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

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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:

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

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.

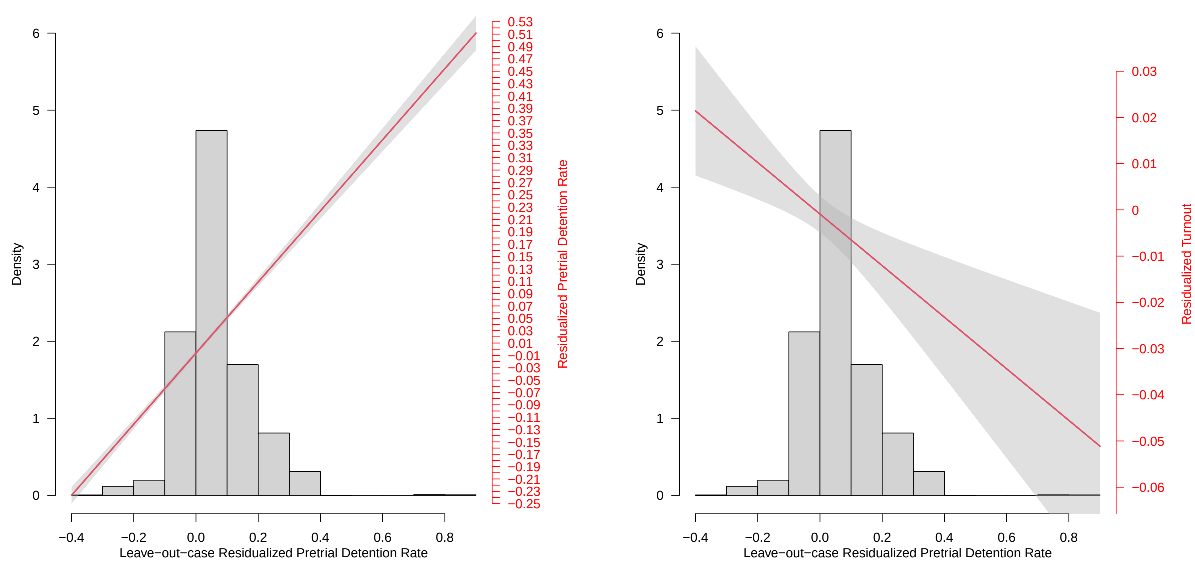

Figure A1: Distribution of our Residualized Judge Punitiveness Instrument against Residualized Pretrial Incarceration (left) and Residualized Turnout (right). Residualizing partials out the variation from the fixed effects.

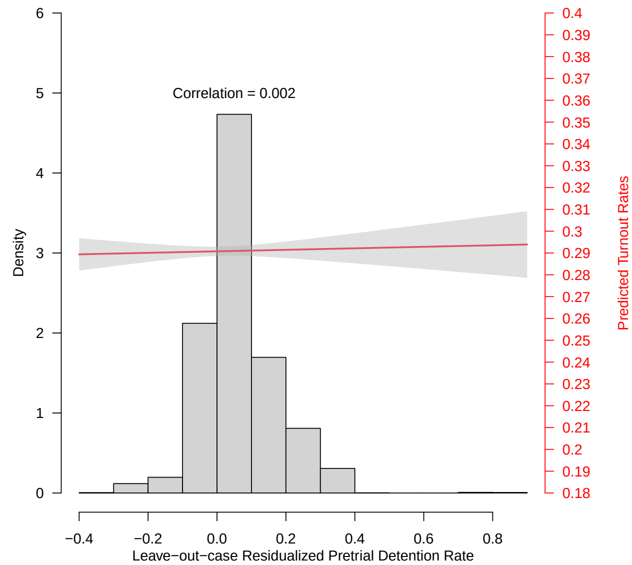

Figure A2: Relationship Between Residualized Judge Punitiveness Instrument and Predicted Turnout. Predicted turnout is based exclusively on demographic and case-level covariates. The flat line indicates no meaningful correlation between these measures (correlation: 0.002).

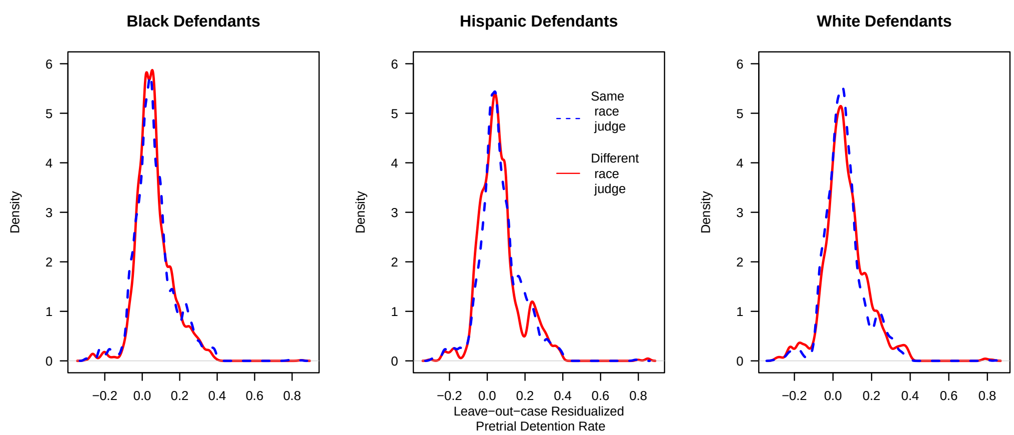

Figure A3: Distribution of the (residualized) Judge Punitiveness Across Race of the Defendant × Race of the Judge

	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).

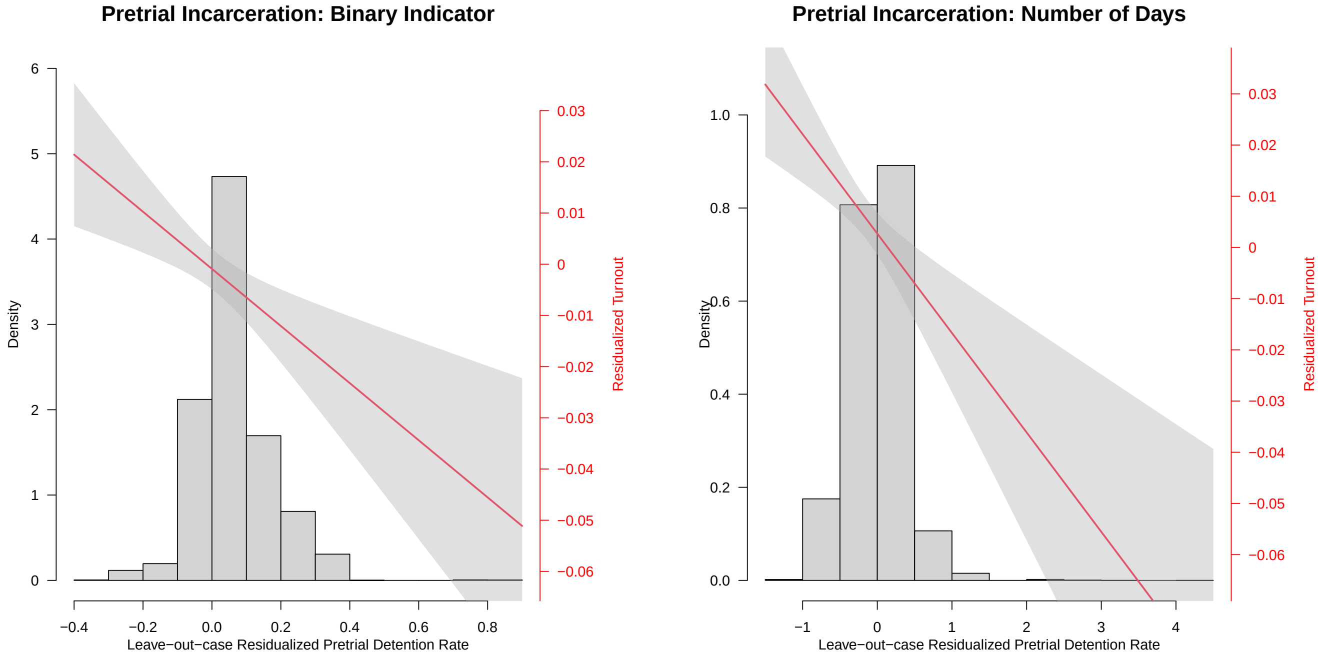

Figure A4: Relationship Between Residualized Judge Punitiveness Instrument and Predicted Turnout. The main (binary) instrument (left) is based on pretrial incarceration coded as 1 if detained for more than 3 days and 0 otherwise. The continuous instrument (right) is based on the logarithm of the number 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.

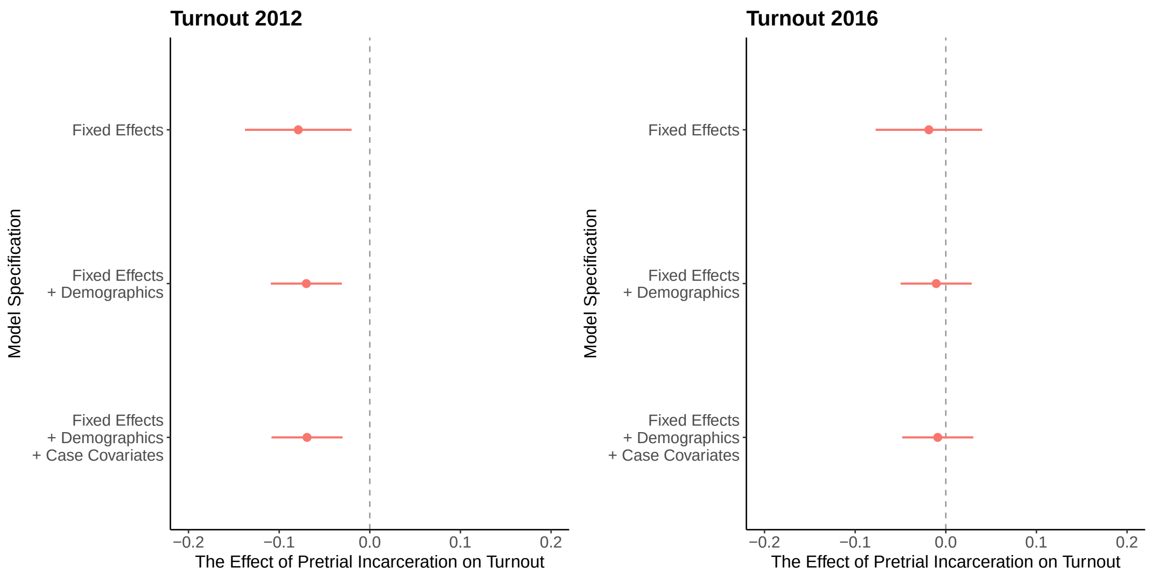

Figure A5: The Effect of Pretrial Incarceration on 2012 Turnout and 2016 Turnout. The sample of interest focuses on those defendants detained between 2008 and 2012 and that were not rearrested between 2012 and 2016. Marginal effects based on the second-stage 2SLS estimates. 95% confidence intervals from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