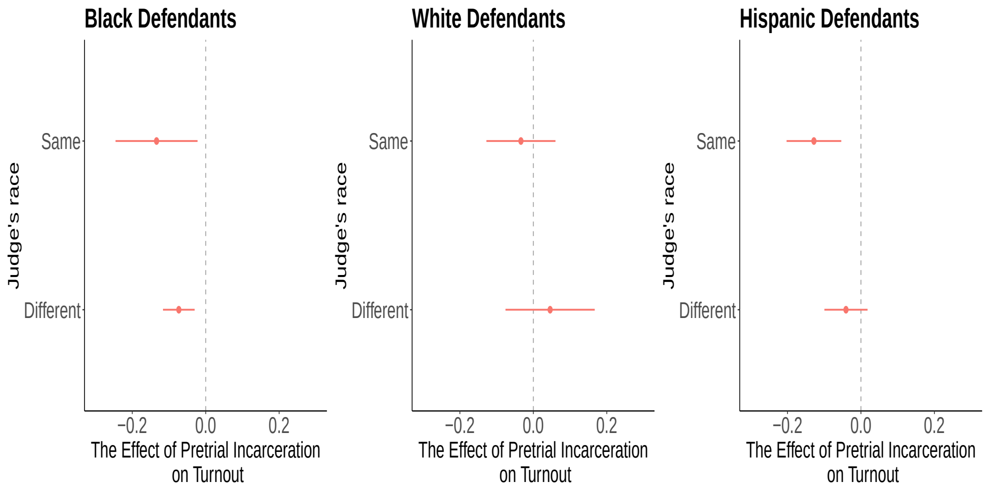

Figure 4: **The Demobilizing Effect of PI by Judge and Defendant Race.** 2SLS marginal effects and 95% CI from a specification including defendant- and case-level covariates, fixed effects, and interactions between PI, defendant race, and judge race.

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 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. The rushed bail hearing lacks the time necessary for full consideration of the facts. This setting may foster an over-reliance on widespread associations between stereotypical defendants and threat. Racial bias may be further exacerbated by the institutional role these judges must inhabit. Judges are incentivized to avoid scandalous crime 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 with 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 over-riding stereotypes and unpracticed in reaching accurate judgements 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' rate does not vary by defendant race, consistent with our hypothesis.[37]

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-offenses, 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 over-ride 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 paper 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.

[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 judge to defendant as captured by the instrument.

[38]Appendix Table A15 presents the results from the 2SLS regressions used to construct Figure 5.

[39]As shown, Hispanic and Black defendants at the margin of PI are indistinguishable.

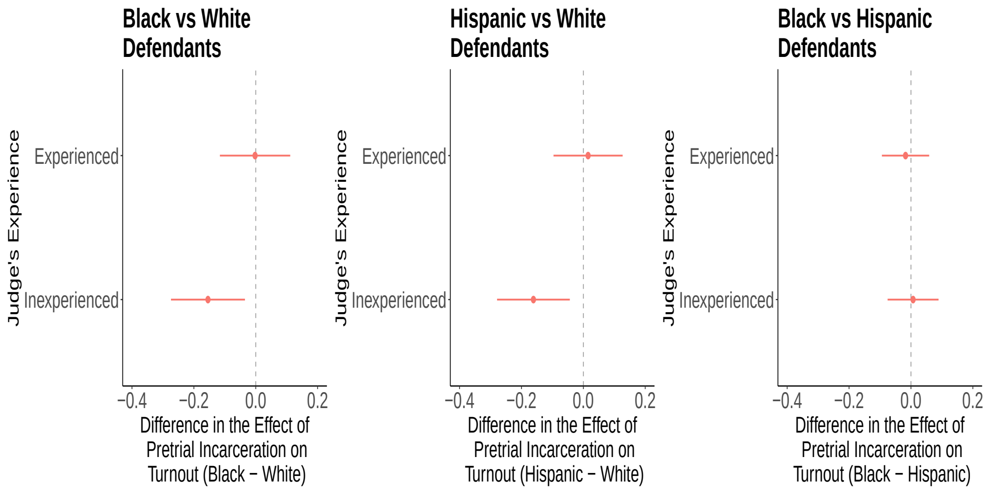

Figure 5: **Judge Experience and Racial Disparities.** Difference in the marginal effects and 95% CI, from a 2SLS specification including defendant- and case-level covariates, fixed effects, and interactions between PI and judge experience.

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, and who are targeted for punitive interactions with government (Hood and Schneider 2019). The findings may generalize to other places where many poor people of color 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. 2021; Owens and Walker 2018; Walker 2020; Weaver et al. 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. These may be resources that particularly interfere with voting and may not much interfere 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 defense 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, programs aiming to reduce PI often fail when they allow discretion by prosecutors or judges, while programs without discretion succeed. As Albright (n.d.) 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; it also has downstream consequences for democracy, and in particular, the ability of groups living in 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 2016, Meredith and Morse 2017, Page et al. 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.

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Supplemental Appendix for "The Shadow Carceral State and Racial Inequality in Turnout."

Contents

A Literature on the Effects of Judge Race

The literature on judge race effects on racial disparities in criminal cases yields mixed results (Harris and Sen, 2019). Some studies find an effect, others find mixed results for judge race, while still others find a null effect. To be sure, these inconsistent effects may be due to many differences across the studies (e.g., different locations, periods, case types, legal settings, control variables, data quality, or research design—which is almost entirely observational). Nevertheless, overall, judge race does not have clear, consistent effects. That is in line with our own study.

Some studies find that minority judges are less likely to punish same-race defendants. In one study, Hispanic-Anglo disparities are produced by Anglo but not Hispanic judges (Holmes et al., 1993). Kastellec (2021) finds a similar pattern for Black-White disparities among as-if randomly assigned judges in appeals panels considering death penalty cases. Specifically, the assignment of a Black vs. White judge to all-non-Black panels substantially increases the likelihood of granting appeals by Black defendants only, suggesting that Black judges are less punitive than White judges regarding Black defendants specifically. Finally, in a hypothetical scenario, White judges overwhelmingly indicate they would convict a violent defendant, regardless of defendant race, while Black judges given the Black defendant were about half as likely to indicate they would convict as those given the White defendant (Rachlinski et al., 2009).

Other studies find mixed results. In Abrams et al. (2012), randomly assigned judges in felony cases differ by race on sentence length but not on the decision to incarcerate: Black (vs. White) judges have smaller anti-Black disparities in sentence length (374). However, even on sentence length, Black judges do exhibit some anti-Black disparity. Thus, this study finds that Black judges are less racially biased but do exhibit some racial bias. In Schanzenbach (2005), the presence of more Black or Hispanic judges in the district does not mute the racial disparity in punishment overall, but does for "less serious" crimes. In Welch et al. (1988), in incarceration decisions, White judges show a racial disparity and Black judges do not, while on sentencing severity, White judges have no disparity and Black judges slightly favor Black defendants.[1]

Still other studies find no effect of judge race. These studies typically find the same racial disparity in punishment regardless of judge race (Spohn, 1990; Uhlman, 2002). For example, Spohn (1990) compared Black and White judges in a large metro area, and found a similar tendency to punish Black defendants more. Notably, some studies find that even when they treat Black and White defendants the same, Black judges may be more punitive toward all defendants (Steffensmeier and Britt 2002; however, Cohen and Yang 2019 find the opposite – Black judges issue shorter

[1] Welch et al. (1988) includes only 10 Black judges, making inferences more uncertain.

sentences).

In sum, then, these studies do not give us strong reason to expect judge race effects on racial disparities in criminal cases.

One consistent finding, in line with ours, is the notable variance among judges, including among Black judges. For example, Abrams et al.'s (2012) main finding is that individual judges vary considerably in their racial disparity in incarceration; in fact, difference within race is larger than difference across race. As Uhlman (2002) concludes: "as a group Black judges establish sanctioning patterns only marginally different from those of their White colleagues. These minor race-related disparities stand in marked contrast to individual judicial behavior which is more strongly associated with case outcome... Black judges display behavioral diversity unrelated to their common racial background" (884).

B Court Data

B.1 Overview of the Bail System in Miami-Dade

In this subsection, we summarize the features of the Miami-Dade Bail System that are central to our research design. We draw on Dobbie et al. (2018) and Arnold et al. (2018), who extensively studied court systems in Miami-Dade and Philadelphia, and on primary sources from the Miami-Dade court system (our information requests to the Eleventh Judicial Circuit Court of Miami-Dade County, and our review of Administrative Orders issued by the Chief Judge of the Eleventh Judicial Circuit Court of Miami-Dade County).

Like the US Constitution, Florida guarantees the right to be considered for pretrial release to most defendants. As Arnold et al write, "according to Article I, §14 of the Florida Constitution, '[u]nless charged with a capital offense or an offense punishable by life imprisonment...every person charged with a crime...shall be entitled to pretrial release on reasonable conditions"' (2018, Appendix p. 46). In Miami-Dade, the bail hearing determines if there is probable cause to detain the defendant, and what (if any) conditions to set on release.

Bail judges typically have four options. First, they can "release on recognizance", and accept the defendant's word that they will return for their arraignment. Second, they can set non-monetary requirements for release. For example, defendants may be subject to monitoring to ensure they attend future court dates. Third, judges can require financial bail as a condition of release. Most commonly, defendants must pay 10 percent of the bail amount. Those who cannot make this payment and wish to be released must borrow this 10 percent from commercial bail bond companies. To do so, the defendant (or their relatives) must put up some form of material property as collateral.

Bondmen usually levy a non-refundable fee, typically 10 percent of the bail amount. They are legally entitled to seize the defendant's or guarantor's assets for failure to pay. (The role of bail bond companies has intensified criticism that the PI system is punitive and unjust (Page et al., 2019).) Finally, the judge may deny bail and detain the defendant until their trial.

In deciding between these options, and in setting bail amounts, judges are allowed broad discretion. They are expected to factor in a variety of considerations, including the strength of evidence in the case, whether the defendant has failed to meet prior release conditions, the severity of the charges, and most importantly, how much physical danger the defendant poses to the community.

Below we list several additional features of the Miami-Dade bail system that are central to our research design.

1. Most defendants in Miami-Dade are eligible for prompt release without a hearing, by posting a predetermined amount from a standard bail schedule, which categorizes offenses by severity. If unable to post the standard bail listed in the bail schedule, defendants have a bail hearing within 24 hours where they can request reduced amounts or an alternative release decision. According to Arnold et al, about 70 percent of defendants have a bail hearing (2018, Appendix p. 47). In our data, 60 percent do.

2. There is a separate bail hearing for felony and misdemeanor cases. Both are conducted via video conference. Weekend hearings occur on Saturdays and Sundays at 9:00 AM.[2]

3. During the bail hearing, the bail judge assesses probable cause for detention and determines which bail conditions to set, if any. Importantly, the bail judge can use the bail hearing to adjust the bail amount based on case specifics and arguments from the defendant, their defense counsel, and the prosecutor. However, as we noted, the compressed time window makes this attention to individual details difficult. In addition, while monetary bail amounts often align with the standard bail schedule, the choice between monetary and non-monetary conditions varies widely among judges in Miami-Dade (Arnold et al., 2018).

4. Unlike weekday bail hearings, which are handled by one judge, weekend cases are heard by a bail judge selected from a set of weekday trial judges who are called to serve as weekend bail judges on a rotating basis. [3] The Miami-Dade Court System assigns weekend bail shifts in

[2]https://www.miamidadeclerk.gov/clerk/criminal-court.page.

[3]Since 1979, the Eleventh Judicial Circuit of Florida has implemented a blind filing system for case assignments. This system ensures that cases are filed equally among the various sections of the court in an "unpredictable manner," as stated in Administrative Order 79-4 on page 1.

chronological sequence to judges by alphabetical order of their last names.[4] These weekend bail judges are typically assigned one weekend a year.[5]

5. As a check, we examined the alphabetical order of actual weekends and the actual number of weekend shifts per judge-year in our data. Reassuringly, 82% of the time the assigned alphabetical order is followed, and 98% of judges in our data served at most two shifts in a calendar year, with an average of 1.2 and a median of 1 (see Table A1).

6. In these cases, defendants cannot select their bail judge. All weekend cases are assigned to the judge on duty that weekend.

7. In addition, judge schedules "also do not align with the schedule of any other actors in the criminal justice system... different prosecutors and public defenders handle matters at each stage of criminal proceedings and are not assigned to particular bail judges" (Dobbie et al., 2018, p. 209).

Taken together, these characteristics of the Miami-Date bail system result in a quasi-random allocation of bail judges to shifts and defendants to bail judges during the weekends.

	No. of Appointments		No. of Cases			
	mean	s.d	mean	s.d	Median	No. of Judges
Calendar Year:						
2009	1.20	0.45	240.67	90.34	254.00	55
2010	1.48	0.65	228.92	83.32	226.50	50
2011	1.15	0.41	184.93	51.63	193.50	54
2012	1.21	0.41	177.37	59.77	188.00	57
2013	1.19	0.39	181.39	66.86	185.50	54
2014	1.33	0.61	151.13	72.72	152.00	55
2015	1.25	0.48	136.45	43.65	139.00	51
2016	1.24	0.47	122.80	50.63	128.00	54

Table A1: Descriptive Statistics of Weekend Appointments and Number of Cases for The Weekend Bail Judges.

[4] This information about bail shift assignments was confirmed by the General Counsel of the Eleventh Judicial Circuit Court of Florida in email to the authors on January 16, 2024, in response to our Public Record Request. As a hypothetical example, in year 20XX, Judge AA is assigned to the first weekend of the year, Judge AB to the second weekend, and so on.

[5] A few are not assigned in a given year because there may be more judges than weekend shifts in the year and the first letter of their last name has not yet come up that year, or they only recently began their service as trial court judges (email on January 16, 2024, from the General Counsel to the authors.

B.2 Person identifier

The court records dataset included a person identifier variable ("id"). However, we observed two concerning patterns with this variable. First, we found a non-neglible percent of exact same name and date of birth combinations associated with different ids (9%). According to the data provider, this can occur by accident when other person fields (eye color, height, weight) do not match with the person's earlier record, leading the court to generate a new id. Second, we found several instances were the same id was associated with different personally identifiable information, such as birth dates and first and last names (12%). While some variation in names across individuals' cases is expected (aliases and legal name changes), birth date differences are not expected at this frequency. These differences could reflect typos in those fields but an accurate id, or they could signal a wrong id (the court is linking different people to the same id).

For these reasons, we generate a new person identifier, according to the following steps. Our goal is to reduce the first issue (instances where the identifier fails to link the same person to the same id). Due to uncertainties over the second issue (when the same identifier may refer to multiple individuals), we defer to the court id in those instances. First, we use the probabilistic record linkage method implemented by fastLink (Enamorado et al., 2019) to identify cases that are likely to involve the same defendant. We run fastLink twice, with two sets of parameters. In the first run, we split the sample by gender and search for matches within gender groups using the same parameters we use in our voter file merges: age difference within 0.33 years, first and last names within a Jaro-Winkler string similar distance of 0.94 or larger. In the second run, we repeat the merge, except we use Jaro string similarity measure with threshold 0.92. The difference between Jaro and Jaro-Winkler measures is that Jaro-Winkler gives more weight to the first-four characters of a string. The logic behind using both is that names and last names vary in the number of characters, and when comparing e.g., short vs. long first names, one may conclude based on Jaro-Winkler that Anna and Annabelle are similar (0.92) names but in fact they are not – the Jaro similarity is 0.86.

Second, we identify those observations whose id is sensitive to the string similarity method. We define an observation as sensitive to the method if the number of court ids associated with the new fastLink id changes from the Jaro-Winkler to the Jaro run. We refer to these as "edge cases" (3% of the total number of cases in the data). In these observations, we could code their person id based on the Jaro or Jaro-Winkler run. To make determinations, we leverage the number of characters in the first and last name, because string similarity is relatively inflated for shorter names (fewer opportunities for there to be a misspelling). We developed the following rules after a careful manual review (Christen, 2012). We use the id from the Jaro-Winkler run only if the observations are within 0.085 units (1 month) from each other in age and if they have very short names (5 or fewer characters) in the following patterns: JW1) in all first and last names, JW2) in all last names only, JW3) in all first names only, JW4) in all last names and in some first names, JW5) in some first names only, JW6) in some last names only, or JW7) in no first names and last names. Conversely, we use the id from the Jaro run if the observations are within 0.085 units apart in age when they have short names (5 or fewer characters) in the remaining patterns: J1) only some of the last names and some of the first names, and J2) all first names and some last names (see Table A2).

First Name Record 1	First Name Record 2	Last Name Record 1	Last Name Record 2	Rule
✓	✓	✓	✓	JW1
✓	✓	✓	×	J2
✓	✓	×	✓	J2
✓	✓	×	×	JW3
✓	×	✓	✓	JW4
✓	×	✓	×	J1
✓	×	×	✓	J1
✓	×	×	×	JW5
×	✓	✓	✓	JW2
×	✓	✓	×	J1
×	✓	×	✓	J1
×	✓	×	×	JW5
×	×	✓	✓	JW2
×	×	✓	×	JW6
×	×	×	✓	JW6
×	×	×	×	JW7

Table A2: Description of Rules for the use of the Jaro-Winkler or Jaro Runs. Note that for a pair of records, × represents a name component with more than 5 characters and ✓ represents the opposite.

Finally, we apply the following correction to all edge cases' final identifier: if we observe middle initial for everyone within that id group, the middle initials are different, and age is different, then we break up the pair (and where relevant, we re-pair observations in the id group that share the same middle initials and age). Otherwise, we keep the id intact.[6]

In the final step, we integrate our new fastLink-generated identifier with the original court id. We keep observations with the same original court id together, even if the fastLink id suggests they are different individuals. As mentioned above, we defer to the original court id due to uncertainty over whether differences in personally identifiable information within the same id reflects valid name changes, typos, or errors with the id. If at least one observation in a court id matches another

[6] We apply the middle name correction only to those with middle initial for everyone in the group to be conservative: if even just one of the observations in the id group is blank, it could be linked with any one of the others with the middle name filled in.

court id's fastLink id exactly, then we combine them under the same id. For our main specification, we use this generated identifier. As a robustness check, we also confirm that the main effects hold with the original court record id (see Table A8).

B.3 Data cleaning

B.3.1 Miami-Dade's Court Records

We obtained court records from the Office of the Miami-Dade County Clerk of the Courts from the 1990s until March 2021. The raw data is at the charge-arrest level, meaning at the time the defendant is first arrested in a case, a row is created for each charge in the case with the associated charge details such as statute, description, charge type, and charge degree. If the defendant is released and later re-arrested for a violation in the case, a new row is added to the dataset, containing a new jail number and the violation details but the same case number, first appearance bail hearing date and judge, and other details. If charges are otherwise added on later, new rows are created and the late addition charges are noted as such in the record. Each observation in the data also contains demographic information about the defendant: name, date of birth, race, gender.

We take several steps to construct our analysis dataset. First, we omit observations that reflect violations in the case because they are post-treatment and not outcomes of interest. Second, we use an auxiliary data table provided to us by the Clerk's office to link cases that involve the same incident but were transferred or consolidated to different case numbers.[7] Linking cases in this way ensures we are not double counting cases involving the same incident and defendant. We then code key variables in linked cases based on the full case history.[8] Third, we subset to our time period of interest when the natural experiment emerges: weekends between the 2008 and 2016 general elections (11/4/2008-11/8/2016). Specifically, we include cases in which the first arrest and first appearance bail hearing occurred on a weekend in this period.[9] This means we drop cases that a) secured release by posting the standard bond and did not have a first appearance bail hearing (approximately 40%) or b) had a first appearance bail hearing on a weekday. Fourth, we collapse the dataset to the defendant-first appearance. This is the relevant unit of analysis because at the first appearance bail hearing, a defendant can face multiple charges in multiple distinct cases.

[7] For example, if all felony charges in case F123 were later downgraded to misdemeanor charges, the defendant's case would be transferred to misdemeanor court and the defendant would receive a new case number, e.g. M456.

[8] For the first arrest, bail hearing and release dates, we select the earliest within linked cases. It is rare for there to be multiple arrests, bail hearings or release dates listed in a set of linked case. For the case outcome, we use the outcomes listed in the post-transfer or post-consolidation case number.

[9] However, if arrest date is missing but the case had a weekend first appearance bail hearing in the time period, we include it in the sample. We do not extend our analyses through the 2020 election due to changes in pretrial incarceration following the onset of Covid-19 and a decline in new cases.

The bail judge's decision is based on all such cases and all such charges. Following this step, our resulting dataset contains one observation per first appearance bail hearing for each defendant, with a summary of the offenses and outcomes across all charges and cases at the first appearance. For release date, we select the earliest across all charges and cases. For simplicity, we call this unit of analysis a "case."

Next, we identify and remove cases involving serious charges in which judges have less discretion. Including these cases would add noise to our measure of judge punitiveness.[10] Specifically, we drop cases involving a charge that meets the following criteria: it is listed in Florida statutes as grounds for either a) denying non-monetary release conditions or b) ordering pretrial incarceration, and more than 85% of the cases we observe in our sample with that charge result in pretrial incarceration for more than 3 days.[11] The charges and defendants that meet these criteria include: kidnapping, homicide, sexual activity with a child by or at solicitation of person in familial or custodial authority, armed burglary, DUI manslaughter with a prior DUI manslaughter or suspended license conviction, sexual battery, armed robbery, home invasion, other offenses which are punishable by the death penalty or life in prison, and defendants who may have been designated as a "three-time violent felony offender" or a "violent career criminal" according to Florida statutes.[12]

Finally, we focus on defendants last case before each general election,[13] and we omit a small number of weekend cases that fall into the following additional categories: a) the defendant's race was identified as Asian or not identified in their court records at all (n=226); b) the case record lacks the bail judge's name, which is necessary for the instrumental variables design (n=684); c) the case was associated with multiple bail judges (n = 1558)[14]; d) the release code in the case indicated the defendant was released to U.S. immigration enforcement, indicating that the defendant was not a U.S. citizen and thus not eligible to vote (n = 672); e) the defendant was younger than 18 at the time of their case (n = 137); f) the case was assigned to a bail judge who saw no more than 25 cases on any day in which they appear in the data, suggesting that the judge served as a temporary,

[10]We do not remove cases that consistently result in release due to low judge discretion. Statutes do not identify charges that should not result in pretrial incarceration and comprehensively identifying such case types was prohibitive given the raw data received.

[11]There are other factors in the statutes which constrain judge discretion but that we do not observe well: previous violations of release conditions, serious convictions in other jurisdictions, and being on probation or parole or having pending an open case involving a serious offense at the time of the focal arrest.

[12]In robustness checks, we use higher thresholds as grounds for removal: PI in 90% and 95% of cases involving the charge. The charges that meet the 95% threshold includes kidnapping, offenses punishable by life or death penalty, and the aforementioned DUI manslaughter cases, whereas the 90% threshold includes cases with these charges, plus homicide, sexual activity with a child by or at solicitation of person in familial or custodial authority, and armed burglary.

[13]Some defendants have weekend cases in both 2008-2012 and 2012-2016; they would appear twice.

[14]We assume these are errors in data entry and remove them to reduce further measurement error in judge leniency.

idiosyncratic replacement (n=123); g) the case was the only one of violent charge s assigned to bail judge j in year t, as there is insufficient data to construct the leave-out judge punitiveness instrument for these cases (n = 3796).

B.3.2 Felony disenfranchisement

Following the construction of the instrument, we remove cases in which the defendant is likely already disenfranchised due to a prior felony conviction. During our observation period in Florida, people convicted of felonies typically lost voting rights permanently unless the state's Clemency Board restored them. Between 2007-2011, rights were restored to approximately 150,000 Floridians. For less serious felony convictions, this happened automatically upon completion of a sentence (if no restitution or charges were pending), but for more serious convictions, such as murder, sexual battery or sexual predation ("level 3"), restoration was much less likely (Florida Parole Commission Annual Report 2006-2007). After 2011, restorations for all types of convictions dropped substantially: fewer than 3,000 individuals regained voting rights in Florida between 2012-2018 (Morris, 2021). Thus, we consider a defendant to be likely disenfranchised if, at any point prior to the focal case, the defendant was convicted of what the Clemency Board defined as a "level 3" felony or if the defendant was convicted of any felony after 2011.[15]

B.3.3 Charge categories, statute-based defendant designations, and violent charge

We construct indicators for various charge categories. The raw data provides a short description of each arrest charge in a case, and we match these descriptions in the data to broader charge categories using regular expressions. We construct three additional broad charge categories, following Dobbie et al. (2018) (any charges involving either drugs, weapons, or property), and we code violent charge as 1 if there was a violent charge at the time of arrest and 0 otherwise. We define violent charges to include: homicide, armed robbery, armed burglary, assault (including aggravated, sexual, simple), battery, rape, manslaughter, domestic violence violations, human trafficking, kidnapping.

[15] This definition may over-state disenfranchisement if those we code as likely disenfranchised in Florida moved and re-gained voting rights in other states. However, we expect that the magnitude of under-counting is likely greater: we include everyone previously convicted of a non 3 felony prior to 2011 to account for the chance that they regained their voting rights, which is optimistic. As of 2010, voting rights had only been restored to 36% of those released in the prior two decades after serving felony sentences (Uggen et al., 2012). Nonetheless, the inclusion of defendants who are already disenfranchised is unlikely to bias the effect in a particular direction; the instrument (judge punitiveness) is not correlated with pretreatment covariates (including having a prior felony conviction).

B.4 Construction of Relevant Covariates

B.4.1 Pretrial incarceration

We code pretrial incarceration as 1 if the time between the first hearing and release is greater than 3 days, and 0 otherwise. In robustness checks, as suggested by Marshall (2016), we use a continuous measure of the treatment: the logged number of days detained pretrial. If the case record lacks a release date, we use the case disposition date as the release date if all charges have reached a disposition.[16] If the case lacks both release and case disposition dates, we assume the defendant is still detained at the time the dataset was provided to us.

B.4.2 Instrument Calculation

Following Aizer and Doyle (2015), Dobbie et al. (2018), and McDonough et al. (2022), our instrument represents the judge's punitiveness net of the focal defendant. Formally:

$$Z_{dtjh} = \frac{(\sum_{k=0}^{N_{tjh}} \sum_{c=1}^{N_{dtjh}} P_{kctjh}) - \sum_{c=1}^{N_{dtjh}} P_{dctjh}}{N_{tjh} - N_{dtjh}} \tag{1}$$

where N_{tjh} is the number of cases assigned to judge j at year t and a proxy for case severity as measured by $h \in \{0 = \text{non-violent crime}, 1 = \text{violent crime}\}$, N_{dtjh} is the number of cases where defendant d was involved and assigned to judge j at year t and case severity h, and $P_{dctjh} \in \{0 = \text{released}, 1 = \text{detained}\}$ represents the pretrial decision made by judge j in case c for defendant d at year t and case severity h.

B.4.3 Race, gender, and age

Court records identify defendants as White, Black, Asian, or unknown/unreported. Only a very small number of defendants are identified as Asian or unknown/unreported (n $<$ 300); we remove these defendants from the sample. We use the method in Xie (2022) to predict the probability a defendant is Hispanic based on their first and last name, as implemented by the `rethnicity` package in R. The algorithm behind `rethnicity` was trained on Florida voting records. If a defendant is coded as Black in the court record but is predicted to be Hispanic based on name, we code them as Black. For a very small number of cases (n=6) with missing gender, we predict gender based on first name using the R package `gender` which draws on several historical data sources, including from the U.S. Social Security Administration and the U.S. Census (Blevins and Mullen, 2015). We define age as of the first appearance bail hearing (hearing date - date of birth).

[16] Specifically, we use the latest disposition date in all charges associated with the case record.

B.4.4 Bail judge data

In the court records, we observe the first and last name of the first appearance bail judge. We format these data fields to correct occasional discrepancies in judge names (e.g. the inclusion of a middle initial, hyphenation or no hyphenation in last names, etc.) This ensures that we do not treat misspelled judge names as separate judges. Crystal Yang generously provided us with the judge race and gender data they collected for their study of Miami-Dade County from 2006-2014 using the court directory and conversations with court staff. There are 61 judges in our sample that are not in their data. For these remaining judges, we used similar methods: we coded judge race and gender using the judicial directory on the court's website.[17] If the judge did not appear in the directory, we coded race and gender based on online news articles and/or the judge's voter registration record in Miami-Dade County if applicable.[18]

B.4.5 Incapacitation

We construct two measures to assess the role of incapacitation (incarceration on election day). In our first measure, we define incapacitation as likely incarceration on election day, either because the defendant was detained pretrial in the focal case, or because the defendant was serving a post-conviction sentence in the focal case. Specifically, incapacitation equals 1 if the defendant received a minimum sentence of 1 day or more before the election, and the estimated sentence release is after Election Day.[19] We deduct from the minimum sentence length the number of days the defendant was detained pretrial following their first appearance bail hearing, reflecting a common practice to provide credit for time served pretrial towards a post-conviction sentence (Stevenson, 2018).[20] Incapacitation also equals 1 if the defendant's estimated pretrial release date after the first appearance bail hearing is past the election. This first measure has the following limitations. In the raw data, we observe only defendants' first pretrial incarceration spell. Thus, if the defendant was released pretrial in the focal case and re-arrested and detained until after the election for violating conditions of release, we do not observe that as incapacitation. Additionally, our measure of incapacitation due to sentencing makes several assumptions as referenced above (e.g. credit for time served, concurrent sentences, no other early release) and does not account for post-conviction incarceration triggered by parole or probation violations. Our second measure identifies case types

[17] https://www.jud11.flcourts.org/About-the-Court/Judges/Judicial-Directory

[18] We only coded judge race and gender based on a voter record if we found only one match based on first name, middle initial and last name, or multiple matches on these fields and all had the same race in the records.

[19] If defendants received multiple sentences in the case (for multiple conviction charges), we use the longest sentence. This effectively assumes that sentences are served concurrently.

[20] We are not able to adjust for time served pretrial that was not following the initial bail hearing, as we do not reliably observe it.

that rarely result in a post-conviction incarceration sentence. To identify these, we first focus on cases with single charges that resulted in conviction. For each charge in this sample, we calculate the proportion of cases involving that charge that resulted in an incarceration sentence greater than 0 days. We then identify those charges where sentences occurred in less than 5% of the cases ("low probability of sentence"). In our main analysis sample, we consider a case to have a low probability of incapacitation from post-conviction sentencing if all (arrest) charges in the case were ones in the low probability of sentence group, we code this as 1. All other cases are coded as 0 i.e., they cannot be qualified as low-probability incapacitation.

B.4.6 Conviction

We construct conviction based on the disposition code included in the data.[21][22] Following Dobbie et al. (2018) (SI, 25), disposition codes that indicate diversion, deferred prosecution, or judgement was withheld are coded as 0s, since these outcomes do not formally count as a conviction or trigger the full set collateral consequences.[23] About 30% of the sample has a conviction.

B.4.7 Prior cases and convictions

We construct several measures of prior experience with the criminal legal system. For each case in our main sample, we look to the full raw dataset to construct any prior case, number of prior convictions, number of prior felony convictions, number of prior felony convictions after 2011, number of prior felony convictions for murder, sexual battery or sexual predation at any time, any prior conviction for DUI manslaughter or suspended license, number of prior convictions for a charge listed in the statutory definitions of "habitual violent offender," "three-time violent felony offender," and "violent career criminal" respectively.[24] Except where noted, we re-code these measures as binary indicators (1 if any prior, 0 otherwise). We define a case as prior to the focal case if the case's first arrest date predated the arrest date in the focal case, whereas we define a conviction as prior to the focal case if it had a disposition date before the arrest date in the focal case and it met the definition of a conviction (see definition above). Due to improvements in data

[21]'Conviction' takes 1 if the disposition code indicates conviction and 0 otherwise. Thus, both the presence of a non-conviction disposition code (e.g. not guilty, dismissed) and the absence of any disposition code (which could indicate transfer, dismissal or pending disposition) are coded the same (as 0s).

[22]Because we will be focusing on cases at least 5 years from the time of the data export, concerns about right censoring (not observing case disposition in more recent cases) are less acute.

[23]To be sure, these dispositions are not the same a finding of innocence or a case dismissal. These dispositions are often accompanied by higher fines and required actions and/or surveillance. They can also cause harsher sentences in future cases and collateral consequences (e.g. some employers require disclosure of criminal cases that resulted in withheld adjudication in addition to conviction).

[24]We use the id we generate for our main specifications and the original id provided in the court records for the robustness check. For details on person id and associated robustness check, see Appendix B.2

collection, we expect these measures are more representative of system involvement in the decade closest to the observation period and due to record sealing and expungement practices, we expect they are most reliable for convictions, particularly for felony convictions. We also note that these measures fail to capture system involvement outside of Miami-Dade County.

B.4.8 Address and address-based income measures

For each case in our analysis sample, we merge in the defendant's closest pretreatment address record from a supplemental file obtained from the Clerk's office. We use zip code to obtain a proxy measure of defendants' income pretreatment: median income in the defendants' zip code in that year from the IRS Statistics of Income. We then categorize defendants as above median, below median income, or unknown (if no address record). Median income is defined based on the sample distribution, which ranges from approximately $28-32,000 depending on the year. If the defendant's pretreatment address record indicates they were homeless, we code them as below median income.

C Merges with Voter Files

We merged the court records from Miami-Dade County with voter files as follows. To classify pairs of records as matches or non-matches, we rely on the Fellegi-Sunter model of probabilistic record linkage as implemented in fastLink (Enamorado et al., 2019). More specifically, we say that a record a in our court data is a potential match of a record b in the voter file if the estimated match probability is the largest among all pairs that involve record a. This procedure yields a one-to-one match. The merge process is as follows:

First, we merge each Florida voter file (2009, 2013, 2017) with our Miami-Dade court data using first, middle, and last name, gender, and date of birth.[25] To make comparisons across our linkage fields, we selected three levels of agreement (different, similar, identical or almost identical) for first name and last name and we used the common Jaro-Winkler measure of string similarity with the thresholds 0.85 and 0.94. For age, we again use three levels of agreement and use the absolute value of the difference (L1 norm) with the thresholds set at 3 months and 6 months of difference. In the case of middle name and gender, we made comparisons based on whether they had an identical value or not. Based on these comparisons, we estimate the probability of being a match for each pair of records.

Second, for defendants not found in the 2013 and 2017 Florida voter files, we merged the unmatched court records with the 2014 and 2017 voter files for all remaining states and D.C using

[25]We convert date of birth to exact age as of Election Day 2016, which avoids comparison based on integers and it is equivalent to counting the number of days between two dates.

the same variables listed in the first step. For these merges, for computational resource reasons, we first use binary comparison based on exact match. After obtaining matches, we calculate the corresponding match probabilities using fastLink. If a defendant was matched to voting records in multiple states, we pick the record the highest match probability.

Given that our merge is based only on a few fields, we adjust merge probabilities by the frequency of the first and last name. Of the sample whose first name in the court records meets the criteria for full or partial agreement with the first name in the voter file, we calculate the relative frequency that the first and last name is common among the set of matches compared to the set of non-matches (see Enamorado et al. (2019) for more details).

Out of our final sample of 45,107 cases, we matched 58% of the records (12671 Black defendants, 9818 Hispanic, 3836 White defendants), of which 2082 matches came from the nationwide voter files and the remainder from the merge with the 2009, 2013 and 2017 Florida voter files. For defendants we do not find in any of the voter files, we assume they were not registered and did not vote. As a robustness check, we instead use a deterministic approach to merge the court records and voter files. We only count as a match those with exactly the same gender, and first name, last name, and age within the agreement threshold (no partial agreements).

D Additional Results

Below we present the additional results mentioned in the main text of the paper. In particular:

- To illustrate the relationships of interest, Figure A1 displays the non-parametric fit between the residualized instrument and residualized pretrial incarceration (left panel) as well as residualized turnout (right panel). By residualizing, we mean removing the variation attributed to fixed effects.

- Figure A2 shows the relationship between residualized judge punitiveness instrument and Predicted Turnout. We find that these measures are not correlated (r = 0.002).

- Figure A3 shows that the distribution of residualized judge punitiveness is almost identical across the combinations of defendant and judge race.

- Table A3 presents the descriptive statistics of case- and defendant-level covariates for the full sample, for defendants detained pretrial for more than 3 days, and for defendants released in 0-3 days.

- Table A4 presents the descriptive statistics of case- and defendant-level covariates for the full sample of weekend cases and the full sample of weekday cases.
- Table A5 presents the estimated effect of pretrial incarceration on turnout using OLS regression. However, OLS estimates may be biased by the correlation between unobserved defendant characteristics and pretrial incarceration.
- If assignment of bail judges is as-if random, case and defendant characteristics should be distributed evenly across judges with different decision tendencies and should not predict the instrument. The first column of Table A6 examines whether such characteristics are significant predictors of PI, while the second column tests whether such characteristics are significant predictors of our instrument.
- The first row of Table A7 presents the first stage results for the full sample (main finding), and the rest of Table A7 presents the first stage results for subsets (gender, defendants charged with different offense types). In all analyses our instrument has a strong positive correlation with pretrial incarceration, and the F-statistic is large. Thus, our 2SLS estimates are unlikely to suffer from weak instrument bias.
- Table A8 presents a series of checks supporting the robustness of our main finding.
- Figure A4 presents the relationship between residualized judge punitiveness instrument and predicted turnout, using two versions of the instrument: the main version (using a binary measure of PI), and a second version (using a continuous measure of PI).
- Table A9 presents our estimates of the effect of PI on turnout by prior case status, by prior turnout, and by prior turnout and race.
- Table A10 (Panel A) presents the effects of pretrial incarceration on turnout after excluding from our analyses cases that are more likely to be incapacitated due to the proximity of the arrest to election day. Panel B presents the effect of pretrial incarceration on turnout for the set of cases that have an offense that rarely results in a post-conviction incarceration sentence. Finally, Panel C excludes those who are likely incapacitated either due to pretrial incarceration or a post-conviction sentence.
- Table A11 presents the test of difference in means across race of the judge for all defendants and Black defendants, respectively. As discussed in the main text, there are no discernible

differences in the characteristics of the defendants and cases to which different judges get assigned.

- Table A12 presents results for the relationship between judge punitiveness and pretrial incarceration and between pretrial incarceration and turnout, respectively. Column 1 controls for judge punitiveness but not judge race, column 2 controls for judge race but not judge punitiveness, and column 3 controls for both judge punitiveness and judge race. We find that White and Hispanic judges are not different from Black judges when predicting PI and turnout, respectively.

- Table A13 presents 2SLS results that assess heterogeneity in the effect by race of the defendant and race of the judge.

- Table A14 presents the test of difference in means across judge experience for all defendants and Black defendants, respectively.

- Finally, Table A15 presents the effect of pretrial incarceration on turnout by defendant race and judge experience.

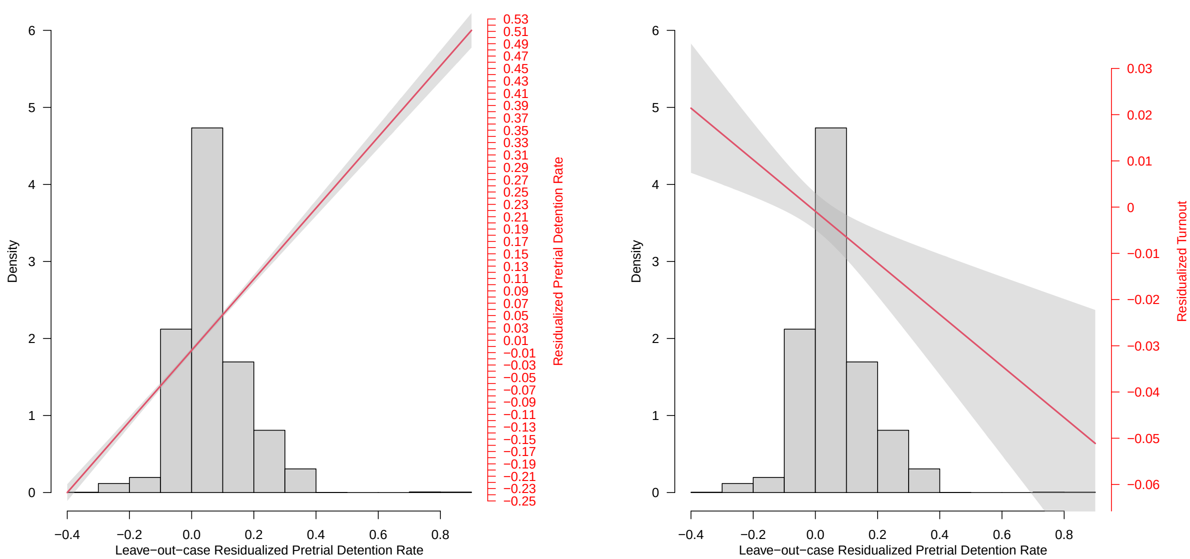

Figure A1: Distribution of our Residualized Judge Punitiveness Instrument against Residualized Pretrial Incarceration (left) and Residualized Turnout (right). Residualizing partials out the variation from the fixed effects.

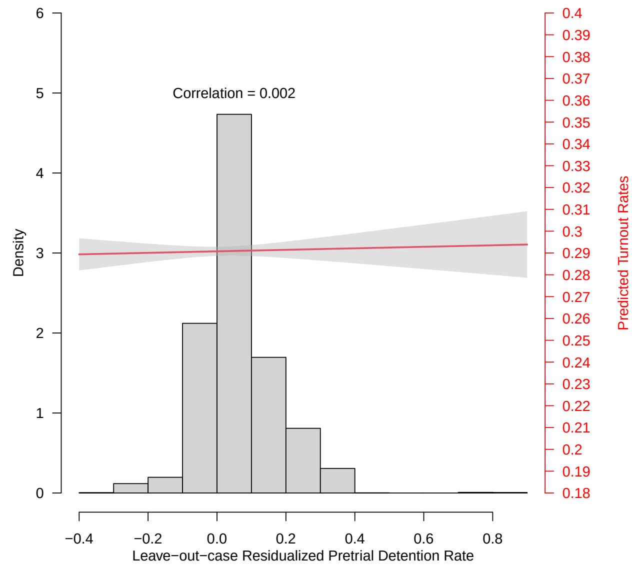

Figure A2: Relationship Between Residualized Judge Punitiveness Instrument and Predicted Turnout. Predicted turnout is based exclusively on demographic and case-level covariates. The flat line indicates no meaningful correlation between these measures (correlation: 0.002).

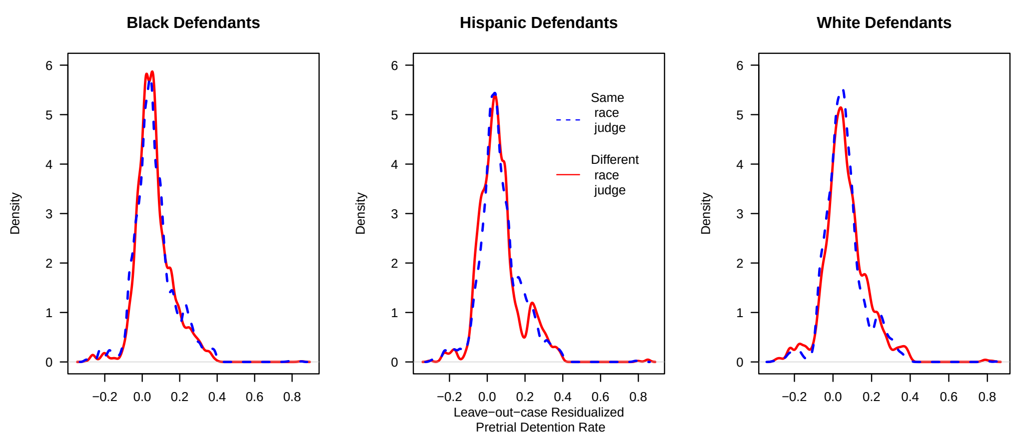

Figure A3: Distribution of the (residualized) Judge Punitiveness Across Race of the Defendant × Race of the Judge

	Full Sample		Detained		Released	
	mean	s.d	mean	s.d.	mean	s.d.
Pretrial incarceration:						
Detained > 3 days	0.23	0.42	1.00		0.00	
Detained 1 year	0.01	0.10	0.04	0.21	0.00	
Total days detained	20.82	100.46	87.11	193.02	0.48	0.88
Demographic:						
Age (years)	35.78	13.04	35.98	12.81	35.71	13.11
Female	0.19	0.40	0.15	0.35	0.21	0.41
Race:						
Black	0.47	0.50	0.51	0.50	0.46	0.50
White	0.15	0.36	0.13	0.34	0.16	0.36
Hispanic	0.38	0.49	0.35	0.48	0.39	0.49
Zip code average income:						
Below Median	0.37	0.48	0.42	0.49	0.35	0.48
Above Median	0.32	0.47	0.37	0.48	0.30	0.46
Unavailable	0.31	0.46	0.20	0.40	0.35	0.48
Case-related:						
Any drug offense	0.25	0.43	0.36	0.48	0.21	0.41
Any firearm offense	0.03	0.18	0.05	0.22	0.03	0.17
Any property offense	0.00	0.05	0.01	0.07	0.00	0.04
Any prior case	0.71	0.45	0.85	0.36	0.67	0.47
First bail amount ($)	9,502	668,919	26,305	980,514	4,348	538,254
Electoral:						
Pretreatment Turnout	0.28	0.43	0.27	0.42	0.29	0.43
Post-treatment Turnout	0.29	0.43	0.27	0.42	0.30	0.43
Voting-age-ineligible	0.07	0.25	0.06	0.25	0.07	0.26
Pretreatment registration	0.58	0.45	0.56	0.45	0.58	0.45
N	45,107		10,588		34,519	

Table A3: Descriptive Statistics. for the full sample, for defendants detained pretrial for more than 3 days, and for defendants released in 0-3 days. Proportions unless noted. “Any property offense” includes motor vehicle theft, burglary, shoplifting, robbery and other theft charges. “Pretreatment registration” is an indicator of whether or not a defendant was registered to vote before their bail hearing. “Voting-age-ineligible” is an indicator of whether or not a defendant was younger than 18 on the day of the pretreatment general election.

	Full Sample: Weekend		Full Sample: Weekday	
	mean	s.d	mean	s.d.
Pretrial incarceration:				
Detained > 3 days	0.23	0.42	0.25	0.44
Detained 1 year	0.01	0.10	0.03	0.16
Total days detained	20.82	100.46	35.52	146.5
Demographic:				
Age (years)	35.78	13.04	35.45	12.85
Female	0.19	0.40	0.18	0.39
Race:				
Black	0.47	0.50	0.46	0.50
White	0.15	0.36	0.14	0.35
Hispanic	0.38	0.49	0.40	0.49
Case-related:				
Any drug offense	0.25	0.43	0.27	0.44
Any firearm offense	0.03	0.18	0.05	0.22
Any property offense	0.00	0.05	0.02	0.12
Any prior case	0.71	0.45	0.69	0.46
First bail amount ($)	9,502	668,919	6,851	275,756
N	45,107		108,528	

Table A4: Descriptive Statistics: Weekend vs Weekday Cases. for the full sample. Proportions unless noted. "Any property offense" includes motor vehicle theft, burglary, shoplifting, robbery and other theft charges.

	OLS Estimates		
	(1)	(2)	(3)
A. Main Result			
Pretrial Incarceration	-0.03	-0.02	-0.01
	(0.00)	(0.00)	(0.00)
B. Pretrial Incarceration × Race			
Pretrial Incarceration (baseline: Black Defendant)	-0.04	-0.02	-0.02
	(0.01)	(0.01)	(0.01)
Pretrial Incarceration × Hispanic	0.03	0.01	0.02
	(0.01)	(0.01)	(0.01)
Pretrial Incarceration × White	0.03	0.02	0.02
	(0.01)	(0.01)	(0.01)
Fixed Effects	✓	✓	✓
Demographic covariates		✓	✓
Case covariates			✓
N	45107	45107	45107

Table A5: The Effect of Pretrial Incarceration on Turnout. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Fixed effects: bail hearing year, month, day-of-the-week, and violent charge. Demographic covariates: age, age squared, gender, race, pretreatment turnout (previous election), voting-age-ineligible, and pretreatment registration. Case covariates: any drug, firearm, and property offense, and prior case status. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

	Randomization Test	
	Pretrial Incarceration	Judge Punitiveness
Demographic:		
Age	0.00293	0.00001
	(0.00077)	(0.00019)
Age^2	-0.00004	0.00000
	(0.00001)	(0.00000)
Female	-0.04824	-0.00481
	(0.00352)	(0.00083)
Race:		
White	-0.01349	-0.00043
	(0.00404)	(0.00113)
Hispanic	-0.01340	-0.00236
	(0.00329)	(0.00083)
Case-related:		
Any drug offense	0.15691	0.00701
	(0.00421)	(0.00075)
Any property offense	0.11601	0.00991
	(0.00374)	(0.00085)
Any firearm offense	0.15147	0.01749
	(0.00914)	(0.00260)
Any prior case	0.13329	0.00996
	(0.00321)	(0.00100)
Electoral:		
Pretreatment turnout	-0.01042	-0.00208
	(0.00370)	(0.00097)
Voting-age-ineligible in the Prior Election	0.00818	0.00272
	(0.00678)	(0.00170)
Pretreatment registration	-0.02551	-0.00136
	(0.00369)	(0.00089)
Joint F-test	219.84	18.56
Fixed Effects	✓	✓
N	45107	45107

Table A6: Randomization Test. The estimates are obtained from linear regression. The F-test of joint significance is for all the covariates listed above ($p < 0.001$ for column 1 and 2). Fixed effects, demographic, and case covariates are as described in Table A5. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

		First Stage Estimates		
		(1)	(2)	(3)
Complete Sample	**Judge Punitiveness**	0.80	0.79	0.74
		(0.01)	(0.01)	(0.01)
	First Stage F-stat	2878.31	2839.52	2600.63
	N	45107	45107	45107
Demographic Subset:				
Black Defendant	**Judge Punitiveness**	0.88	0.87	0.83
		(0.02)	(0.02)	(0.02)
	First Stage F-stat	1477.16	1435.49	1365.62
	N	21199	21199	21199
White Defendant	**Judge Punitiveness**	0.75	0.75	0.70
		(0.03)	(0.03)	(0.03)
	First Stage F-stat	411.86	410.98	367.56
	N	6831	6831	6831
Hispanic Defendant	**Judge Punitiveness**	0.71	0.71	0.65
	Std. Error	(0.02)	(0.02)	(0.02)
	First Stage F-stat	963.86	961.00	848.68
	N	17077	17077	17077
Male	**Judge Punitiveness**	0.84	0.83	0.78
		(0.01)	(0.01)	(0.01)
	First Stage F-stat	2535.49	2515.46	2301.32
	N	36314	36314	36314
Female	**Judge Punitiveness**	0.61	0.60	0.56

Table continues on the next page

		First Stage Estimates		
		(1)	(2)	(3)
		(0.02)	(0.02)	(0.02)
	First Stage F-stat	363.81	359.06	333.61
	N	8793	8793	8793
CASE-RELATED SUBSET:				
Any Prior Case	**Judge Punitiveness**	0.94	0.93	0.90
		(0.01)	(0.01)	(0.01)
	First Stage F-stat	2562.46	2534.42	2365.98
	N	32131	32131	32131
No Prior Case	**Judge Punitiveness**	0.45	0.44	0.43
		(0.02)	(0.02)	(0.02)
	First Stage F-stat	381.34	374.01	367.51
	N	12976	12976	12976
Any Drug Offense	**Judge Punitiveness**	0.78	0.74	0.70
		(0.04)	(0.04)	(0.04)
	First Stage F-stat	196.78	178.44	162.66
	N	11102	11102	11102
No Drug related offense	**Judge Punitiveness**	0.80	0.79	0.75
		(0.01)	(0.01)	(0.01)
	First Stage F-stat	2730.22	2696.52	2517.62
	N	34005	34005	34005
Any Property offense	**Judge Punitiveness**	0.87	0.86	0.84
		(0.03)	(0.03)	(0.03)
	First Stage F-stat	572.44	563.32	542.03
	N	13067	13067	13067
No property Offense	**Judge Punitiveness**	0.77	0.76	0.72
		(0.01)	(0.01)	(0.01)
	First Stage F-stat	2241.41	2203.86	2063.91
	N	32040	32040	32040
Any Weapon	**Judge Punitiveness**	1.19	1.16	1.13
		(0.05)	(0.05)	(0.05)
	First Stage F-stat	316.46	298.55	274.53
	N	1527	1527	1527
No weapon	**Judge Punitiveness**	0.77	0.76	0.72
		(0.01)	(0.01)	(0.01)
	First Stage F-stat	2517.30	2484.46	2304.82
	N	43580	43580	43580
Any violence	**Judge Punitiveness**	0.89	0.88	0.83
		(0.01)	(0.01)	(0.01)
	First Stage F-stat	2874.36	2844.86	2765.61
	N	10696	10696	10696
No violence	**Judge Punitiveness**	0.59	0.59	0.53
		(0.02)	(0.02)	(0.02)
	First Stage F-stat	387.46	380.92	318.74
	N	34411	34411	34411
	Fixed Effects	✓	✓	✓
	Demographic covariates		✓	✓
	Case covariates			✓

Table A7: FIRST-STAGE: THE EFFECT OF JUDGE PUNITIVENESS ON PRETRIAL INCARCERATION BY SUBGROUPS. Fixed effects, demographic, and case covariates are as described in Table A5. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
1. *Including Outliers*:			
Pretrial Incarceration (Baseline: non-outliers)	-0.17	-0.14	-0.14
	(0.04)	(0.03)	(0.03)
First Stage F-stat	598.59	587.23	539.71
Pretrial Incarceration × Outlier	0.17	0.13	0.14
	(0.04)	(0.04)	(0.04)
First Stage F-stat	353.77	352.68	348.75
N	49035	49035	49035
2. *Different Cutpoints for Pretrial Incarceration*:			
Pretrial Incarceration (7+ days)	-0.10	-0.08	-0.08
	(0.02)	(0.02)	(0.02)
First Stage F-stat	2060.49	2027.89	1830.50
N	45107	45107	45107
Pretrial Incarceration (14+ days)	-0.12	-0.09	-0.09
	(0.03)	(0.02)	(0.02)
First Stage F-stat	1460.20	1436.46	1288.75
N	45107	45107	45107
Pretrial Incar. (0 if <3; 1 if in [3, 21]; 2 if >21 days)	-0.06	-0.05	-0.04

Table continues on the next page

	Second Stage Estimates		
	(1)	(2)	(3)
	(0.01)	(0.01)	(0.01)
First Stage F-stat	2066.95	2046.37	1869.47
N	45107	45107	45107
Pretrial Incarceration (Log Number of days)	-0.03	-0.02	-0.02
	(0.01)	(0.01)	(0.01)
First Stage F-stat	1880.66	1859.01	1684.34
N	45107	45107	45107
3. *Residualized Instrument*:			
Pretrial Incarceration	-0.09	-0.07	-0.07
	(0.02)	(0.02)	(0.02)
First Stage F-stat	2875.69	2837.43	2596.80
N	45107	45107	45107
4. *Deterministic Merge*:			
Pretrial Incarceration	-0.10	-0.09	-0.09
	(0.02)	(0.01)	(0.02)
First Stage F-stat	2878.31	2842.20	2604.36
N	45107	45107	45107
5. *Bivariate Probit*:			
Pretrial Incarceration			-0.06
			(0.01)
6. *Miami-Dade Court Record Person Identifier*:			
Pretrial Incarceration	-0.08	-0.07	-0.07
	(0.02)	(0.02)	(0.02)
First Stage F-stat	3133.32	3087.59	2830.64
N	45445	45445	45445
7. *Additional Covariates Included*:			
Pretrial Incarceration	-0.09	-0.07	-0.08
	(0.02)	(0.02)	(0.02)
First Stage F-stat	2878.31	2839.52	2077.04
N	45107	45107	45107
8. *Placebo Test: Predicting 2008 Turnout*:			
Pretrial Incarceration	-0.02	-0.01	-0.01
	(0.03)	(0.02)	(0.02)
First Stage F-stat	2454.89	2422.45	2271.79
N	39165	39165	39165
9. *Bootstrap Clustered Std Errors at the Judge-Level*:			
Pretrial Incarceration	-0.09	-0.07	-0.08
	(0.02)	(0.02)	(0.02)
N	45107	45107	45107
10. *Heteroskedasticity-consistent Std Errors*:			
Pretrial Incarceration	-0.09	-0.07	-0.08
	(0.02)	(0.02)	(0.02)
N	45107	45107	45107
Fixed Effects	✓	✓	✓
Demographic covariates		✓	✓
Case covariates			✓

Table A8: ROBUSTNESS CHECKS. Fixed effects, demographic, and case covariates are as described in Table A5. For bivariate probit (biprobit), we use convert the continuous measures of turnout and registration (weighted by matched probability) into binary, with a 0.8 threshold (e.g. 1 if pretreatment turnout is greater than 0.8, 0 otherwise). The biprobit estimate in this table reflects the average difference in predicted probabilities when moving pretrial incarceration from 0 to 1, holding all else constant. Outliers in terms of judge punitiveness are flagged using the inter-quartile definition of an outlier. Additional covariates include felony charge and any prior conviction. For the placebo test, the sample includes defendants age 18 and older at the time of the 2008 election and the outcome is turnout in 2008. Unless otherwise noted, bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses. Specification 9 presents bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge level are presented in parentheses. Specification 10 presents heteroskedasticity-consistent Std Errors as suggested by Abadie et al. (2023).

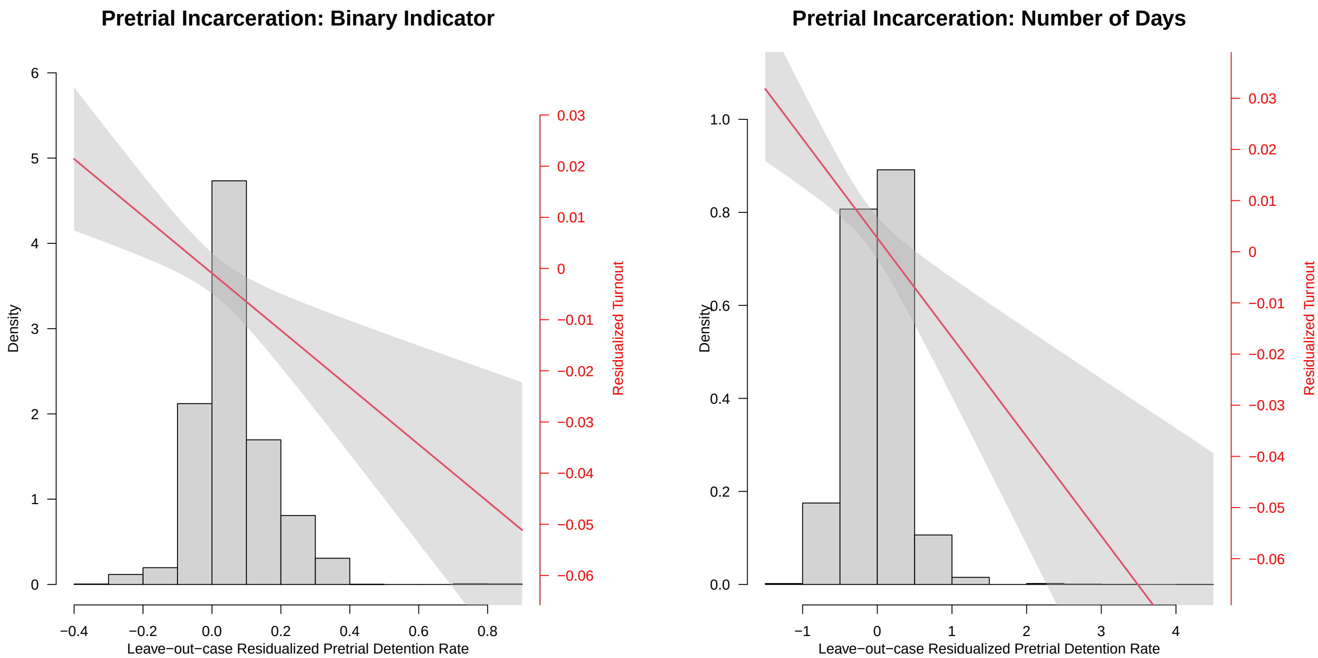

Figure A4: Relationship Between Residualized Judge Punitiveness Instrument and Predicted Turnout. The main (binary) instrument (left) is based on pretrial incarceration coded as 1 if detained for more than 3 days and 0 otherwise. The continuous instrument (right) is based on the logarithm of the number of days detained pretrial. Predicted turnout is based exclusively on demographic and case-level covariates. The gradient of both lines shows that the relationship we are measuring is similar whether we use the binary or the continuous version of the instrument.

	Second Stage Estimates		
	(1)	(2)	(3)
A. By Prior Turnout:			
Pretrial Incarceration (Baseline: Non Prior Turnout)	-0.04	-0.04	-0.03
	(0.02)	(0.02)	(0.02)
F-stat	1443.00	1424.80	1304.04
Pretrial Incarceration × Prior Turnout	-0.12	-0.13	-0.13
	(0.04)	(0.04)	(0.04)
F-stat	616.43	620.19	649.28
N	45107	45107	45107
B. By Prior Turnout and Defendant's race:			
Pretrial Incarceration (baseline: Black Defendant, No prior voter)	-0.06	-0.05	-0.05
	(0.02)	(0.02)	(0.02)
First Stage F-stat	480.28	475.59	436.64
Pretrial Incarceration × Hispanic	0.01	0.00	0.00
	(0.03)	(0.03)	(0.03)
First Stage F-stat	327.22	327.48	330.84
Pretrial Incarceration × White non-Hispanic	0.11	0.07	0.07
	(0.05)	(0.04)	(0.04)
First Stage F-stat	112.70	110.29	109.51
Pretrial Incarceration × Prior Voter	-0.11	-0.13	-0.13
	(0.05)	(0.05)	(0.05)
First Stage F-stat	221.07	211.24	217.01
Pretrial Incarceration × Hispanic Defendant × Prior Voter	0.02	0.03	0.03
	(0.08)	(0.07)	(0.07)
First Stage F-stat	93.52	90.94	93.02
Pretrial Incarceration × White non-Hispanic × Prior Voter	-0.05	-0.01	-0.00
	(0.13)	(0.12)	(0.12)
First Stage F-stat	23.99	23.50	22.85
Fixed Effects	✓	✓	✓
Demographic covariates		✓	✓
Case covariates			✓
N	45107	45107	45107

Table A9: Second-Stage: The Effect of Pretrial Incarceration on Turnout by Prior Turnout, and By Prior Turnout and Race. Fixed effects, demographic, and case covariates are as described in Table A5. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
A: Excluding Cases:			
2 Months From Election Day:			
Pretrial Incarceration	-0.09	-0.06	-0.06
	(0.02)	(0.02)	(0.02)
First Stage F-stat	2616.78	2582.81	2354.28
N	43188	43188	43188
4 Months From Election Day:			
Pretrial Incarceration	-0.10	-0.07	-0.07
	(0.02)	(0.02)	(0.02)
First Stage F-stat	2474.63	2440.97	2233.70
N	41289	41289	41289
6 Months From Election Day:			
Pretrial Incarceration	-0.09	-0.07	-0.07
	(0.02)	(0.02)	(0.02)
First Stage F-stat	2384.84	2352.14	2154.83
N	39475	39475	39475
B: Excluding the Incapacitated:			
Pretrial Incarceration	-0.09	-0.07	-0.07
	(0.02)	(0.02)	(0.02)
F-stat	2539.85	2509.75	2325.47
N	43983	43983	43983
C: Heterogeneity in the Effect by Low Prob. of Post-Conviction			
Pretrial Incarceration	-0.07	-0.06	-0.05
	(0.02)	(0.02)	(0.02)
First Stage F-stat	1489.35	1450.90	1375.18
Pretrial Incarceration × Low Prob. of Post-Conviction	-0.39	-0.22	-0.23
	(0.14)	(0.12)	(0.12)
N	45107	45107	45107
Fixed Effects	✓	✓	✓
Demographic covariates		✓	✓
Case covariates			✓

Table A10: The Effect of Pretrial Incarceration on Turnout, Excluding Cases Before Election Day (Panel A); Excluding the Incapacitated (Panel B), and By Low Probability of Post-Conviction Incapacitation (Panel C). For each threshold (from 0-6 months before the election), we exclude the cases filed in that time period. Fixed effects, demographic, and case covariates are as described in Table A5. For details on our measure of likely incapacitation and the construction of this sample (cases that rarely result in post-conviction incarceration), see Appendix B.4.5. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

A. Complete Sample: 45107 observations			
		Differences Between:	
	Black and White Judges	Black and Hispanic Judges	Hispanic and White Judges
Pretrial Incarceration	-0.02	-0.01	-0.01
p-value	(0.00)	(0.12)	(0.03)
Age (years of age)	-0.23	-0.34	0.11
p-value	(0.31)	(0.15)	(0.40)
Black defendant	-0.01	-0.02	0.00
p-value	(0.09)	(0.06)	(0.60)
White defendant	0.01	0.01	0.00
p-value	(0.50)	(0.35)	(0.59)
Hispanic defendant	0.01	0.01	0.00
p-value	(0.50)	(0.35)	(0.59)
Female	0.01	-0.00	0.01
p-value	(0.40)	(0.77)	(0.04)
Prev. Turnout	0.00	0.00	0.00
p-value	(0.63)	(0.79)	(0.73)
Turnout	0.01	0.01	0.00
p-value	(0.47)	(0.68)	(0.61)
Not Eligible	0.00	0.00	-0.01
p-value	(0.32)	(0.82)	(0.03)
Registration	0.01	0.00	0.01
p-value	(0.38)	(0.87)	(0.21)
Violent charge	0.00	0.01	0.00
p-value	(0.77)	(0.37)	(0.26)
Any Drug	0.00	0.00	0.00
p-value	(0.76)	(0.84)	(0.86)
Any Weapon	0.00	0.00	0.00
p-value	(0.37)	(0.18)	(0.38)
Any Property	0.00	0.00	0.00
p-value	(0.95)	(1.00)	(0.90)
Any Prior	-0.01	-0.01	0.00
p-value	(0.19)	(0.42)	(0.42)
Num. days PI	-3.48	-1.80	-1.68
p-value	(0.01)	(0.19)	(0.09)
B. Sample of Black Defendants: 21199			
		Differences Between:	
	Black and White Judges	Black and Hispanic Judges	Hispanic and White Judges
Pretrial Incarceration	-0.02	-0.01	-0.01
p-value	(0.03)	(0.26)	(0.09)
Age (years of age)	0.12	-0.07	0.19
p-value	(0.72)	(0.84)	(0.31)
Female	0.02	0.01	0.01
p-value	(0.09)	(0.40)	(0.13)
Prev. Turnout	0.00	0.00	0.01
p-value	(0.73)	(0.87)	(0.36)
Turnout	0.00	0.00	0.01
p-value	(0.71)	(0.92)	(0.39)
Not Eligible	-0.01	0.01	-0.01
p-value	(0.45)	(0.63)	(0.02)
Registration	0.01	0.00	0.01
p-value	(0.50)	(0.85)	(0.12)
Violent charge	0.01	0.01	-0.01
p-value	(0.49)	(0.23)	(0.32)
Any Drug	-0.01	-0.01	0.00
p-value	(0.24)	(0.42)	(0.54)
Any Weapon	0.00	0.01	-0.01
p-value	(0.96)	(0.26)	(0.04)
Any Property	0.01	0.01	0.00
p-value	(0.44)	(0.58)	(0.73)
Any Prior	0.00	0.01	-0.01
p-value	(0.80)	(0.33)	(0.18)
Num. days PI	-3.54	-2.92	-0.63
p-value	(0.08)	(0.17)	(0.66)

Table A11: Difference in Case and Demographic Covariates Across Judge Race. Panel A: all cases. Panel B: cases involving a Black defendant. This table contains tests of difference in means across the specified groups, p-values are reported in parentheses.

The Effect of Pretrial Incarceration on Turnout

	2SLS (1)	OLS (2)	2SLS (3)
All Defendants:			
Pretrial Incarceration	-0.07		-0.07
	(0.02)		(0.02)
Hispanic Judge		-0.01	-0.01
		(0.01)	(0.01)
White Judge		-0.01	0.00
		(0.01)	(0.01)
N	45107	45107	45107
Black Defendants:			
Pretrial Incarceration	-0.11		-0.11
	(0.03)		(0.03)
Hispanic Judge		-0.00	-0.00
		(0.01)	(0.01)
White Judge		-0.00	0.00
		(0.01)	(0.01)
N	21199	21199	21199
Hispanic Defendants:			
Pretrial Incarceration	-0.03		-0.03
	(0.04)		(0.04)
Hispanic Judge		-0.00	-0.00
		(0.01)	(0.01)
White Judge		-0.00	0.00
		(0.01)	(0.01)
N	17077	17077	17077
Fixed Effects	✓	✓	✓
Demographic covariates	✓	✓	✓
Case covariates	✓	✓	✓

Table A12: The Effect of Pretrial Incarceration on Turnout with and without Race of the Judge as Control. Fixed effects, demographic, and case covariates are as described in Table A5. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
Pretrial Incarceration (baseline: Black Defendant, Black Judge)	-0.19	-0.17	-0.17
	(0.08)	(0.07)	(0.07)
First Stage F-stat	319.61	316.32	290.17
Pretrial Incarceration × Hispanic Defendant	0.16	0.11	0.12
	(0.17)	(0.13)	(0.13)
First Stage F-stat	221.74	221.47	221.39
Pretrial Incarceration × White Defendant	-0.06	-0.03	-0.02
	(0.12)	(0.10)	(0.10)
First Stage F-stat	77.53	75.74	73.91
Pretrial Incarceration × Hispanic Judge	0.16	0.15	0.15
	(0.09)	(0.07)	(0.07)
First Stage F-stat	227.89	212.27	212.52
Pretrial Incarceration × White Judge	0.04	0.06	0.06
	(0.09)	(0.07)	(0.07)
First Stage F-stat	331.10	321.59	304.99
Pretrial Incarceration × Hispanic Defendant × Hispanic Judge	-0.30	-0.22	-0.23
	(0.13)	(0.11)	(0.11)
First Stage F-stat	90.50	87.69	87.48
Pretrial Incarceration × White Defendant × Hispanic Judge	0.21	0.16	0.15
	(0.19)	(0.16)	(0.16)
First Stage F-stat	27.47	26.39	24.56
Pretrial Incarceration × Hispanic Defendant × White Judge	-0.08	-0.05	-0.05
	(0.13)	(0.11)	(0.11)
First Stage F-stat	139.07	138.11	138.47
Pretrial Incarceration × White Defendant × White Judge	0.15	0.09	0.09
	(0.18)	(0.14)	(0.14)
First Stage F-stat	46.71	45.41	44.87
N	45107	45107	45107
Fixed Effects	✓	✓	✓
Demographic covariates		✓	✓
Case covariates			✓

Table A13: Second-Stage: The Effect of Pretrial Incarceration on Turnout by Race of the Defendant and Race of the judge (All Interactions). Fixed effects, demographic, and case covariates are as described in Table A5. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

A. Complete Sample: 45107 observations	Differences Between: Experienced and Inexperienced Judges
Pretrial Incarceration	-0.01
p-value	(0.02)
Age (years of age)	0.09
p-value	(0.45)
Black defendant	-0.01
p-value	(0.25)
White defendant	0.00
p-value	(0.63)
Hispanic defendant	0.01
p-value	(0.13)
Female	0.00
p-value	(0.40)
Prev. Turnout	-0.00
p-value	(0.33)
Turnout	0.00
p-value	(0.37)
Not Eligible	0.01
p-value	(0.00)
Registration	0.01
p-value	(0.25)
Violent charge	0.00
p-value	(0.90)
Any Drug	-0.02
p-value	(0.00)
Any Weapon	0.00
p-value	(0.42)
Any Property	0.01
p-value	(0.00)
Any Prior	0.01
p-value	(0.07)
Num. days PI	-1.52
p-value	(0.11)

B. Sample of Black Defendants: 21199	Differences Between: Experienced and Inexperienced Judges
Pretrial Incarceration	-0.01
p-value	(0.09)
Age (years of age)	-0.24
p-value	(0.17)
Female	0.01
p-value	(0.21)
Prev. Turnout	-0.01
p-value	(0.13)
Turnout	0.00
p-value	(0.42)
Not Eligible	0.02
p-value	(0.00)
Registration	0.00
p-value	(0.79)
Violent charge	0.01
p-value	(0.22)
Any Drug	-0.03
p-value	(0.00)
Any Weapon	0.00
p-value	(0.08)
Any Property	0.02
p-value	(0.01)
Any Prior	0.00
p-value	(0.88)
Num. days PI	-1.40
p-value	(0.28)

Table A14: Difference in Case and Demographic Covariates Across Judge Experience. Panel A: all cases. Panel B: cases involving a Black defendant. This table contains tests of difference in means across the specified groups, p-values are reported in parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
Pretrial Incarceration (baseline: Black Defendant, Inexperienced Judge)	-0.09	-0.08	-0.08
	(0.03)	(0.03)	(0.03)
First Stage F-stat	475.10	469.76	431.13
Pretrial Incarceration × Hispanic Defendant	-0.03	-0.01	-0.00
	(0.05)	(0.04)	(0.04)
First Stage F-stat	327.56	327.93	329.93
Pretrial Incarceration × White Defendant	0.07	0.14	0.15
	(0.07)	(0.06)	(0.06)
First Stage F-stat	112.16	109.77	108.27
Pretrial Incarceration × Experienced Judge	-0.02	0.00	0.00
	(0.04)	(0.04)	(0.04)
First Stage F-stat	429.62	413.34	413.11
Pretrial Incarceration × Experienced Judge × Hispanic Defendant	0.08	0.01	0.02
	(0.06)	(0.06)	(0.06)
First Stage F-stat	176.23	174.89	179.35
Pretrial Incarceration × Experienced Judge × White Defendant	-0.04	-0.14	-0.14
	(0.10)	(0.08)	(0.08)
First Stage F-stat	58.10	55.82	54.18
N	45107	45107	45107
Fixed Effects	✓	✓	✓
Demographic covariates		✓	✓
Case covariates			✓

Table A15: Second-Stage: The Effect of Pretrial Incarceration on Turnout by Race of the Defendant and Judge Experience (All Interactions). Fixed effects, demographic, and case covariates are as described in Table A5. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

E Compliers

In Table A16, we compare the sample of compliers to the overall sample using the stratified approach to characterize compliers in Dahl et al. (2014) and Abadie (2003). Table A17 follows the approach advanced by Aronow and Carnegie (2013) and weights our 2SLS main specification by the inverse of the probability of being a complier (Dahl et al. (2014)). Table A16 shows that compliers are not substantially different from the average defendant.

	All	
	Sample	Compliers
Demographic:		
Age	35.775	35.149
	(0.049)	(0.090)
Female	0.195	0.208
	(0.001)	(0.002)
Race:		
Black	0.470	0.449
	(0.002)	(0.003)
White	0.151	0.143
	(0.001)	(0.002)
Hispanic	0.379	0.408
	(0.002)	(0.003)
Income:		
Below Median	0.368	0.356
	(0.002)	(0.005)
Above Median	0.317	0.310
	(0.002)	(0.004)
Not available	0.315	0.335
	(0.002)	(0.007)
Case-related:		
Any drug offense	0.246	0.124
	(0.002)	(0.003)
Any violent offense	0.145	0.199
	(0.001)	(0.001)
Any property offense	0.290	0.189
	(0.002)	(0.003)
Any firearm offense	0.034	0.052
	(0.002)	(0.016)
Any prior case	0.712	0.683
	(0.001)	(0.011)
Electoral covariates:		
Post-treatment turnout	0.280	0.278
	(0.002)	(0.003)
Pretreatment turnout	0.291	0.299
	(0.001)	(0.002)
Voting-age-ineligible	0.070	0.063
	(0.001)	(0.002)
Pretreatment registration	0.527	0.520
	(0.001)	(0.011)

Table A16: Complier Comparison. This table presents the covariate means for the overall sample and the sample of "compliers", following the estimation approach in Dahl et al. (2014) and Abadie (2003). Compliers are defined as the defendants whose pretrial incarceration decision would have been different had their case been assigned to the most strict instead of the most lenient judge. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
A. Main Finding			
Pretrial Incarceration	-0.13	-0.10	-0.10
	(0.03)	(0.02)	(0.03)
First Stage F-stat	993.50	973.32	686.35
B. Pretrial Incarceration × Race			
Pretrial Incarceration (baseline: Black Defendant)	-0.12	-0.09	-0.09
	(0.04)	(0.03)	(0.03)
First Stage F-stat	329.84	325.83	229.47
Pretrial Incarceration × Hispanic	-0.06	-0.03	-0.03
	(0.05)	(0.04)	(0.04)
First Stage F-stat	176.16	175.76	178.16
Pretrial Incarceration × White	0.06	0.03	0.04
	(0.07)	(0.06)	(0.06)
First Stage F-stat	57.74	58.64	59.87
Fixed Effects	✓	✓	✓
Demographic covariates		✓	✓
Case covariates			✓
N	45107	45107	45107

Table A17: SECOND-STAGE: THE EFFECT OF PRETRIAL INCARCERATION ON TURNOUT (WEIGHTED BY PROBABILITY OF BEING A COMPLIER). Fixed effects, demographic, and case covariates are as described in Table A5. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

F Persistence

We examine whether the effect we have presented is long-lasting. We restrict our attention to those individuals that had cases between 2008 and 2012, and estimate the effect of pretrial incarceration on 2012 and 2016 turnout. Figure A5 shows that the effect is negative and statistically significant for 2012 turnout but not for 2016 turnout.

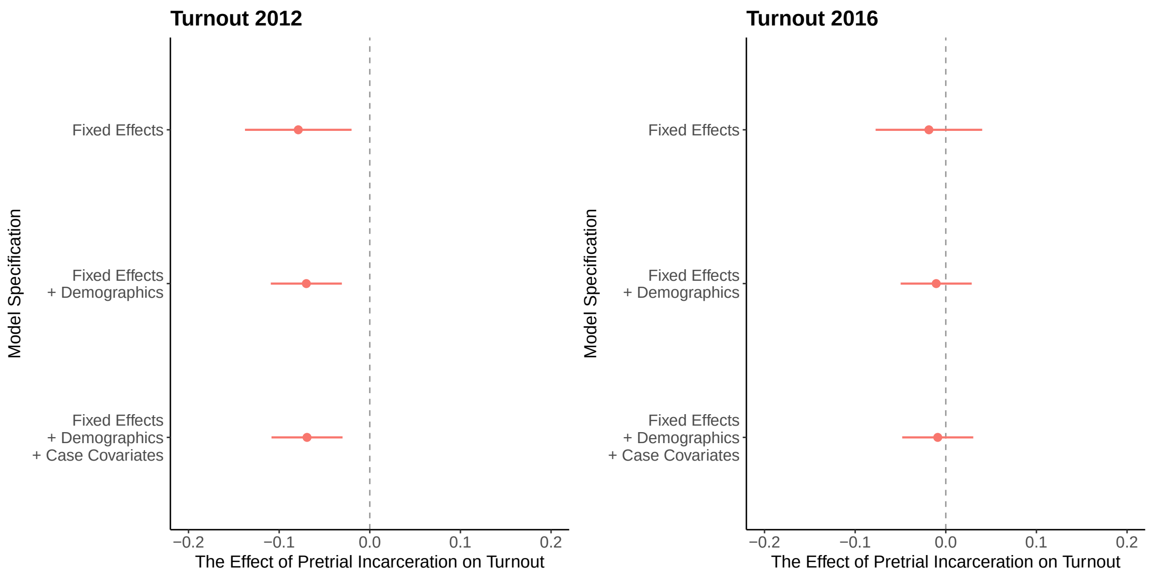

Figure A5: The Effect of Pretrial Incarceration on 2012 Turnout and 2016 Turnout. The sample of interest focuses on those defendants detained between 2008 and 2012 and that were not rearrested between 2012 and 2016. Marginal effects based on the second-stage 2SLS estimates. 95% confidence intervals from models that include fixed effects, demographic and case covariates.

To separate the direct effect of pretrial incarceration on 2016 turnout from the indirect effect of pretrial incarceration on 2016 turnout (through 2012 turnout), we use the approach of Dippel et al. (2020) for mediation analysis with one instrument. The lack of a long-term effect is corroborated when we resort to mediation analysis where 2012 turnout is the mediator and 2016 turnout is the outcome, as the direct and indirect effects are all near zero as shown in Table A18. The results are consistent with the possibility that the impact of pretrial incarceration does not operate through long-term constant losses but through shorter-term or long-term nonconstant factors, such as short-lived resource losses and decaying socialization.

	Direct Effect	Indirect Effect	Total Effect
Estimate	0.004	-0.018	0.014
Std. Error	(0.010)	(0.030)	(0.020)
N	27687		

Table A18: The Direct and Indirect (through 2012 Turnout) Effects of Pretrial Incarceration on 2016 Turnout. The sample of interest focuses on those defendants detained between 2008 and 2012. Bootstrapped Std. Errors (500 bootstrap samples) clustered at the Judge-Year level are presented in parentheses.

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