Jailed While Presumed Innocent: The Demobilizing Effects of Pretrial Incarceration[12]

Anne McDonough[3] Ted Enamorado[4] Tali Mendelberg[5]

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

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[3] Department of Politics, Princeton University. 001 Fisher Hall, Princeton NJ, 08544. Email: mcdonough@princeton.edu.

[4] Assistant Professor of Political Science, Washington University in St. Louis. CB1063, One Brookings Drive, St. Louis, MO 63130-4899. Email: ted@wustl.edu.

[5] Corresponding author, John Work Garrett Professor of Politics, Princeton University. 001 Fisher Hall, Princeton NJ, 08544. Phone: 609-258-4750. Email: talim@princeton.edu.

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

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

Pretrial incarceration is one such practice. It is significant for three reasons. First, it is a major component of the American carceral state. Local jails process roughly 10 million more cases a year than state and federal prison combined, and on a given day, nearly two-thirds of those in local jail are awaiting trial (Sawyer and Wagner 2020). Second, pretrial incarceration contravenes standard civil liberties protections and imposes harsh punishment (Meares and Rizer 2020). It can average five months in many jurisdictions, even though most cases involve nonviolent charges (Sawyer and Wagner 2020; Stevenson 2018). That is nearly half the minimum punishment for involuntary manslaughter (U.S. Sentencing Commission 2018). Yet bail hearings often last less than 2 minutes (Meares and Rizer 2020, 19; Scott-Hayward and Ottone 2017, 172). Furthermore, as many as half are later found not guilty or have their charges dropped (Rabuy and Kopf 2016). Third, the pretrial system is upwardly redistributive, benefitting economically powerful private bail companies at the expense of the accused (Page et al. 2019). The median person in local jail is

nonwhite with a pre-incarceration income of $16,000 a year (Gupta et al. 2016; Page et al. 2019, 156; Rabuy and Kopf 2016).

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

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

Estimating the effect of pretrial incarceration is challenging. Those who experience it differ from those who do not on important covariates. To identify causal estimates, we merge voter records with over 100,000 court cases from a large natural experiment in Philadelphia County. Two features of Philadelphia's pretrial process produce this natural experiment. First, after arrest, defendants are as-if randomly assigned to one of six bail magistrates who decide whether to release the defendant and on what conditions (see Figure A1 in Appendix A). Assignment is as-if random

because defendants are automatically assigned to the magistrate on duty, and all magistrates rotate through all shifts. The magistrates thus face the same types of defendants, cases, and circumstances, overall. Second, magistrates have discretion, and they vary in their tendency to set burdensome conditions for release and thus in their rate of pretrial incarceration (Dobbie et al. 2018, Stevenson 2018). By comparing similar defendants as-if randomly assigned to relatively harsh or lenient magistrates, we can isolate the causal effect of pretrial incarceration. The Philadelphia data offers additional advantages. It provides full case records, including covariates such as crime severity and prior offenses. This avoids error-prone self-reports and omitted-variable bias (White 2019). Finally, the Philadelphia case generalizes to other large jurisdictions in its use of money bail and deference to magistrate discretion, in the typical length of pretrial incarceration, and in the overrepresentation of Black Americans among pretrial detainees (Pretrial Justice Institute 2009; Olson 2012, Chauhan et al. 2017; Arnold et al. 2018).

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

Even with these limitations, there is clear evidence that pretrial incarceration in the months before the election reduces turnout for Black defendants. We are not aware of political science studies that focus on pretrial incarceration (or on other "shadow carceral" practices). These findings suggest that studies omitting the shadow carceral state under-estimate the reach of the carceral state. The effects of overall incarceration extend to a much larger population than those

convicted, who have been the focus of the literature. In addition, the study has implications for racial inequality. The expansion of the carceral state has disproportionately affected Black Americans (Baumgartner et al. 2018; Burch 2011; Lerman and Weaver 2014; Western 2006). We find that this racially disparate impact extends to pretrial incarceration. Pretrial incarceration thus makes a substantial negative difference for citizens who otherwise would have exercised an important right of citizenship and gained political representation.

The Shadow Carceral State and Pretrial Incarceration

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

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

accounts for nearly the entire growth of the jail population since 1997 and is a chief reason the U.S. leads the world in the number incarcerated.[6]

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

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

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

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

The Impact of Pretrial Incarceration on Behavior

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

the shadow carceral state's arbitrary, racially disparate, and extractive practices would do so at least as much.

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

Burch arrived at a different conclusion, using voting and correctional records in five states. Comparing people who had been convicted before and after the 2008 election, Burch found that conviction *increases* turnout in three states. Burch explains that prison may spur a "revolutionary consciousness" among those who perceive their incarceration to be "harsh or unfair" (2011, 723). However, this finding could have resulted from the historic nature of the 2008 election and the grassroots organizing that targeted individuals with felony convictions. Moreover, that data excludes jails, where most cases of incarceration – and pretrial incarceration – occur.

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

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

incarceration.[7]

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

The Negative Effects of Pretrial Incarceration

Hypothesis 1. *Main Hypothesis: Pretrial Incarceration Decreases Voter Turnout*

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

First, pretrial incarceration triggers real costs (Gupta et al. 2016; Heaton et al. 2017; Stevenson 2018). It increases job and housing instability and family disruption, and causes

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

substantial income drops. In Philadelphia, it is associated with an average loss of $40,000 in reported earnings and government benefits and 11% lower chance of being employed (Dobbie et al. 2018). It may demobilize defendants because it diminishes the resources that are well-known to facilitate political participation (Schlozman et al. 2012). Heterogenous effects consistent with the resources mechanism are further described in hypotheses 2 and 3 below.

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

Third, pretrial incarceration may affect voting through incapacitation. It directly increases the difficulty of voting while incarcerated during the election, and indirectly increases the outright inability to do so because of felony imprisonment. This process is not a violation of assumptions or a source of bias; rather, it is caused by pretrial incarceration, and follows it in the sequence of time. We elaborate on this mechanism in hypothesis 4, below.[8]

[8] Other mechanisms include misinformation and system avoidance (see Appendix C).

Hypothesis 2. *Resource Deprivation*

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

Hypothesis 3. *Racially Disparate Impact*

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

Hypothesis 3a: If resource deprivation drives the treatment effect, the consequences will be more severe for Black than White defendants, because Black defendants have fewer resources. Black defendants have lower pre-arrest wealth and much less access to family assets (Page et al. 2019). Many Black Americans live in disproportionately Black neighborhoods and those areas are more resource-poor (Massey and Denton 1993). The criminal justice system triggers larger decreases in Blacks' incomes and stigmatizes them more when seeking employment (Apel and Powell 2019; Harris and Harding 2019). That is, the resource effects of incarceration are likely worse for Blacks. If racial disparities are due to resources, and Blacks are more affected because they are more likely to live in poverty, racially disparate impacts will diminish when we condition on income. Incarceration effects will be equal for Black and White defendants at every income level.

Hypothesis 3b: A second reason why pretrial incarceration may especially affect Black defendants is vote propensity. Blacks are more likely than Whites to have voted before

incarceration (White 2019). Blacks' turnout thus has more room to decline, and incarceration may make a bigger difference for them than for Whites. We examine this hypothesis by comparing treatment effects for Black and White prior voters, and for Black and White nonvoters. If racial heterogeneity remains, then it is not due to prior voting.

Hypothesis 3c: If political socialization explains the effect of pretrial incarceration, it will likely manifest as racialized political socialization, and pretrial incarceration will especially affect Blacks (Lerman and Weaver 2014). Through racially targeted practices, law enforcement associates nonwhite identity with inferior citizenship (Soss and Weaver 2017; see also Baumgartner et al. 2018; Mummolo 2018). These practices influence Blacks' perceptions of fairness in the criminal justice system and government institutions more broadly (Cohen 2010). Survey data reveals that "when Blacks are treated unfairly because of their race they are likely to impugn the fairness of the wider system" (Peffley and Hurwitz 2010, 55).

This logic applies even more to pretrial incarceration, a particularly harsh and racially disparate feature of the carceral state (Page et al. 2019). Black defendants face much higher bail amounts than White defendants with similar charges and conviction histories, and are therefore less likely to be released pretrial (Arnold et al. 2018; See also Appendix H). This racially disparate reality is not difficult for detainees to perceive. A defendant incarcerated pretrial in Philadelphia on a typical day would see mostly other Black defendants (Philadelphia Research Initiative 2011). Testimonials suggest that Blacks perceive the pretrial system specifically as a racial injustice. As a Black man held pretrial for four months put it:

> "...If they can make a dollar off of us, they will...They are not in any big hurry to get you to a trial, to get you to a judge. They make money off of you sitting in there... You need to let me go. They need to incriminate real criminals. Stop the discrimination based on race, and if this

person dresses a certain way. Stop degrading people and tearing people down within the system... the system has no heart. It's just a zombie going around killing people, destroying lives." (Gilbert, n.d.).

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

Hypothesis 3d: These mechanisms imply moderate demobilizing effects among Hispanic defendants. Hispanics occupy a liminal position. Hispanic defendants' pre-arrest incomes are typically between White and Black defendants' incomes (Page et al. 2019). Some Hispanics experience targeted policing and perceive bias in the legal system, though not as strongly as Blacks (Walker 2019). Therefore, they are less likely than Blacks to interpret punitive practices as a form of structural bias against their group. We analyze the effects of pretrial incarceration separately for Hispanic defendants. However, this test is tentative because of limitations in identifying Hispanics (as discussed later).

Hypothesis 4. *Incapacitation*

Aside from resources and race, incarceration may reduce voting through incapacitation. Pretrial incarceration increases the marginal likelihood of being convicted, because its cascading effects on lost employment, income, relationships, and ability to build a defense can lead defendants to plead guilty (Dobbie et al. 2018; Stevenson 2018). Those serving a felony sentence during the election are prohibited by Pennsylvania law from voting. Thus, pretrial incarceration may reduce turnout by triggering prison time, which in turn may reduce turnout. In addition, while defendants awaiting trial and those serving a non-felony sentence are allowed to vote, it may be difficult to do so (Lerman and Weaver 2014). Because only 5.2% of the sample are incarcerated pretrial on election day, we cannot test this latter pathway by itself, but we can combine it with

post-conviction incapacitation, maximizing our power to detect overall incapacitation effects. (Appendix T offers further detail.) As a suggestive test of incapacitation, we use a mediation analysis.

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

Data on Pretrial Incarceration and Turnout

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

We use all cases filed in Philadelphia County between the 2008 and 2012 general elections (Stevenson 2018). Each observation contains defendant information (name, birthdate, race, gender, zip code, the number of prior cases and convictions in Pennsylvania) and case information (arrest date, date and time of the bail hearing, bail magistrate, offense charges, pretrial conditions such as released on recognizance or monetary bail, and pretrial incarceration release date). Appendix D provides details. These data provide a rich set of covariates, and directly measure pretrial detention.

Following Dobbie et al. (2018) and Stevenson (2018), we measure pretrial incarceration as being detained for more than three days after the bail hearing, unless otherwise noted.

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

We then merge the court records with state voter files. First, we use raw Pennsylvania (PA) files from 2009 and 2013 to measure 2008 and 2012 turnout and registration, respectively.[9] However, using PA voter records exclusively would bias our estimates if people detained pretrial are subsequently more likely to move out of state. Therefore, we also match defendants not in the PA voter file with the L2 uniform 2014 voter files of all remaining states. This latter match represents a small percent of our matched cases. See Appendix E for more details.

To merge these records, we use the probabilistic method developed by Enamorado et al. (2019). Its main advantage is the flexibility to account for the uncertainty surrounding the merging process (by controlling error rates). Moreover, as shown by Enamorado et al. (2019), it is robust to typographical errors and missing data, and outperforms deterministic (rule-based) approaches. We merge the records using name, gender, and birthdate. The estimated match probability is reweighted to account for the frequency of names: matches on common (less common) names are

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

down-weighted (up-weighted) according to the empirical distribution of each name. In post-merge analysis, we reweight turnout and registration status by the match probabilities to account for the uncertainty surrounding the merge and produce consistent estimates (see Appendix F). Altogether, the match rate is 55%. Appendix N shows a similar rate with a deterministic approach.

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

Table G1 describes the sample. In our sample, 36% of defendants were incarcerated pretrial (detained more than 3 days). Detainees' pretrial jail time averaged nearly 5 months, with a median of 2.6 months. Their median bail was $10,000, suggesting most were unable to secure $1,000 or less. Detained and released defendants share some similarities. Both tend to be male, Black, live in poor areas, face minor and nonviolent charges, have a prior case, and are unlikely to vote.[10] There are also some differences. Compared to released defendants, detainees are more likely to be Black, poor, and male. They face somewhat more serious charges, but their charges tend to be minor nonetheless, and most did not face any violent charge. Detainees are less likely to have

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

voted than released defendants, but this gap grows post-treatment, suggesting a pretrial incarceration effect.

Natural Experiment

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

To overcome this challenge, we analyze a natural experiment in Philadelphia's court system. Philadelphia County uses an as-if-random process to assign defendants to bail magistrates who differ in their propensity to incarcerate defendants pretrial. Specifically, assignment is as-if random because defendants are assigned to the magistrate on duty, and magistrates rotate through all shifts. One magistrate works a particular shift for five days, then takes five days off, then works a different shift for five days, and so on. This rotation proceeds throughout weekends and holidays. Studies of this jurisdiction find no evidence of strategic manipulation or substantial deviation from the assigned schedule (Stevenson 2018, 516-519). Some magistrates are consistently more likely to set higher bail amounts that result in pretrial incarceration compared to magistrates deciding observably similar cases, as studies of this jurisdiction show (Dobbie et al. 2018; Stevenson 2018).

The decision tendencies of randomly assigned magistrates present an exogenous source of variation in pretrial incarceration. A defendant who was released by one magistrate may have been detained pretrial had they been assigned to a magistrate with more punitive tendencies. This design identifies the local average treatment effect (LATE) for defendants on the margin of incarceration and release. For these defendants, the likelihood of experiencing incarceration is not driven by

confounding, preexisting characteristics. Instead, these defendants are incarcerated due to an exogenous source of variation: the randomly assigned magistrate's decision tendencies.

Constructing the Instrument

Following Aizer and Doyle (2015) and Dobbie et al. (2018), we construct an instrument using the as-if random assignment of bail magistrates to cases. The instrument leaves out the focal case and uses all the other cases seen by the same magistrate in the same time period. Similarly to Aizer and Doyle (2015) and Stevenson (2018), we allow our instrument to vary by case severity and year. For example, a magistrate who is more lenient than others on low-level offenses may be harsh on severe offenses. Such heterogeneity in magistrate tendencies has been documented in our setting (Stevenson 2018).[11] To code case severity, we sum the Pennsylvania Offense Gravity Scores (OGS) across the offenses in the case, and bin that into terciles (see Appendix D). Thus, for a given defendant, the instrument represents the proportion of other cases of a similar severity level decided by the same magistrate in the same year that resulted in pretrial incarceration. Specifically, as in Aizer and Doyle (2015) and Dobbie et al. (2018), we construct our instrument using the following equation:

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

where N_{tjh} is the number of cases seen by magistrate j at year t and case severity h, N_{dtjh} is the number of cases where defendant d was involved and seen by magistrate j at year t and case severity h, and $P_{dctjh} \in \{0,1\}$ represents the decision (detained = 1 or released = 0) made by

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

magistrate *j* in case *c* for defendant *d* at year *t* and case severity *h*. Our final sample includes 6 magistrates per year, except 2008 and 2009 when there were 2 vacancies (see Appendix M). The median number of cases per magistrate-by-year is 6,569.5, and the median number of cases per magistrate-by-year-by-OGS-tercile is 2,031. The leave-out-case pretrial detention rate ranges from 0.06 to 0.71, with an average of 0.35 and a standard deviation of 0.20. Moving from the most to the least lenient magistrate increases the likelihood of pretrial detention by 14 percentage points for defendants in the lowest tercile of offense severity, almost 16 points for those in the middle tercile, and 18 points for those with the most serious offenses.

To illustrate the relationship between magistrate leniency and our variables of interest, Figure H1 presents the non-parametric fit between the residualized instrument and the residualized pretrial detention (left panel) and residualized turnout (right panel). As expected, the figure shows a strong and positive relationship between our instrument and pretrial incarceration, and a negative relationship (via reduced form) between our instrument and turnout. The figure also shows the residualized distribution of the instrument, confirming that the tails of the distribution do not drive the main effect. We find sufficient variation in the instrument to predict the endogenous variable (pretrial incarceration) and the outcome (turnout), motivating further analysis.

Empirical Strategy

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

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

and the second stage is:

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

where d indicates defendant, j is for magistrate, and h is the offense severity level. $T_{d,2012}$ is an indicator for voting in 2012, P_{dtjh} is an indicator for being detained pretrial more than 3 days, Z_{dtjh} is the instrument, and X_{dt} is a set of defendant, case covariates and fixed effects.[7]

Assessing the Instrument's Validity

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

We discuss each assumption in turn. First, our instrument must be exogenous, meaning that the likelihood that a defendant faces a harsh or lenient judge is uncorrelated with preexisting characteristics. If the instrument is not exogenous, observed associations between pretrial incarceration and turnout may reflect, for example, defendant or case-related confounders. As noted by Stevenson (2018), if our instrument is exogenous, we should observe that case and defendant characteristics are distributed evenly across magistrates with different decision tendencies and should not be predictive of the instrument. We find that most covariates are not significantly related to the instrument (see Table H2). While some covariates on their own are significant predictors of the instrument and the covariates are jointly correlated with the instrument (joint F-stat = 3.320), the magnitude of those correlations is close to zero. To further test for potential violations of randomization, we construct a measure of predicted turnout using only the

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

demographic and case-level covariates, so that all the variation explaining turnout is coming from these covariates. As the flat red line in Figure H2 shows, there is no sizable correlation between this measure and the residualized instrument, as expected (r = -0.003). Finally, to check that omitting interactions between fixed effects in the randomization process does not lead to any biases, Table H3 replicates the results with interacted time fixed effects, following Dobbie et al. (2018).

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

Third, our instrument must be monotonic, meaning assignment to a more punitive magistrate should increase defendants' probability of pretrial incarceration regardless of their characteristics. If our instrument does not meet the monotonicity assumption, then it is not possible to identify the effect on compliers (Angrist and Pishke 2008). While no direct test of the monotonicity assumption exists, we can at least provide evidence that the instrument satisfies "average monotonicity" (Frandsen et al. 2019). Tables I1 and I2 present the first stage coefficients across a variety of subsamples. Assignment to stricter magistrates substantially increases the likelihood of pretrial detention across a wide variety of characteristics. To alleviate concerns about monotonicity violations of the instrument (that the instruments work differently for some

subgroups), we follow Stevenson (2018) and Aizer and Doyle (2015) in allowing the leniency of the judge to vary by time and case severity.

Finally, our instrument must meet the exclusion restriction, which requires that the treatment—assignment to a magistrate with a particular tendency—affects the outcome (turnout) only through the pretrial decision (release or incarceration pretrial). If the magistrate affects a defendant's political behavior through other channels, this assumption would be violated, and other unobserved confounders could drive our result. As noted by Stevenson (2018, 516-519), several factors lend support for the validity of the exclusion restriction. First, arrestees have no agency in selecting their bail magistrate. Second, bail magistrates' interactions with defendants are brief (less than two minutes on average), leaving minimal time for comments aside from release conditions. Finally, bail magistrates have no further interaction with defendants or jurisdiction over cases after the preliminary pretrial decision, meaning their pretrial decision is the only plausible way that they could affect the defendant.

Main Effect

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

[8] We do not use biprobit extensively because it can be sensitive to heteroscedasticity and computationally expensive due to the joint distributional assumptions (Chiburis et al. 2012). In

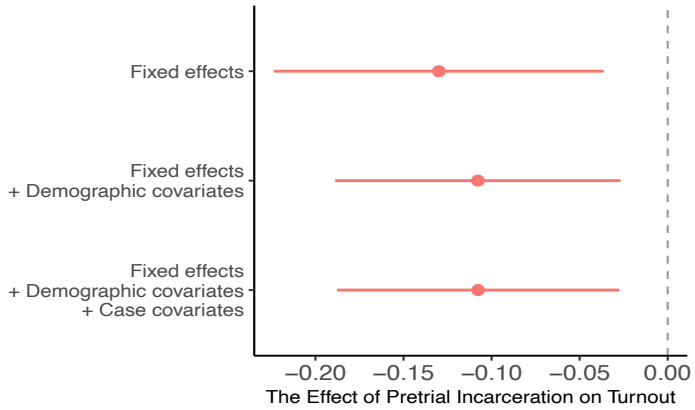

Figure 1. **The Effect of Pretrial Incarceration on 2012 Turnout.** Second-stage 2SLS estimates with 95% confidence intervals from models that include fixed effects only (top), fixed effects and demographic covariates (middle) and fixed effects, demographic and case-level covariates (bottom). Heteroskedastic-consistent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

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

practice, biprobit produces similar results to 2SLS but is not as robust to misspecification of the first stage (Angrist and Pischke 2008). OLS estimates show a 7 to 11 point effect (Table L1).

pretreatment) on the treatment instrument (Table M4). As expected, the treatment does not predict the pretreatment outcome.[9]

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

Resource Deprivation

If pretrial incarceration affects voting through resource deprivation, the magnitude may be greater among lower-income defendants, who have few resources to mitigate the socioeconomic repercussions. This requires a test for heterogeneous treatment effects by income. We code defendants with zip codes in Pennsylvania into terciles of average zip code income: bottom (< $25,888), middle ($25,888-$34,090), or top (> $34,090).[10] To keep the 2SLS model identified, we interact income terciles with leniency in the first stage and with predicted pretrial incarceration in the second stage (see Appendix J). We conduct two-tailed significance tests on these estimates (in Table J1), and also use them to calculate marginal effects (in Figure 2). Figure 2 (left panel) presents the marginal effect of pretrial incarceration on turnout for each income tercile, derived

[9] As an additional robustness check, we assess a related outcome – registering to vote post-treatment – among those not registered pre-treatment (39,751 defendants). The negative effect on registration is of similar magnitude (15 percentage points), though imprecise (Table M5).

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

from the interaction model in the third column of Table J1 (the specification that controls for fixed effects and case and demographic characteristics). The demobilizing effect of pretrial incarceration is acute among defendants in the bottom and middle terciles. For such defendants, pretrial incarceration reduces turnout by 12 percentage points. In the top tercile, the effect is -9 percentage points but cannot be statistically distinguished from zero. The effects in the bottom and middle terciles are not statistically distinguishable from one another ($p < 0.54$), whereas the effects on top-tercile defendants are statistically distinguishable from both middle ($p < 0.002$) and bottom-tercile defendants ($p < 0.02$). These results are consistent with the resource mechanism: pretrial incarceration especially affects defendants less able to withstand socioeconomic destabilization. To be sure, we cannot adjudicate whether income itself or factors correlated with income explain the income heterogeneity, and the aggregated, zip-code income measure that we use in this analysis is a noisy measure of individual resources.

Racially Disparate Impact

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

[11] We rely on surname prediction to identify Hispanic defendants (see Appendix D).

effects for Whites, White Anglos, and White Hispanics (see Table J1). Black defendants are especially affected by pretrial incarceration.

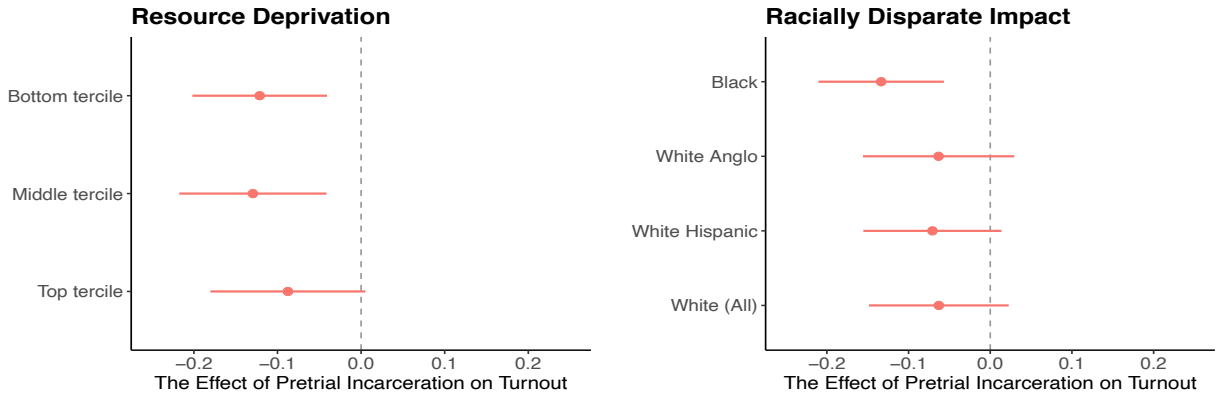

Figure 2. The Effect of Pretrial Incarceration on 2012 Turnout by Income (left) and Race (right). Marginal effects based on the second-stage 2SLS estimates of the models with interactions between pretrial incarceration and income (left) and between pretrial incarceration and race (right). 95% confidence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

Are Black defendants especially affected because they tend to be poor? If so, pretrial incarceration would affect poor Blacks and Whites equally, and income would matter for each racial group. To test this racialized resources hypothesis, we examine heterogeneity in treatment effects for Blacks and Whites at each income tercile, using a triple interaction model (see Table P1). Contrary to the racialized resource hypothesis, the racial disparity persists at each income level. The effect for Blacks in the lowest income is large and statistically significant (-14 percentage points, s.e. = 4 pp). That effect drops by a statistically detectable magnitude among Whites (-7 pp, s.e. = 2 pp), representing a 50% decrease by race. In other words, the lowest-income defendants are doubly affected if they are Black. Moreover, in this analysis, income has no effect within racial groups: all the interaction terms for middle and high income are small and non-significant. Figure 3 illustrates this finding more clearly. In sum, the effect of income previously seen in Figure 2 may be mostly the result of race. Pretrial incarceration may not affect Black defendants because of their lower income, and Black defendants may be more affected than White

defendants whether or not they are poor.[12] We refrain from drawing stronger conclusions from this test, as our aggregated, zip-code income measure is more prone to measurement error.

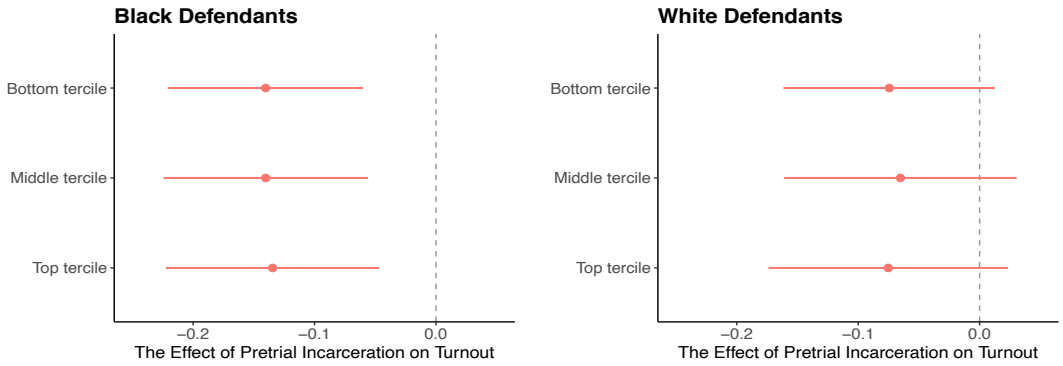

Figure 3. The Effect of Pretrial Incarceration on 2012 Turnout by Race-Income groups. Marginal effects for Black (left) and White defendants (right) based on the second-stage 2SLS estimates of models with interactions between pretrial incarceration, income, and race. 95% confidence intervals from models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

The strong effect on Black defendants may be driven by Blacks' higher baseline turnout (White 2019). Such higher turnout is evident in our data (Table Q1). If Blacks are affected because they are more likely to be voters, we would find equally large treatment effects for White and Black prior voters. However, a triple interaction model disconfirms this prediction (Table Q2). The effects are significantly more negative on Black than White prior voters ($p < 0.01$). They are also statistically different for Black than White non-voters ($p < 0.01$). Thus, the racially disparate effect

[12] The effect among Blacks and Whites is significantly different in the bottom and top terciles ($p < 0.01$), but not for the middle tercile ($p < 0.14$) (see Table P2). We caution against inferring that income is entirely irrelevant. This sample has much lower income than the Philadelphia population. In a sample with population levels of income variation, income may matter much more.

exists among those with the same voting history.[13] In sum, race substantially conditions the effect even when accounting for prior voting.[14]

Timing and the Role of Incapacitation

Next, we examine the temporal dynamics of the effect, as a preliminary exploration of incapacitation. As discussed above in Hypothesis 4, in Philadelphia, while pretrial detention and incarceration for a non-felony conviction do not automatically disenfranchise a defendant, they likely make it more difficult to cast a ballot. In addition, those incarcerated post-conviction for a felony conviction during the election are not allowed to vote.[15] If incapacitation explains the effect, then we may see a time gradient. We interact our instrument with *year-quarter* fixed effects, and plot the coefficients in Figure 4. As shown, the negative impact of pretrial incarceration is concentrated in the four quarters before Election Day.[16] This is consistent with the possibility that the impact of pretrial incarceration does not operate through long-term resource losses, or constant, fixed socialization. It is consistent with shorter-term factors, such as short-lived resource losses, decaying socialization, incapacitation, or election salience. That said, this analysis confounds

[13] In addition, while the -8 percentage point effect among Black non-voters is close to being distinguishable from zero ($p < 0.055$), the -4 percentage point effect among White non-voters is not ($p < 0.40$).

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

[15] Pretrial incapacitation is more prevalent than post-conviction incapacitation (5.2% v. 3.5%).

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

various time-varying and defendant-level covariates. It cannot isolate any one of them, determine if long-term factors indeed decay, or choose among potential shorter-term factors. It merely describes how the effect varies with case timing.

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

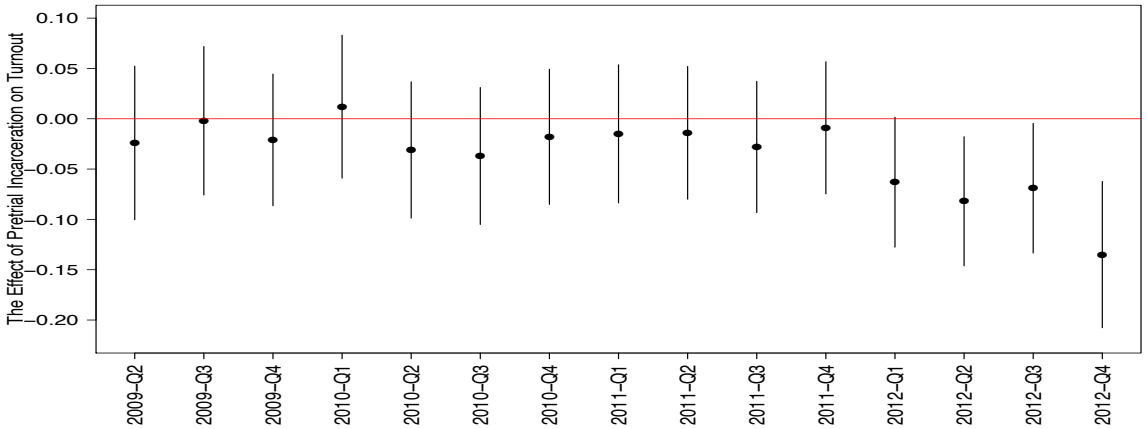

Figure 4. Dynamics of the Effect of Pretrial Detention on Turnout. We present the coefficients and the corresponding 95% confidence interval associated with the interaction between quarter-year and pretrial incarceration. Q1 of 2009 is the reference category. Model includes fixed effects, demographic and case-level covariates. Heteroskedastic-consistent standard errors are used to construct the confidence intervals.

As shown in Table 1 (row 1), the indirect effect of pretrial incarceration through incapacitation is large, but not statistically significant. Moreover, pretrial incarceration continues to exercise a substantial, precisely estimated direct effect (not accounted for by incapacitation). In other words, if incapacitation matters, we cannot detect that, and pretrial incarceration also matters in other ways. For cases closer to Election Day, Table 1 (row 2) shows roughly similar magnitudes,

but the smaller sample reduces precision, and selecting on timing introduces confounds, as noted above. In sum, the data do not offer definitive evidence about time-varying mechanisms or the role of incapacitation, but suggests future research might examine each.

	Mediation Analysis		
	Direct Effect (Pretrial Incarceration)	Indirect Effect (Incapacitation)	Total Effect
1. Nov. 2008 - Nov. 2012	-0.059	-0.049	-0.108
	(0.011)	(0.042)	(0.041)
% Incapacitated	8.7		
% Pretrial incarcerated	35.5		
N	100,821		
2. Nov. 2011 - Nov. 2012	-0.052	-0.071	-0.124
	(0.119)	(0.137)	(0.068)
% Incapacitated	15.7		
% Pretrial incarcerated	40.7		
N	33,500		

Table 1: Incapacitation Mechanism: The Mediated Effect of Pretrial Incarceration on 2012 Turnout. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Incapacitation takes 1 if a) the individual's case reached a disposition before the election, there was a minimum sentence of 1 day or more, and the estimated sentence release is after Election Day or b) their estimated pretrial release date after the preliminary arraignment hearing is past Election Day. Specifications include the instrument, case- and demographic-level covariates and fixed effects. Heteroscedasticity-consistent standard errors are presented within parentheses.

Conclusion

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

In this paper, we focused on two intertwined aspects of this shadow carceral state: the bail system and pretrial incarceration. Pretrial incarceration is a prevalent and iconic feature of the

shadow carceral state. Like much of the shadow carceral state, pretrial incarceration carries implications for social inequalities (Soss and Weaver 2017). Does this racially-disparate experience affect political behavior? To date, studies have not examined this question.

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

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

While pretrial incarceration – and perhaps other aspects of the shadow carceral state – have a demobilizing effect on voting, they may have mobilizing effects on other civic behaviors or attitudes (Walker 2019). If declining to vote is an act of active avoidance or even resistance, it may go hand in hand with oppositional collective consciousness (Weaver et al. 2019, 39). Whether the shadow carceral state mobilizes in some ways while it demobilizes in others is a question for further research.

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Online Appendix for *Jailed While Presumed Innocent: The Demobilizing Effects of Pretrial Incarceration*

November 11, 2021

A. Criminal Case Process

Figure A1 outlines the criminal case process in Philadelphia. For simplicity, we include only the stages that are most relevant to this study. After arrest, defendants are scheduled to appear at the first bail hearing, where the assigned magistrate reads the defendant the charges against them and sets bail conditions (see Stevenson 2018 for details).[1] In our study, if a defendant is unable to post bail within 3 days, we code them as "detained" and otherwise "released." Between arraignment, when formal charges are filed, and case disposition, defendants may plead guilty or be found guilty by a bench or jury trial determination ("convicted"). Alternatively, their case may be dismissed or they may be acquitted ("not convicted"). At the sentencing stage, in this diagram, "incarceration" refers to a minimum sentence of one or more days, and "Other Punishment" includes all other penalties (e.g. probation, community supervision, fines, etc.). As discussed in Appendix D, we lack data for some of these stages.

[Figure A1 about here.]

B. The "Managerial State"

The rise of pretrial incarceration is part of the turn by the courts from an "adjudicative" model toward a "managerial" model (Kohler-Hausmann 2018, 4-5). In the adjudicative model, courts apply established procedure to decide whether an individual committed a behavior defined by law as a crime, the level of blame the accused deserves, and the legally prescribed punishment. By contrast, in the managerial model, courts use more informal procedures: constructing records that handicap individuals for years and are difficult to amend; imposing procedural hassles with

[1] Pretrial conditions may be later modified, but we omit that possibility in this figure.

heavy costs; or assessing a person's criminal character by whether they comply with these procedural requirements. In the managerial model, punishment is often triggered by behavior that does not meet the legal definition of a crime. The person is not found guilty nor sentenced. Yet the courts impose substantial penalties, through mandatory fees, stigma, or the chain reaction of missed work, lost employment and income, relationship friction, and eviction.[2]

C. Additional Mechanisms

Misinformation: Pretrial detainees may be demobilized because of misinformation about voting eligibility, which is prevalent among individuals with conviction histories (Meredith and Morse 2015, 43). Some pretrial detainees may incorrectly believe that any time in jail is disenfranchising. We unable to test this mechanism, but it is consistent with our main hypothesis.

System Avoidance: Pretrial incarceration may decrease political participation by triggering "system avoidance" (Brayne 2014; Cohen 2010). Compared to those who were released pretrial, detainees who have experienced the severe costs of pretrial incarceration may be even more motivated to stay "below the radar" and avoid activities like voting that they may associate with an increased risk of scrutiny and legal entanglement (Lerman and Weaver 2014). We are unable to test this mechanism, but it is another reason for our main hypothesis.

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

D. Court Records Data

In this section, we provide additional details on the court records data from Philadelphia County, which is coterminous with the city of Philadelphia, the sixth-largest city in the US.

Race

Court records identify defendants as White, Black, Asian, Native American/Alaskan Native, or unknown/unreported. In all models that do not subset by race, we include all defendants, regardless of racial identification. We do not report estimates of the effects separately for defendants identified as Asian or Native American/Alaskan Native due to small sample sizes (less than 4,000 defendants), or for defendants whose race is not reported.[3]

We used Imai and Khanna's (2016) method to predict the probability a defendant identified as White in the court records is Hispanic based on their surname (and county for defendants with zip codes in Pennsylvania). The method uses the Census Bureau's Surname List and generates ethnicity predictions informed by the frequency with which a given surname is associated with particular ethnic identifications in census data. We then manually reviewed names with a predicted probability of Hispanic above 50%. Thus, to be classified as Hispanic for our analysis, an individual's surname must have a predicted probability above 50% and pass our manual

[3] Using surname prediction based on geolocation (Imai and Khanna 2016), we find that among defendants for whom race is not reported (18% of our sample), the predicted share of Whites is similar to the proportion of Whites among defendants with recorded race (26% vs. 32% respectively). Because surname methods predict nonwhite race imprecisely, we do not use them for those with unrecorded race nor subset by unrecorded race (Sood and Laohaprapanon 2018).

assessment.[4] This approach generates some false negatives: individuals who identify as Hispanic, but whose surname is not commonly associated with Hispanics.[5] Because it may have measurement error, we do not use the Hispanic variable in our main models, and instead control for race using the race variable from court records.

Prior Cases

The measure of prior cases captures only prior cases in Pennsylvania state courts. Federal charges are not included. The measure dates back to the 1970s, although we expect it is most reliable in the decade closest to the observation period, due to improvements in record-keeping.

Offense Severity

Offense severity affects pretrial determinations, and we control for it. We use the total Offense Gravity Score (OGS) of all charges in a case. Under Pennsylvania law, each criminal offense is assigned an OGS to indicate its severity (204 Pa. Code § 303.15.). Scores range from 1-14. Misdemeanors mostly have scores between 1-3, and felonies have scores from 4-14. For example, third-degree murder has an OGS of 14, narcotics acquisition by fraud ranges from 5-13 depending on the amount of narcotics, and disorderly conduct has an OGS of 1. The sample mean total OGS score is 10. We bin total OGS into terciles, with 3 and 11 as the cutpoints. The minimum number of cases per year and case severity tercile is 1,617 and the median is 11,736, (or 10,355

[4] As a robustness check, we use a higher threshold of 90%. With this classification, the sample of White Hispanic defendants declines by 12% (n=8,422). The estimate for White Hispanics decreases by almost half and the estimate for White Anglos declines to near-zero. The standard errors are similar. Using this threshold makes no substantive difference.

[5] This approach may misclassify as Anglo those with a non-Hispanic surname.

and 12,234 when we fold 2008 into 2009, since our sample only includes the last two months of 2008); thus, including offense severity in our analyses is unlikely render estimates sensitive to the inclusion of a particular observation.

Defendants with Multiple Cases

Most (67%) defendants in our sample have one case between 2008 and 2012, and 19% have two. For individuals with multiple cases, we include only their last case before Election Day. Among such individuals, their two most recent cases are far apart (315 days on average), and we expect the case closest to the election is most relevant for turnout.

Unobserved Variables

We do not observe whether or not someone was convicted; we can only infer conviction for those sentenced to incarceration (since we observe minimum and maximum sentence length). We also lack data on several dates in the lifecycle of a case (e.g. arraignment, trial, exact incarceration sentence start and end).

Pretrial Release Date

The granularity of our data allows us to code pretrial release date in great detail. For each case docket, if the bail type set at the preliminary arraignment hearing in the case is release on recognizance (ROR), nonmonetary or unsecured, then we assume that the defendant was released on the day of the preliminary arraignment hearing. If the bail type is monetary or nominal, we assume that the defendant was released at the time they posted bail or the time at which their bail was reduced to ROR, nonmonetary or unsecured. If there is no record in the case that the defendant posted bail or that their bail was reduced to ROR, nonmonetary or unsecured, then we assume that the person was detained until the case disposition date. If there is no bail action at the time of the first appearance and the case involved no felonies, we assume the case was diverted to a

community court, and the defendant was released on the day of their preliminary arraignment hearing. If such a case *did* involve a felony, we assume the defendant was denied bail at the first bail hearing and was detained either until release conditions were instated and subsequently met by the defendant, or if that did not occur, until case disposition.

E. Voter Turnout Data

In this section, we describe the voter files we use and how we validated their accuracy.

First, we use the April 2009 and February 2013 raw Pennsylvania voter files to measure, respectively, 2008 and 2012 turnout. We obtain these raw files from L2, which obtained them directly from the Pennsylvania Department of State and made no modifications to them. The April 2009 and February 2013 voter files are the first to contain complete information about turnout in the previous November elections. Because we use voter files soon after the 2008 and 2012 elections, concerns about voter purges are minimal (Gerber et al. 2017). These voter files also include records of absentee voters, which is important for our study: Pennsylvania residents who, at the time of the election, are incarcerated pretrial or on an exclusively misdemeanor sentence, detained on house arrest, or detained in a half-way house or other institutional facility are eligible to vote absentee.[6]

To confirm the accuracy of these Pennsylvania voter files, we compare them to official records. We find negligible differences in the number of registered voters in the April 2009 and

[6] Felony conviction and incarceration do not trigger removal from voting rolls in Pennsylvania (Cortes 2016, 3; American Civil Rights Union v. Philadelphia City Commissioners 2017). Although the L2 voter files do not contain records for overseas voters, we do not expect this population is relevant to our sample (Federal Voting Assistance Program 2014).

February 2013 Pennsylvania voter files and the official counts around that time.[7] In terms of vote counts, again we find small differences: within 0.2-1.2% of official vote counts. The raw February 2009 and April 2013 voter files record 5,923,958 and 5,730,469 votes in the 2008 and 2012 general elections, respectively, while the official numbers published on the Department of State website are 5,999,137 and 5,742,040. Thus, we expect our files provide accurate turnout data.

Second, we merge the court records of defendants who were not found in the 2013 Pennsylvania voter file with the voter files of the remaining 49 states and the District of Columbia, using the 2014 uniform voter files from L2. This merge allows us to recover the voting records of two smaller groups who would not appear in the Pennsylvania records: 1) those who moved out of state *before* the 2012 election and voted elsewhere; and 2) those who voted in Pennsylvania and moved out of state between November 2012-February 2013 (because these defendants may have been purged from the February 2013 Pennsylvania file we described above[8]). The 2014 files are the first snapshot after the 2012 election for all states and the District of Columbia. Unlike the raw files described above, the uniform files reflect L2's modifications, which use data that states may not incorporate as quickly. For example, since L2 keeps records of registered voters that have moved and reported their move via NCOA, L2 pulls those individuals from the voter file of their old state to the voter file of their new state as soon as their move is reported.

[7] For example, our file from February 2013 reports 8,210,700 registered voters and the Department of State website reported 8,217,005 in July 2013.

[8] This could occur if such defendants submitted a change of address, contacted the relevant officials to terminate their registration, or re-registered in their new state. If they *did not* take these steps, we expect they would still be in the February 2013 Pennsylvania voter file.

After these extensive merge procedures, defendants who voted in the 2012 election and died soon after is the one group that we are unable to find in the voter files, but we do not expect it is large enough to bias the estimates. First, the age distribution of our sample skews young: over half of defendants in our sample are under the age of 35 in 2012, and only 1% was over 65. Second, while researchers have found associations between incarceration and mortality in the short-term, the absolute number of deaths reported after release from Philadelphia jails represents a small fraction of our sample and is unlikely to account for the effects we observe.[9]

Finally, we collect additional voter registration data from the voter files as follows. For defendants who matched with the 2013 or 2014 voter files, we take their earliest date of voter registration recorded in those voter files. For defendants who we were not able to match using the 2013 or 2014 voter files but we were able to match using the 2009 voter file: we use the earliest date of registration recorded in their 2009 voter file record.

F. The Merge

We merged the court records from Philadelphia 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).[10] More specifically, we say that record $a \in \{1,\ldots, A\}$ in our court data is a potential match of record $b \in \{1,\ldots, B\}$ in the voter file if the estimated match probability ψ_{ab} is the largest among all pairs that involve record a. This

[9] According to Pennsylvania death records, 2% of the individuals released from the Philadelphia County jail system between 2010-2012 died within 5-7 years (Pizzicato 2018).

[10] Goel et al. (2020) found that fastLink is able to identify record matches with a high degree of precision, even in the presence of measurement error in the linkage fields.

procedure yields a one-to-one match for each record a with an estimated maximum match probability of $\zeta_a = \max_{b=1,\ldots,B} \psi_{ab}$. The merge follows two steps:

Step 1: We merge each Pennsylvania voter file (2009 and 2013) with our court data using first, middle, and last name, gender, and date of birth.[11] We convert date of birth to exact age as of Election Day 2012, which avoids comparison based on integers and it is equivalent to counting the number of days between two dates.

Step 2: For defendants not found in the 2013 Pennsylvania voter file, we merged the unmatched records with the 2014 voter files for all remaining states and D.C using the same variables listed in step 1.

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. For these string-valued variables, we used the Jaro-Winkler measure of similarity with the following thresholds: 0.85 and 0.94. Jaro-Winkler is a common string similarity measure that has proven to work extremely well with names. It is based on a character-wise comparison of two strings which puts more emphasis on their first four characters (see Enamorado et al. 2019 for more details). In the case of age, we use the absolute value of the difference (L1 norm) with the thresholds set at 6 months and 1 year of difference. In other words, we classify as agreements (partial agreements) in age those observations that are at most 6 months (1 year) apart. In the case of middle name and gender, we made comparisons based on whether they had an identical value or not. Based on these

[11] Using zip code as a linkage field does not substantively affect the results. We do not include zip code in order to avoid a non-match on zip code due to moving, which may be correlated with treatment.

comparisons, the probabilistic model behind fastLink assigns a probability of being a match for each pair of records. The intuition is simple: the more a pair of records agree in the linkage fields, the more likely that pair of records is a match.

In total, out of our final sample of 100,821, we matched 55,010 records (31,194 Blacks and 13,570 Whites), of which 1,577 matches came from the 2014 uniform nationwide voter files and the remainder from the merge with the 2013 Pennsylvania file. In the merge with the 2009 voter file to recover 2008 turnout, we find that out of the 91,605 defendants that were old enough to vote in 2008, we could match 54,739 (30,670 Blacks and 14,095 Whites) records with the 2009 voter file.[12] For defendants we do not find in any of the voter files, we assume they were not registered and did not vote. In our data, few defendants were too young to vote in the 2012 election (245) because the court records do not include those prosecuted in the juvenile criminal system.

[12] While our overall match rates are somewhat larger for Blacks (57% in 2009 and 58% in 2013) than Whites (53% in 2009 and 51% in 2013), the change from 2009 to 2013 in matching rates within race appears to have remained almost constant. Thus, it is unlikely that the racial heterogeneity we find in the treatment effects can be explained by differences in the matching rates across race. Moreover, the regression specifications that include all racial groups account for differences in match rates by incorporating our matching probabilities (as described in the section "Data on Pretrial Incarceration and Turnout") and the year fixed effects, further assuaging this concern. Specifically, because we reweight turnout and registration variables by match probabilities, merge-related confounders (e.g. name frequency, propensity for misspellings, etc.) that may vary across race are unlikely to explain the estimates of interest as they are accounted for (see Enamorado et al. 2019 for more details).

G. Descriptive Statistics

[Table G1 about here]

H. Instrument Validation Checks

Non-Monotonicity By Race

A magistrate may be more lenient than colleagues in cases involving White defendants or harsher than colleagues regarding Black defendants (Arnold et al. 2018, 1887). This non-monotonicity may bias estimates (Stevenson 2018). To test for this non-monotonicity, we construct the instrument separately for Black and White defendants based only on other defendants of the same race. The results in Table H1 suggest again that pretrial incarceration demobilizes Black defendants, though the estimate is less precise. As before, the effect on White defendants is small and imprecise.

We do not use this approach (or otherwise account for non-monotonicity by race) in the main analyses for several reasons. First, adding race to the instrument (so that magistrate leniency varies by case severity, by year, and by the four racial identifications in our data) considerably reduces the cell counts for leniency calculations and imbalances them due to the skewed racial composition of the sample.[13] This makes estimation highly sensitive. The smaller the grid, the greater the importance of the excluded data point in calculating the leave-out-case pretrial detention rates, which dwarfs the overall predictive power of the instrument: those not detained pretrial will always face a harsher magistrate, while those detained will always face a more lenient magistrate. Second, including race in the full sample also makes the estimates more sensitive specifically to the inclusion or exclusion of defendants whose race is not identified in the court

[13] See Table G1.

records, which is a form of missing data. Third, including race in the instrument would increase mechanical correlation between covariates, which could increase bias in the estimates.

[Table H1 about here.]

Instrument Variation

To illustrate the relationship between magistrate leniency and our variables of interest, Figure H1 presents the residualized distribution of the instrument (which betters capture leniency as we remove the variation explained by the fixed effects from our instrument, see Aizer and Doyle 2015 and Dobbie et al. 2018), and the non-parametric fit between the residualized instrument and residualized pretrial detention (left panel) and residualized turnout (right panel). Results are discussed in the main paper.

[Figure H1 about here]

Randomization Test

As noted above, if bail magistrate is as-if randomly assigned, case and defendant characteristics should be distributed evenly across magistrates with different decision tendencies and should not be predictive of the instrument. The first column of Table H2 examines whether such characteristics are significant predictors of whether a defendant is incarcerated pretrial, while the second column tests whether such characteristics are significant predictors of our instrument. Results are discussed in the main text.

[Table H2 about here]

To further test for potential violations of randomization, we construct a measure of predicted turnout using only the demographic and case-level covariates, so that all the variation in turnout is from these covariates. In Figure H2, we plot the distribution of the residualized magistrate instrument (leave-out-case pretrial detention rate) and the relationship between the

residualized instrument and the predicted turnout measure (red line). The flat line indicates no meaningful correlation between these measures (-0.003).

[Figure H2 about here]

In addition, to check that omitting interactions between fixed effects in the randomization process does not lead to any biases, we use alternative time fixed effects following Dobbie et al. (combined year-day-of-the-week, month-day-of-the-week, and day-of-the-week-shift), which does not alter our findings (see Table H3).

[Table H3 about here]

In addition to these randomization tests, we rely on Stevenson's (2018) validation of the random assignment process (pp. 516-519). This approach conforms to Dunning's design-based framework for validating natural experiments (Dunning 2012). Arrestees have no agency in selecting their bail magistrate, magistrates' shifts are randomly assigned to balanced schedules, and bail hearings are too brief to allow magistrates to affect turnout without pretrial incarceration. Furthermore, to alleviate concerns about monotonicity violations (that the instruments work differently for some subgroups), we follow Stevenson (2018) and Aizer and Doyle (2015), allowing our instrument to vary by time and case severity.

I. First Stage Results

Table I1 presents the first stage results associated with each analysis in Figures 1-3, and Table I2 present the first stage results for additional subsets (gender, defendants charged with different offense types). Column 1 includes only time and case severity fixed effects, column 2 adds demographic controls, and column 3 adds additional case controls**.** These results show that in all analyses our instrument has a strong positive correlation with pretrial incarceration and the

F-statistic is large. Thus, we do not expect our two-stage least squares estimates will suffer from weak instrument bias.

[Table I1 about here.]

[Table I2 about here.]

J. Two-Stage Least Squares Regressions: Effects of Pretrial Incarceration on Turnout Among All Defendants and by Income, Race, and Prior Case Status

To explore heterogeneity in treatment effects, we use an interaction model. We add to our main specifications interaction terms between the pretrial detention instrument and the variable of interest, e.g., income tercile. Specifically, the first stage equation is:

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

and in the second stage:

$$T_{d,2012} = \beta_0 + \beta_1 \widehat{P_{dtjh}} + (\widehat{P_{dtjh}} \times W_d)^\top \zeta + W_d^\top \xi + X_{dt}^\top \Lambda + \varepsilon_{dt}$$

where *d* indicates defendant, *j* is for magistrate, and *h* is the offense severity level. $T_{d,2012}$ is an indicator for voting in 2012, P_{dtjh} is an indicator for being detained pretrial more than 3 days, Z_{dtjh} is the instrument (leave-out-case pretrial detention rates), W_d is the variable we are interacting (W_d is a matrix when our variable of interest has many categories), and X_{dt} is a set of defendant and case covariates and fixed effects as described in Table I1.

Table J1, Column 3 reports these estimates, and Figures 1-3 shows the corresponding marginal effects. Columns 1 and 2 show models with fixed effects only and fixed effects plus demographic covariates, respectively. While the magnitude of the estimate varies across these models, the results are substantively consistent. In addition, the effects of pretrial incarceration on turnout are substantively similar to those from a subset analysis, but using the complete sample allows substantial precision gains.

[Table J1 about here]

To calculate the standard errors of the marginal effects presented in Figures 1 - 3 in the main text, we take advantage of the linear additive model and estimate them using the general formula for the variance of the sum of K random variables $\{C_1, C_2, \ldots, C_K\}$ i.e.,

$$Var\left(\sum_{i=1}^{K} C_i\right) = \sum_{i=1}^{K} Var(C_i) + \sum_{i \neq j} Cov(C_i, C_j)$$

where each C_i represents a coefficient of interest when calculating the marginal effects, and the square of this expression is our estimate of the standard error of the marginal effect.

Results presented in rows 1-5 (full sample, by race and by income) of Table J1 are discussed in the main paper. Here, we discuss whether the effects of pretrial incarceration are strongest among those undergoing their first arrest (no prior cases) in Pennsylvania, as those with prior cases may have already been demobilized (Gerber et al. 2017). The results, presented in row 6 of Table J1, are not consistent with this prediction. Pretrial incarceration has a significantly more negative effect on those with prior cases.

K. Bivariate Probit Regressions

Turnout and pretrial incarceration are dichotomous. Thus, we conduct a robustness check with bivariate probit regression (using the same instrument). Table K1 presents the average difference in predicted probabilities when moving pretrial incarceration from 0 to 1 and holding everything else constant, and the associated standard errors, which are calculated via non-parametric bootstrap (500 bootstrap samples). Similar to the 2SLS results in Table J1, the marginal effect of pretrial incarceration on turnout is -12 percentage points and statistically significant. The biprobit estimates are statistically indistinguishable from their counterparts in Table J1; the t-statistic is -0.18, p=0.86 (all defendants).

[Table K1 about here.]

L. OLS Regressions

Table L1 presents the estimated effect of pretrial incarceration on turnout using OLS regression. With OLS, pretrial incarceration is associated with a 7 to 11 percentage point decrease in the probability of voting in 2012. However, as noted, OLS estimates may be biased by the correlation between unobserved defendant characteristics and pretrial detention.

[Table L1 about here.]

M. Additional Robustness Checks

We assess the robustness of the main effects in Figure 1 and Table J1. Results for the full sample are presented in Table M1.

Residualized Instrument

We re-estimate the effects with an alternative instrument specification. Following Dobbie et al. (2018), we isolate the leniency of a magistrate by using the residuals from a regression of pretrial incarceration on the fixed effects for severity tercile, bail hearing year, month, day of the week, and shift, and conduct a leave-out-case analysis similar to our non-residualized instrument. We present these results in row 1, which show consistent results.

Excluding Years with Magistrate Vacancies

In this section, we assess potential sources of error from magistrate vacancies. In 2008-2009, two magistrates retired (in March 2008 and March 2009) and were not replaced until August 2009 and January 2010. This affects the analyses in two main ways. First, we observe fewer named

bail magistrates in this period: 4 rather than the usual 6.[14] Second, we observe a higher frequency of missing data for the bail magistrate in the case for this period.[15]

To address these concerns, we first assess whether observations with a named bail magistrate differ observably from those without by conducting balance tests on demographic and case covariates (see Table M3). While the observations with missing bail magistrate information differ on some demographic (gender, race), offense (any drug, violence, firearm offense) and bail hearing (day and shift) covariates, the magnitudes of these differences are substantively small. Next, we show that our results persist in the subset of cases in January 2010 onwards, a period with all 6 magistrates. This analysis effectively drops almost all cases of missing magistrates without selecting on missingness. The effects are substantively comparable to those from the full time period (Table M1, row 2).

[14] We observe 5 magistrates between November 2008-March 2009, 4 magistrates between April 2009-July 2009 and 5 magistrates between August 2009-December 2009.

[15] In fact, most (89%) of the 15,181 cases without a named bail magistrate in the data from 2008-2012 occur in between 2008-2009. Missing data for this field indicates that the bail hearings were likely conducted by one of multiple legal attorneys from the central court office who filled for the vacancies (Roberts, Gabriel, email communication, July 3, 2018). We exclude these cases from our main sample, as they do not allow us to calculate the magistrate leniency instrument. Excluding cases with missing data for the bail magistrate could introduce selection bias if missing bail magistrate information is not randomly distributed across cases.

Expanded Definition of "Violent" and "Property" Offenses

We obtain similar results using alternative measures of offense type. In row 3, we use expanded definitions of violent and property offense types. We add simple assault and sexual assault offenses (rape, statutory rape, aggravated indecent assault) to our measure of "any violent offense," which in all other specifications includes only murder and aggravated assault.[16] We also add possession of stolen property to our measure of "any property offense," which in other specifications includes burglary, shoplifting, robbery, motor vehicle theft, and other theft charges. The estimates are nearly identical to those reported in the main text.

Finer-grain Offense Covariates

In row 4, we use finer-grain offense and prior case covariates. Instead of the 5 aggregated offense categories (any drug, DUI, property, firearm, or violent offense), we add indicators for all offenses that appear in more than 5% of the sample (robbery, simple assault, aggravated assault, theft, marijuana possession, drug possession, drug sale, drug purchase, third degree felony firearm possession, possession of stolen property, vandalism, DUI 1st offense, trespassing, reckless endangerment). Additionally, along with the one covariate we used in other models to represent a defendant's prior contact with the courts (an indicator of whether or not the defendant had any prior case in Pennsylvania state courts), we add 3 more specific indicators: number of prior felony charges, number of prior convictions, and number of prior charges for violent offenses. This specification resembles Stevenson (2018) and is designed to increase the power of the instrument. As expected, the strength of the instrument increases (as measured by the first-stage F-test). The

[16] The percentage with any violent offense increases from 15% to 29% when including sexual assault and simple assault (a misdemeanor).

treatment effects remain substantively consistent (within 0.6 percentage points of those in the main text).

Different Measures of Pretrial Incarceration

In our main specification, we define pretrial incarceration as 3 or more days, following Dobbie et al. (2018) and Stevenson (2018), who note that a magistrate is more likely to have an impact on pretrial detention during the first 3 days after the bail hearing. After the 3-day period, defendants are more likely to begin petitioning for a reconsideration of their pretrial conditions, which are often reviewed by separate judges. In addition, advocacy groups use this cutpoint based on qualitative evidence that the pervasive effects of pretrial detention start after 3 days (Pretrial Justice Institute, 2015). Table M2 shows that if the cutpoint was changed to 5, 7, 10 or 14 days, our conclusions would remain unchanged. Table M2 also shows that when we use the logged number of days detained pretrial instead of binary cutpoints, the negative effect of pretrial incarceration remains. With the longer cutpoints and continuous measure, we find suggestive evidence that longer incarceration is associated with larger demobilizing effects on turnout.

[Table M1 about here.]
[Table M2 about here.]
[Table M3 about here.]

Placebo Test

Table M4 presents the results from a placebo test that examines whether treatment assignment in cases after the 2008 general election is associated with turnout in 2008. For this analysis, we exclude defendants too young to vote in the 2008 election. This test yields null effects as expected, further evidence for a causal rather than spurious effect of pretrial incarceration on turnout.

[Table M4 about here.]

Alternative Outcome: Voter Registration

We also evaluate the impact of pretrial incarceration on registration as an outcome. In this analysis, we focus only on defendants who were not registered pretreatment, because this subset is the only one for whom registration status can change post-treatment from not being registered pretreatment to being registered. Here, the registration outcome is a binary indicator of whether or not the defendant registered to vote before the 2012 general election. Table M5 presents the results. Pretrial incarceration decreases the likelihood that Blacks but not Whites registered to vote before the 2012 election, but neither effect can be distinguished from zero. Nor can these effects be distinguished from each other (p = 0.25 - 032).

[Table M5 about here.]

Permutation Test and Other Alternative Approaches to Estimating Standard Errors

To further assess the possibility that our approach to estimating standard errors (heteroskedasticity-robust) is not sufficiently conservative, we implement several alternative approaches. First, we conduct a permutation test (row 5 of Table M1). As in Stevenson (2018), we create 1,000 "false" work schedules for the magistrates. Each false schedule has a magistrate working for 5 days in a row on the same shift, and each magistrate only works one shift per 5-day period. For each false schedule, we re-calculate the 2SLS estimates and collect the t-statistic on the instrument for pretrial incarceration in the second stage. The empirical p-values are the fraction of false-schedule t-statistics for pretrial detention which are greater in absolute value than the t-statistic from the real data. Since we expect that any potential within-magistrate clustering of cases is due to correlation in cases across time, this technique can be thought of as a non-parametric way of clustering standard errors. (There are too few magistrates to effectively employ parametrically clustered standard errors.) The empirical p-values are similar to those estimated parametrically and

statistically significant at the usual levels, confirming that the estimated effect is unlikely to be due to random chance. [17]

Second, though we have few clusters at the experiment-level, we cluster our standard errors at the magistrate by year by case severity level (69 clusters) (for the full sample and by race and by income). We present this in Table M6. While the standard errors become slightly larger, our conclusions remain unchanged. Last, to increase the number of clusters, we also cluster at the magistrate by year by month by case severity level (Table M7). Regardless of the clustering choice, the substantive interpretation of our main findings remains as in the heteroskedastic robust standard errors case.

[Table M6 about here.]
[Table M7 about here.]

N. Deterministic Merge

When we instead use the deterministic approach to merge the court records and voter files and only count as a match those with exactly the same gender and exactly the same (or nearly identical) name and age, our match rate drops by only 5 points (to 50%), and resembles some similar studies (Gerber et al. 2017). Moreover, the main conclusions hold, as shown in Table N1. We prefer our probabilistic methodology as it accounts for more potential errors.

[Table N1 about here.]

O. Compliers

Following Dahl et al. (2014) and Dobbie et al. (2018), we characterize the fraction of compliers in our data and check their similarity to our complete sample of defendants. In this setting, *compliers* are defined as defendants in the same offense severity tercile whose pretrial

[17] Because the permutation test is computationally expensive, we do not use it extensively.

detention decision would have been different had their case been assigned to the strictest instead of the most lenient magistrate. Formally, the share of *compliers* can be calculated as: $Pr(Pretrial\ detention_d = 1|Z = z^*) - Pr(Pretrial\ detention_d = 1|Z = z_*) = \alpha_1(z^* - z_*)$, where z^* (z_*) is the less (most) lenient magistrate and α_1 is our first stage estimate from equation 2. *Always takers* are those defendants who would always be detained before trial regardless of the bail magistrate assigned to their case. In other words, $Pr(Pretrial\ detention_d = 1|Z = z_*) = \alpha_0 + \alpha_1 z_*$. Finally, *never takers* are defendants who would never be detained before trial $Pr(Pretrial\ detention_d = 1|Z = z^*) = 1 - \alpha_0 - \alpha_1 z^*$

Table O1 presents the share of *compliers*, *always takers*, and *never takers* across the different levels of case severity. Similarly to Dahl et al. (2014), within each level, z^* (z_*) is defined as the 99^{th} (1^{st}) percentile of our instrument. Across level of severity, the share that is *compliers* is estimated to lie between 9.7% and 12.4%. The estimated share of *always takers* (*never takers*) increases (decreases) as the level of severity of the offense(s) associated to a defendant's case increases. Altogether, these results show that the while the share of *compliers* is stable, there are clear patterns of heterogeneity across severity levels for *always takers* and *never takers*.

Table O2 compares the sample of compliers to the overall sample using the stratified approach to characterize *compliers* in Dahl et al. (2014) and Abadie (2003). While compliers are less likely to be charged with a drug or DUI offense, and slightly more likely to face charges classified as violent, firearm-related, and property, these differences are all minor (3 percentage points at most), and on age, gender, race, prior case status, turnout, and registration, compliers are not substantially different from the average defendant in our sample.

Tables O2-O4 compare compliers across subgroups on observed covariates, following Dahl et al. (2014). We present these results with the usual caution, as the method is suggestive (it describes

the relatively likelihood of observed covariates among compliers) and estimates become more susceptible to outliers as data are subset further. We find that:

1. Top income compliers are less likely to be Black, to have prior cases, and to face drug offenses (Table O2).
2. Black compliers are more likely to be in the bottom and middle income tercile, face firearm charges, register to vote pretreatment, and vote in 2008 and 2012 (Table O2).
3. Comparing White and Black compliers who did not vote in 2008, Blacks are less likely to be in the top income and more likely to have voted in 2012 (Table O3).
4. Comparing by race across income, Black compliers in the bottom tercile are more likely to face a drug charge and have a slightly lower severity score on average (Table O4).

[Table O1 about here.]
[Table O2 about here.]
[Table O3 about here.]
[Table O4 about here.]

P. Resource Deprivation By Race

Table P1 replicates the resource deprivation mechanism test by race. There are no apparent differences in the effect across income terciles for Black defendants nor for White defendants. However, the effects among White defendants of any income are smaller than those among Blacks.[18] These findings suggest that the demobilizing effects of pretrial incarceration may hold only for Blacks, regardless of income level.

[Table P1 about here.]

Q. Race and Pretreatment Turnout

[18] We do not disaggregate the sample of White defendants into Hispanic and Anglo by tercile because the resulting cell counts would be prohibitively small.

Prior studies have reported higher levels of pretreatment voter turnout among Black than White defendants, which could explain the concentrated impact of incarceration on Black defendants (White 2019). Table Q1 presents the results from a linear regression of 2008 turnout on race. We find that 2008 turnout is about 11 points higher among Black defendants than White defendants.

[Table Q1 about here]

We further assess whether Black detainees' greater likelihood of prior voting explains the larger effect of pretrial incarceration. Table Q2 presents the second-stage estimates of pretrial incarceration for Black and White non-voters in 2008 and voters in 2008. We omit defendants who were too young to vote in 2008. Results are discussed in the paper.

[Table Q2 about here]

R. Race and Pretreatment Registration Status

In the main text (see *Racially Disparate Impact* section) and in Appendix Q, we discuss the possibility that the demobilizing effects of pretrial incarceration are driven by Black defendants because they are more likely to be politically engaged previously, and we test this by examining effects by pretreatment turnout. Here, we use an alternative (though weaker) form of electoral engagement: registration. If we observe demobilizing effects concentrated among Black defendants because of higher levels of preexisting electoral engagement, we would expect to see similar demobilizing effects among both Blacks and Whites who were registered pretreatment, and null effects on both Blacks and Whites who were not registered pretreatment.

Table R1 presents the results by registration status. We pool White Anglo and White Hispanic for statistical power. Like in Appendix Q, we find larger effects on registered Black than White defendants, and the difference passes significance thresholds ($p < 0.001$ across

specifications). We also find null effects on both Blacks and Whites who were not registered pretreatment.

[Table R1 about here.]

S. Timing Before the Election

We assess the temporal dynamics of the effect, as a preliminary exploration of time-varying mechanisms. We use the timing of the case before the election and drop cases close to the election. We caution that case timing is not randomly assigned. Thus, we cannot isolate the effect of case timing from other time-correlated factors. If the pretrial effect is weaker when we drop cases nearer to the election, this could reflect incapacitation, or the decay of long-term factors, or the salience of the election, or other time-varying mechanisms.

With these caveats in mind, we re-run the analysis after dropping cases filed within 2, 4, 6, and 12 months before the election (respectively). This approach reduces the number exposed only to short-term factors such as Election-Day incarceration and election salience, without directly selecting on these variables. Results are shown in Table S1. Once we exclude defendants with cases within 2 months of the election, whose turnout is more likely to be affected by short-term factors, the effect of pretrial incarceration drops by about a third (to 6.8 points). As we continue to drop earlier cases, the estimate declines further, suggesting that pretrial incarceration is most demobilizing when it occurs close to the election. This is consistent with incapacitation, or decay, or election salience, or other time-correlated factors. It is also consistent with individual-level

covariates related to the timing of the case. Given the difficulties with this analysis, we do not put much inferential weight on it.[19]

[Table S1 about here.]

T. The Indirect Role of Incapacitation

Pretrial incarceration may reduce turnout because detainees may be held on Election Day (pretrial or post-conviction). Such a mediation effect would not weaken modeling assumptions or introduce bias. Instead, it is one possible mechanism for the effect of pretrial incarceration.[20] Below, we explain our measure and present the details of our mediation analysis. As reported in the paper, we find that incapacitation may play a role in explaining the effect of pretrial

[19] Timing results may also reflect sample selection. As previously discussed, our main sample includes one case per defendant, and for defendants with multiple cases, we include their last case before the election. Individuals with multiple cases are different from those with just one case (even when we account for the prior offense status). As a partial check, we re-run the temporal analysis in Figure 3 of the paper, controlling for more exhaustive pre-treatment prior case variables (such as the number of cases the defendant had in the time period), and find the same temporal trend. However, we do not claim that any period differences in Table S1 or Figure 3 represent a causal effect.

[20] We can rule out an additional incapacitation dynamic. Pretrial incarceration could increase the likelihood of future re-arrest, which could result in future pretrial and post-conviction incarceration during the election. However, our main analyses account for these dynamics by focusing only on defendants' last case in the time period (removing the possibility of pretrial or post-conviction incarceration in a subsequent case).

incarceration, but we cannot statistically distinguish that effect from zero, and moreover, pretrial incarceration continues to exercise a substantial direct effect (not accounted for by incapacitation).

Measuring Incapacitation: We operationalize incapacitation as likely incarceration on Election Day. It equals 1 if the individual's case reached a disposition before the election, there was a minimum sentence of 1 day or more, and the estimated sentence release is after Election Day. We subtract from the minimum sentence length the number of days the individual was detained pretrial following their preliminary arraignment hearing, because we assume defendants are likely to be given credit for time served pretrial towards their post-conviction sentence (Stevenson 2018). We calculate sentence release by adding the minimum sentence length to the disposition date. In addition, we code as being incarcerated on Election Day defendants whose estimated pretrial release date after the preliminary arraignment hearing is past Election Day. [21] By our measure, 8.7% of the sample is incapacitated. [22]

[21] We make the following additional assumptions in constructing this variable, following Stevenson (2018): a) defendants with multiple sentences serve them concurrently, and b) defendants are granted credit for time served only for the time spent detained following the preliminary arraignment hearing in the case (which is the only pretrial detention spell we consistently observe). Additionally, we do not account for parole or probation revocations or changes to pretrial release conditions that result in re-incarceration, except as noted above. This introduces some error in the mediator.

[22] We are unable to separately test pretrial and post-conviction incapacitation. Omitting one source of incapacitation from the indirect effect would mean that the direct effect captures the

We perform the following checks to ensure that we did not misclassify defendants who were *not* in fact incapacitated as incapacitated:

1. We pulled probation sentences for the cases coded as incapacitated pretrial. If the probation sentence date was before the election and probation was not revoked before the election (likely triggering re-incarceration), we code them as not incapacitated.
2. We reviewed all pretrial release conditions that were set in the case. If non-monetary bail was set before the election and was not revoked and replaced by monetary bail before Election Day, we code them as not incapacitated.[23]

Mediation Analysis: We use causal mediation analysis in an instrumental variable setting to more directly explore whether the effect of pretrial incarceration could be driven (in part) by incapacitation. We follow Dippel et al. (2020), using a single instrument to uncover the direct and indirect effect (through incapacitation) of pretrial incarceration on turnout. The key identifying assumption advanced by Dippel et al. (2020), *partial unconfoundedness*, requires that unobserved confounders that make incapacitation endogenous when explaining turnout (e.g., economic stability, or time-varying confounders related to it, like the diligence of good lawyers) are independent of unobserved confounders that make pretrial incarceration endogenous when

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omitted incapacitation pathway, resulting in biased estimates. Thus, our incapacitation measure combines them.

[23] Between steps 1 and 2, there were 689 cases that were mis-coded as incapacitated based on our original coding of pretrial release date. Since these cases represent only a small fraction (less than 1 percent) of our final sample, preserving the original coding does not change our mediation results.

explaining incapacitation (e.g., institutional factors such as a large backlog of cases, which delay judicial processes and makes those incapacitated on Election Day less likely to vote). It can be argued that defendants' economic stability is unlikely to be correlated with institutional factors (not in the hands of defendants or their lawyers) related to the timing of incapacitation on Election Day, meeting the partial unconfoundedness assumption.

Figure T1 illustrates the direct (red) and indirect (green + blue) paths we seek to estimate. To uncover these direct and indirect effects, we estimate the two 2SLS regressions, in Table T1. The partial unconfoundedness assumption allows identification using two sequential steps. First, we use our instrument to identify the effect of pretrial detention on incapacitation (green line in Figure T1, and the first 2SLS regression of Table T1). Then, conditional on that first effect, we isolate the effect of incapacitation on turnout (blue line in Figure T1, and the second 2SLS regression of Table T1). In a nutshell, our instrument induces a shock in incapacitation that purges incapacitation from its endogeneity when explaining turnout. Of course, if there are unobserved confounders that jointly determine pretrial incarceration, incapacitation, and turnout, then we cannot separate the direct effect and the indirect effect (through incapacitation) of pretrial incarceration on turnout.

[Figure T1 about here.]
[Table T1 about here.]

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Tables and Figures for Online Appendix

A. Criminal Case Process

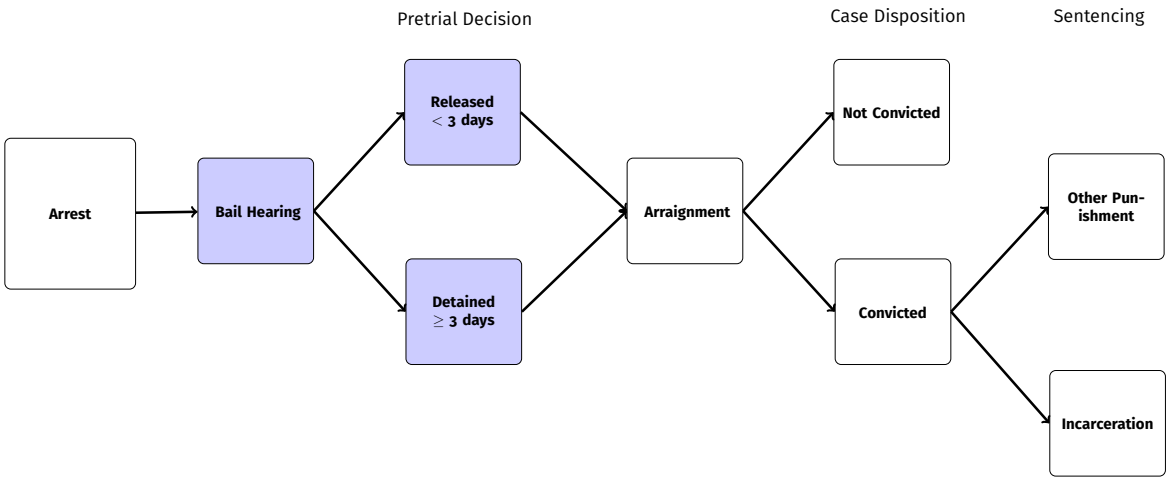

Figure A1: **Criminal Case Process.** See Appendix A.

G. Descriptive Statistics

	Full Sample		Detained		Released	
	mean	s.d	mean	s.d.	mean	s.d.
Pretrial incarceration:						
Detained $>$ 3 days	0.36	0.48	1		0	
Detained 1 year	0.04	0.20	0.12	0.32	0	
Total days detained	56.62	160.04	158.99	236.31	0.22	0.58
Demographic:						
Age (years)	35.71	11.85	34.80	11.39	36.21	12.07
Female	0.19	0.39	0.12	0.32	0.23	0.42
Race:						
Black	0.53	0.50	0.63	0.48	0.48	0.50
White	0.27	0.44	0.26	0.44	0.27	0.44
White Anglo	0.17	0.38	0.14	0.35	0.19	0.39
White Hispanic	0.09	0.29	0.12	0.32	0.08	0.28
Other	0.02	0.15	0.02	0.15	0.02	0.16
Not reported	0.18	0.38	0.09	0.28	0.23	0.42
Zip code average income:						
Bottom tercile ($<$ \$25,888)	0.30	0.46	0.33	0.47	0.28	0.45
Middle tercile (\$25,888-\$34,090)	0.30	0.46	0.30	0.46	0.30	0.46
Top tercile ($>$\$34,090)	0.29	0.45	0.25	0.43	0.31	0.46
Case-related:						
Any drug offense	0.36	0.48	0.27	0.45	0.41	0.49
Any violent offense	0.15	0.35	0.24	0.43	0.09	0.29
Any violent offense (expanded)	0.29	0.45	0.42	0.49	0.22	0.41
Any firearm offense	0.08	0.27	0.18	0.38	0.03	0.16
Any property offense	0.20	0.40	0.29	0.46	0.15	0.36
Any property offense (expanded)	0.21	0.41	0.31	0.46	0.16	0.36
Any DUI offense	0.11	0.31	0.04	0.19	0.15	0.36
Offense Gravity Score (OGS) tercile	1.95	0.82	2.43	0.68	1.68	0.77
OGS less than 3 (tercile 1)	0.36	0.48	0.11	0.31	0.50	0.50
OGS between 4 and 11 (tercile 2)	0.32	0.47	0.35	0.48	0.31	0.46
OGS more than 12 (tercile 3)	0.31	0.46	0.54	0.50	0.19	0.39
Misdemeanor charge only	0.48	0.50	0.19	0.39	0.63	0.48
Any prior case	0.73	0.45	0.84	0.36	0.66	0.47
# prior cases with felony charges	1.26	1.97	1.97	2.37	0.87	1.58
# prior cases with violent charges	0.89	1.70	1.35	2.07	0.63	1.38
# prior convictions	1.57	2.39	2.22	2.76	1.22	2.07
First bail amount (\$)	31,804	354,511	83,131	591,673	3,620	14,919
Electoral:						
Pretreatment Turnout in 2008	0.28	0.40	0.23	0.37	0.31	0.41
Post-teatment Turnout in 2012	0.28	0.40	0.21	0.36	0.32	0.41
Voting-age-ineligible in 2008	0.09	0.29	0.11	0.31	0.08	0.27
Pretreatment registration	0.53	0.41	0.50	0.41	0.54	0.40
N	100,821		35,819		65,002	

Table G1: **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. "Other" consists of Asian or Native American/Alaskan Native defendants. "White Anglo" and "White Hispanic" are based on surname prediction (see Appendix D). "Any property offense" includes motor vehicle theft, burglary, shoplifting, robbery and other theft charges, whereas the expanded version also includes possession of stolen property. "Any violent offense" includes aggravated assault and murder charges, whereas the expanded version includes simple assault, rape, statutory rape, and aggravated indecent assault. "Pretreatment registration" is an indicator of whether or not a defendant was registered to vote before their bail hearing. "Voting-age-ineligible in 2008" is an indicator of whether or not a defendant was younger than 18 on the day of the 2008 general election.

H. Instrument Validation Checks

	Second Stage Estimates		
	(1)	(2)	(3)
Black	−0.163	−0.125	−0.123
	(0.069)	(0.061)	(0.058)
First Stage F-test	190.009	186.054	210.178
N	53,601		
White	0.031	0.036	0.036
	(0.080)	(0.069)	(0.069)
First Stage F-test	101.871	104.008	107.515
N	26,846		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table H1: **The Effect of Pretrial Incarceration on 2012 Turnout by Race with Instrument Constructed Separately by Race.** Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1, except the instrument is constructed separately for Black defendants based only on other cases involving Black defendants and vice versa for White defendants. Heteroscedasticity-consistent standard errors are presented within parentheses.

	Randomization Test	
	Pretrial Incarceration	Magistrate Leniency
Demographic:		
Age	0.00531	−0.00008
	(0.00074)	(0.00006)
Age^2	−0.00007	0.00000
	(0.00001)	(0.00000)
Female	−0.12742	−0.00065
	(0.00326)	(0.00026)
Race:		
White	−0.02541	−0.00040
	(0.00320)	(0.00025)
Other	−0.04845	−0.00073
	(0.00853)	(0.00065)
Not reported	−0.09298	−0.00009
	(0.00386)	(0.00031)
Case-related:		
Any drug offense	−0.03057	0.00067
	(0.00346)	(0.00026)
Any violent offense	0.03798	−0.00032
	(0.00482)	(0.00033)
Any firearm offense	0.21491	−0.00089
	(0.00509)	(0.00038)
Any property offense	0.02731	−0.00048
	(0.00401)	(0.00029)
Any DUI offense	−0.02593	−0.00012
	(0.00416)	(0.00041)
Any prior case	0.16411	0.00039
	(0.00330)	(0.00028)
Electoral:		
Pretreatment turnout, 2008	−0.08981	0.00037
	(0.00391)	(0.00031)
Voting-age-ineligible in 2008	0.00213	0.00044
	(0.00553)	(0.00044)
Pretreatment registration	−0.01017	−0.00055
	(0.00395)	(0.00030)
Joint F-test	733.01	3.320
Fixed Effects	✔	✔

Table H2: **Randomization Test for Pretrial Incarceration and Magistrate Leniency.** The estimates and their corresponding heteroscedasticity-consistent standard errors 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). Pretrial incarceration is 1 if detained for more than 3 days pretrial and 0 otherwise. Magistrate leniency instrument: leave-out case mean pretrial incarceration rate. Fixed effects: bail hearing year, month, day of the week, and shift, and case severity tercile indicators.

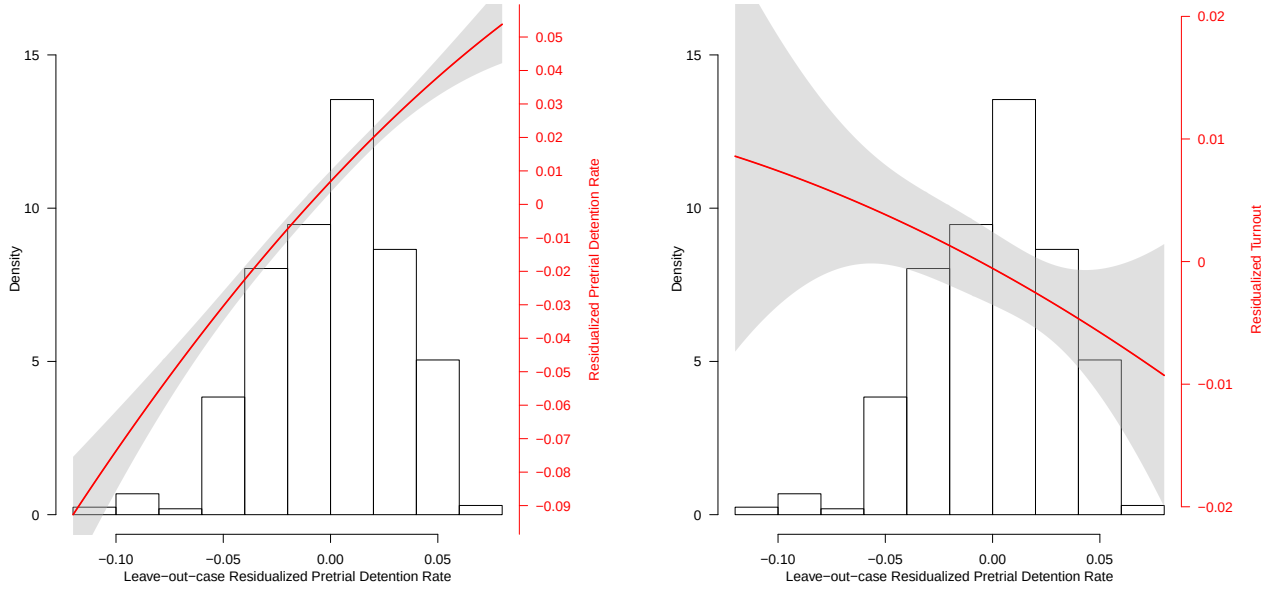

Figure H1: **Distribution of our Residualized Magistrate Leniency Instrument against Residualized Pretrial Incarceration (left) and Residualized Turnout (right).**

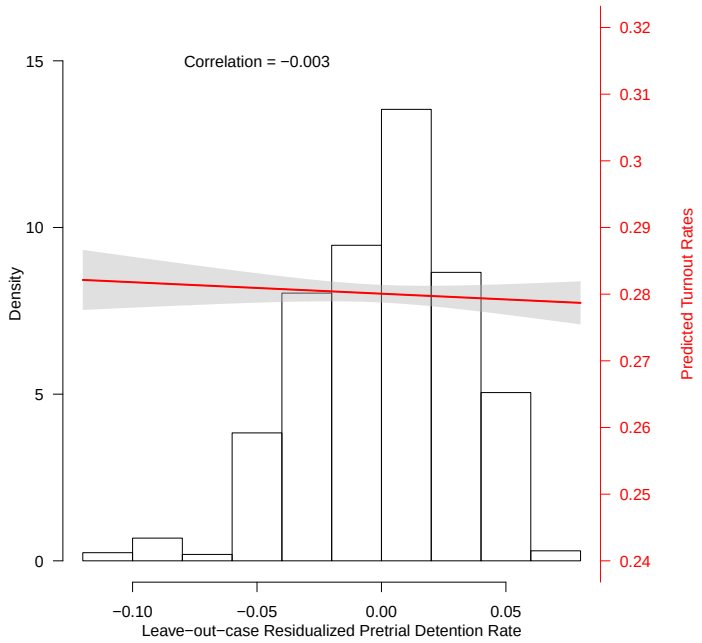

Figure H2: **Relationship Between Residualized Magistrate Leniency Instrument and Predicted Turnout.** Predicted turnout is based exclusively on demographic and case-level covariates.

	Randomization Test	
	Pretrial Incarceration	Magistrate Leniency
Demographic:		
Age	0.00531	−0.00008
	(0.00074)	(0.00006)
Age^2	−0.00007	0.00000
	(0.00001)	(0.00000)
Female	−0.12732	−0.00057
	(0.00326)	(0.00026)
Race:		
White	−0.02553	−0.00038
	(0.00320)	(0.00025)
Other	−0.04829	−0.00078
	(0.00853)	(0.00065)
Not reported	−0.09328	−0.00010
	(0.00386)	(0.00031)
Case-related:		
Any drug offense	−0.03000	0.00060
	(0.00346)	(0.00026)
Any violent offense	0.03792	−0.00044
	(0.00482)	(0.00033)
Any firearm offense	0.21456	−0.00090
	(0.00509)	(0.00038)
Any property offense	0.02743	−0.00048
	(0.00401)	(0.00029)
Any DUI offense	−0.02467	−0.00026
	(0.00417)	(0.00041)
Any prior case	0.16393	0.00039
	(0.00330)	(0.00028)
Electoral:		
Pretreatment turnout, 2008	−0.08982	0.00031
	(0.00391)	(0.00030)
Voting-age-ineligible in 2008	0.00226	0.00041
	(0.00553)	(0.00044)
Pretreatment registration	−0.01015	−0.00052
	(0.00395)	(0.00030)
Joint F-test	731.540	3.163
Fixed Effects	✔	✔

Table H3: **Randomization Test for Pretrial Incarceration and Magistrate Leniency (Combined Fixed Effects).** The estimates and their corresponding heteroscedasticity-consistent standard errors 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). Pretrial incarceration is 1 if detained for more than 3 days pretrial and 0 otherwise. Magistrate leniency instrument: leave-out case mean pretrial incarceration rate. Fixed effects: bail hearing year-day-of-the-week, month-day-of-the-week, and day-of-the-week-shift, and case severity tercile indicators.

I. First Stage Results

	First Stage Estimates		
	(1)	(2)	(3)
1. Main Result:			
All defendants	0.817	0.794	0.802
	(0.040)	(0.039)	(0.038)
First Stage F-test	421.944	420.538	448.211
N	100,821		
2. Resource Deprivation:			
Bottom tercile	0.896	0.883	0.907
	(0.075)	(0.073)	(0.071)
First Stage F-test	141.716	145.801	161.259
N	29,976		
Middle tercile	0.773	0.741	0.750
	(0.073)	(0.071)	(0.070)
First Stage F-test	111.469	108.124	115.383
N	30,384		
Top tercile	0.811	0.790	0.784
	(0.071)	(0.069)	(0.068)
First Stage F-test	129.682	129.488	133.236
N	29,236		
3. Racially Disparate Impact:			
Black	0.824	0.806	0.835
	(0.055)	(0.055)	(0.053)
First Stage F-test	221.257	218.772	246.518
N	53,598		
White (All)	0.855	0.869	0.866
	(0.075)	(0.074)	(0.073)
First Stage F-test	130.362	137.658	140.913
N	26,843		
4. Prior Case Status:			
Prior case	0.884	0.875	0.893
	(0.047)	(0.046)	(0.046)
First Stage F-test	353.289	355.201	377.774
N	73,197		
No prior case	0.502	0.460	0.470
	(0.067)	(0.065)	(0.064)
First Stage F-test	56.965	50.167	54.607
N	27,624		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table I1: **The Effect of Magistrate Leniency Instrument on Pretrial Incarceration Among All Defendants and by Income, Race and Prior Case Status.** Pretrial incarceration is coded as 1 if detained pretrial for more than 3 days and 0 otherwise. Fixed effects: bail hearing year, month, day of the week, and shift, and case severity tercile. Demographic covariates: age, age^2, gender, race, pretreatment turnout (in 2008), voting-age-ineligible in 2008, and pretreatment registration, except race is omitted in analyses that subset by race (row 3). Case covariates: any drug, DUI, violent, firearm, and property offenses, and prior case status, except prior case status is omitted as a covariate in analyses that subset by prior case (row 4). Heteroscedasticity-consistent standard errors are presented within parentheses.

	First Stage Estimates		
	(1)	(2)	(3)
1. Gender:			
Male	0.806	0.806	0.820
	(0.044)	(0.044)	(0.043)
First Stage F-test	331.936	340.816	368.935
N	81,175		
Female	0.720	0.710	0.709
	(0.081)	(0.080)	(0.079)
First Stage F-test	78.255	78.07	80.300
N	19,646		
2. Offense Type:			
Any drug offense	0.899	0.916	0.919
	(0.058)	(0.057)	(0.056)
First Stage F-test	243.990	258.570	267.404
N	36,356		
Any violent offense	0.658	0.624	0.676
	(0.155)	(0.144)	(0.136)
First Stage F-test	18.113	18.822	24.773
N	14,638		
Any firearm offense	0.262	0.340	0.318
	(0.168)	(0.159)	(0.154)
First Stage F-test	2.427	4.554	4.253
N	8,090		
Any DUI offense	0.527	0.537	0.525
	(0.093)	(0.092)	(0.091)
First Stage F-test	31.881	33.876	33.420
N	11,206		
Any property offense	0.920	0.940	0.976
	(0.107)	(0.102)	(0.097)
First Stage F-test	73.534	85.506	101.320
N	20,142		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table I2: **The Effect of Magistrate Leniency on Pretrial Incarceration By Gender and Offense Types.** Pretrial incarceration is coded as 1 if an individual was detained pretrial for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1, with exceptions. In row 1, gender is used to subset the data and is omitted as a covariate. In row 2, offense indicators are used to subset the data and omitted in relevant analyses (e.g. "Any drug offense" includes the subset of defendants charged with drug offenses and includes all offense indicators noted in Table I1 as covariates except "any drug offense"). Heteroscedasticity-consistent standard errors are presented within parentheses.

J. Two Stage Least Squares Regressions: Effects of Pretrial Incarceration on Turnout Among All Defendants and by Income, Race, and Prior Case Status

	Second Stage Estimates		
	(1)	(2)	(3)
1. Main Result:			
Pretrial Incarceration (PTI)	−0.130	−0.108	−0.108
	(0.048)	(0.041)	(0.041)
First Stage F-test	421.944	420.538	448.211
N	100,821		
2. Racially Disparate Impact:			
PTI (Black as baseline)	−0.185	−0.136	−0.133
	(0.044)	(0.039)	(0.039)
First Stage F-test	485.427	444.626	378.854
PTI × White (Anglo & Hispanic)	0.111	0.076	0.071
	(0.014)	(0.013)	(0.012)
First Stage F-test	1624.849	1591.137	1660.799
N	100,821		
3. Racially Disparate Impact (disaggregated):			
PTI (Black as baseline)	−0.185	−0.136	−0.133
	(0.044)	(0.038)	(0.039)
First Stage F-test	392.579	362.108	305.144
PTI × White Anglo	0.123	0.077	0.070
	(0.019)	(0.017)	(0.015)
First Stage F-test	704.204	690.809	709.632
PTI × Hispanic	0.092	0.064	0.062
	(0.019)	(0.016)	(0.016)
First Stage F-test	521.928	534.653	539.537
N	100,821		
4. Resource Deprivation:			
PTI (Bottom tercile as baseline)	−0.151	−0.120	−0.121
	(0.048)	(0.041)	(0.041)
First Stage F-test	160.421	162.167	166.610
PTI × Middle tercile	−0.024	−0.012	−0.008
	(0.016)	(0.013)	(0.013)
First Stage F-test	2725.226	2784.793	2848.166
PTI × Top tercile	0.073	0.030	0.034
	(0.017)	(0.015)	(0.015)
First Stage F-test	2167.546	2213.764	2288.696
N	89,596		
5. Resource Deprivation (different baseline group):			
PTI (Middle tercile as baseline)	−0.174	−0.132	−0.129
	(0.052)	(0.045)	(0.045)
PTI × Top tercile	0.097	0.042	0.042
	(0.016)	(0.014)	(0.014)
N	89,596		
6. Prior Case Status:			
PTI (No prior case as baseline)	−0.046	−0.037	−0.038
	(0.071)	(0.061)	(0.058)
First Stage F-test	704.699	697.34	649.797
PTI × Prior case	−0.097	−0.080	−0.079
	(0.029)	(0.025)	(0.023)
First Stage F-test	12606.164	11747.912	10623.517
N	100,821		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table J1: **The Effect of Pretrial Incarceration on 2012 Turnout Among All Defendants and By Income, Race and Prior Case.** "PTI" is the main effect of pretrial incarceration for the baseline category. "PTI × ..." is the interaction term. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Each of the analyses presented in this table include interactions between the covariates of interest (income, race, prior case) and pretrial incarceration. In addition, we interact the instrument with each covariate of interest. Heteroskedastic-consistent standard errors are presented within parentheses.

K. Bivariate Probit Regressions

	Bivariate Probit Estimates
1. Main Result:	
Pretrial Incarceration (PTI)	−0.100
	(0.009)
N	100,821
Instrument + Fixed Effects	✔
Demographic Covariates	✔
Case Covariates	✔

Table K1: **Average Marginal Effect: The Effect of Pretrial Incarceration on 2012 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 shift, and case severity tercile. Demographic covariates: age, age^2, gender, race, pretreatment turnout (in 2008), voting-age-ineligible in 2008, and pretreatment registration. Case covariates: any drug, DUI, violent, firearm, and property offense, and prior case status. Standard errors in parentheses obtained via 500 bootstrap samples.

L. OLS Regressions

	OLS Estimates		
	(1)	(2)	(3)
1. Main Result:			
Pretrial Incarceration (PTI)	−0.114	−0.074	−0.069
	(0.003)	(0.003)	(0.003)
N	100,821		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table L1: **OLS Estimated Effect of Pretrial Incarceration on 2012 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 shift, and case severity tercile. Demographic covariates: age, age^2, gender, race, pretreatment turnout (in 2008), voting-age-ineligible in 2008, and pretreatment registration. Case covariates: any drug, DUI, violent, firearm, and property offense, and prior case status. Heteroscedasticity-consistent standard errors are in parentheses.

M. Additional Robustness Checks

	Second Stage Estimates		
	(1)	(2)	(3)
1. Residualized Instrument			
Pretrial Incarceration (PTI)	−0.132	−0.111	−0.111
	(0.048)	(0.041)	(0.041)
First Stage F-test	416.361	415.172	443.308
N	100,821		
2. Excluding Years with Magistrate Vacancies (2008-2009):			
Pretrial Incarceration (PTI)	−0.199	−0.165	−0.162
	(0.058)	(0.050)	(0.051)
First Stage F-test	304.100	298.779	303.280
N	80,936		
3. Expanded Definition of "Violent" and "Property" Offenses:			
Pretrial Incarceration (PTI)			−0.106
			(0.041)
First Stage F-test			448.016
N	100,821		
4. Finer-Grain Offense Covariates:			
Pretrial Incarceration (PTI)			−0.103
			(0.041)
First Stage F-test			426.610
N	100,820		
5. Permutation Test			
Pretrial Incarceration (PTI)	−0.130	−0.108	−0.108
	(0.048)	(0.041)	(0.041)
p-value	0.000	0.000	0.000
N	100,821		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table M1: **Robustness Checks: The Effect of Pretrial Incarceration on 2012 Turnout.** Instrument refers to the leave-out case mean pretrial incarceration rate (equation 1), except in row 1 which uses the residualized version of the instrument. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Covariates and fixed effects are described in the notes to Tables I1, with the following exceptions. In row 3, case covariates include indicators for: any drug-related, DUI, violent, firearm, and property offenses, where "any violent offense" includes simple and sexual assault offenses (rape, statutory rape, aggravated indecent assault) in addition to murder and aggravated assault, and "any property offense" includes possession of stolen property in addition to burglary, shoplifting, robbery, motor vehicle theft, and other theft charge. In row 4, case covariates include indicators for any robbery, simple assault, aggravated assault, theft, marijuana possession, drug possession, drug sale, drug purchase, third degree felony firearm possession, possession of stolen property, vandalism, DUI 1st offense, trespassing, reckless endangerment, any prior case, number of prior felony charges, number of prior convictions, and number of prior charges for violent offenses. Heteroscedasticity-consistent standard errors are presented within parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
1. Pretrial incarceration: 5+ days			
Pretrial Incarceration (PTI)	−0.135	−0.116	−0.116
	(0.049)	(0.042)	(0.042)
First-stage F-stat	404.347	403.220	432.144
N	100,821		
2. Pretrial incarceration: 7+ days			
Pretrial Incarceration (PTI)	−0.158	−0.143	−0.143
	(0.050)	(0.044)	(0.043)
First-stage F-stat	381.928	375.965	404.216
N	100,821		
3. Pretrial incarceration: 10+ days			
Pretrial Incarceration (PTI)	−0.175	−0.163	−0.164
	(0.051)	(0.046)	(0.045)
First-stage F-stat	354.584	344.644	375.247
N	100,821		
4. Pretrial incarceration: 14+ days			
Pretrial Incarceration (PTI)	−0.187	−0.180	−0.182
	(0.055)	(0.050)	(0.049)
First Stage F-test	305.020	296.199	323.871
N	100,821		
5. Pretrial incarceration: continuous, logged number of days			
Pretrial Incarceration (PTI)	−0.039	−0.036	−0.036
	(0.011)	(0.010)	(0.010)
First Stage F-test	355.879	351.907	397.637
N	100,821		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table M2: **The Effect of Pretrial Incarceration on 2012 Turnout Using Different Cutpoints for Pretrial Incarceration.** Pretrial incarceration is coded as 1 if detained for more days than the corresponding cutpoint and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Heteroskedastic-consistent standard errors are presented within parentheses.

	Balance on Selected Covariates			
	Named Magistrate	Unnamed Magistrate	Diff.	p-value
Demographic:				
Age	35.71	35.80	0.09	0.36
Male	0.81	0.83	0.02	0.00
Race				
Black	0.53	0.58	0.05	0.00
White	0.27	0.29	0.02	0.00
Other	0.02	0.02	0.00	0.18
Not reported	0.18	0.11	−0.07	0.00
Case-related:				
Any drug offense	0.36	0.41	0.05	0.00
Any violent offense	0.15	0.13	−0.02	0.00
Any firearm offense	0.08	0.08	0.00	0.22
Any property offense	0.20	0.21	0.01	0.00
Any DUI offense	0.11	0.09	−0.02	0.00
Offense Gravity Score	10.04	9.62	−0.42	0.00
Any prior case	0.73	0.76	0.03	0.00
Day of week:				
Weekday	0.68	0.71	0.03	0.00
Weekend	0.32	0.29	−0.03	0.00
Shift:				
Morning	0.31	0.30	−0.01	0.04
Afternoon	0.36	0.40	0.04	0.00
Evening	0.34	0.30	−0.04	0.00
N	100,821	15,181		

Table M3: **Balance on Selected Covariates between Observations With and Without Named Magistrate.** p-values are adjusted for multiple testing using Benjamini and Hochberg (1995). Day of week and Shift refer to the bail hearing.

	Second Stage Estimates		
	(1)	(2)	(3)
1. Main Result:			
Pretrial Incarceration (PTI)	0.004	0.007	0.009
	(0.052)	(0.054)	(0.051)
First Stage F-test	383.489	380.059	408.837
N	91,605		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table M4: **Placebo Test: The Effect of Pretrial Incarceration on 2008 Turnout.** Sample includes defendants age 18 and older at the time of the 2008 election. Outcome: turnout in 2008. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1, except electoral covariates are omitted (voting-age-ineligible in 2008, pretreatment registration, pretreatment turnout). Heteroscedasticity-consistent standard errors are presented within parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
1. Main Result:			
Pretrial Incarceration (PTI)	−0.169	−0.153	−0.148
	(0.087)	(0.089)	(0.086)
First Stage F-test	202.665	198.511	219.369
N	39,751		
2. Racial Disparate Impact:			
PTI (Black as baseline)	−0.173	−0.145	−0.141
	(0.079)	(0.081)	(0.081)
First Stage F-test	186.452	173.294	153.295
PTI × White	−0.031	−0.029	−0.026
	(0.027)	(0.028)	(0.026)
First Stage F-test	883.952	855.461	884.098
N	39,751		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table M5: **Alternative Outcome: The Effect of Pretrial Incarceration on Registration Among Defendants Not Registered To Vote Before Focal Case.** "PTI" is the main effect of pretrial incarceration for the baseline category. "PTI × ..." is the interaction term. Sample includes defendants who were age 18 and older at the time of the 2008 election and not registered to vote pretreatment. Outcome: registered to vote before the 2012 general election. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1 with exceptions. Pretreatment registration is used to construct the samples and omitted as a covariate in all rows. Pretreatment turnout is omitted in all rows because the sample only includes defendants who were not registered to vote pretreatment. Heteroscedasticity-consistent standard errors are presented within parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
1. Main Result:			
Pretrial Incarceration (PTI)	−0.130	−0.108	−0.108
	(0.050)	(0.047)	(0.047)
N	100,581		
2. Racially Disparate Impact:			
PTI (Black as baseline)	−0.185	−0.136	−0.133
	(0.047)	(0.043)	(0.044)
PTI × White (Anglo & Hispanic)	0.111	0.076	0.071
	(0.016)	(0.014)	(0.012)
N	100,821		
3. Resource Deprivation:			
PTI (Bottom tercile as baseline)	−0.151	−0.120	−0.121
	(0.050)	(0.049)	(0.049)
PTI × Middle tercile	−0.024	−0.012	−0.008
	(0.015)	(0.014)	(0.014)
PTI × Top tercile	0.073	0.030	0.034
	(0.015)	(0.012)	(0.011)
N	89,596		
4. Resource Deprivation (different baseline group):			
PTI (Middle tercile as baseline)	−0.174	−0.132	−0.129
	(0.054)	(0.052)	(0.053)
PTI × Top tercile	0.097	0.042	0.042
	(0.015)	(0.013)	(0.013)
N	89,596		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table M6: **The Effect of Pretrial Incarceration on 2012 Turnout Among All Defendants and By Race and Income (Clustered Std. Errors by magistrate × year × case severity).** "PTI" is the main effect of pretrial incarceration for the baseline category. "PTI × ..." is the interaction term. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Standard errors clustered at the magistrate × year × case severity, p-values are presented within parentheses.

	Second Stage Estimates		
	(1)	(2)	(3)
1. Main Result:			
Pretrial Incarceration (PTI)	−0.130	−0.108	−0.108
	(0.047)	(0.042)	(0.042)
N	100,581		
2. Racially Disparate Impact:			
PTI (Black as baseline)	−0.185	−0.136	−0.133
	(0.044)	(0.039)	(0.040)
PTI × White (Anglo & Hispanic)	0.111	0.076	0.071
	(0.014)	(0.013)	(0.012)
N	100,821		
3. Resource Deprivation:			
PTI (Bottom tercile as baseline)	−0.151	−0.120	−0.121
	(0.047)	(0.041)	(0.041)
PTI × Middle tercile	−0.024	−0.012	−0.008
	(0.016)	(0.014)	(0.014)
PTI × Top tercile	0.073	0.030	0.034
	(0.017)	(0.015)	(0.015)
N	89,596		
4. Resource Deprivation (different baseline group):			
PTI (Middle tercile as baseline)	−0.174	−0.132	−0.129
	(0.051)	(0.046)	(0.046)
PTI × Top tercile	0.097	0.042	0.042
	(0.016)	(0.013)	(0.013)
N	89,596		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table M7: **The Effect of Pretrial Incarceration on 2012 Turnout Among All Defendants and By Race and Income (Clustered Std. Errors by magistrate × year × month × case severity).** "PTI" is the main effect of pretrial incarceration for the baseline category. "PTI × ..." is the interaction term.Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Standard errors clustered at the magistrate × year × month × case severity, p-values are presented within parentheses.

N. Deterministic Merge

	Second Stage Estimates		
	(1)	(2)	(3)
1. Main Result:			
Pretrial Incarceration (PTI)	−0.150	−0.122	−0.122
	(0.055)	(0.046)	(0.045)
First-stage F-stat	421.944	418.888	446.849
N	100,821		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table N1: **Deterministic Merge: The Effect of Pretrial Incarceration on 2012 Turnout.** Defendants are declared to be matches, if names and age match exactly (or nearly so) i.e., those observations with that agree in all linkage fields as described in Appendix F. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Heteroskedastic-consistent standard errors are presented within parentheses.

O. Compliers

	Case Severity		
	Low	Medium	High
Compliers	0.098	0.097	0.124
Always-takers	0.061	0.338	0.545
Never-takers	0.841	0.565	0.331

Table O1: **Share of Compliers Across Levels of Case Severity.**

	All		Black		White		Bottom		Middle		Top	
	Sample	Compliers	Sample	Compliers	Sample	Compliers	Sample	Compliers	Sample	Compliers	Sample	Compliers
Demographic:												
Age	35.711	35.498	35.995	35.919	35.807	35.684	35.778	35.751	35.455	35.255	35.935	35.704
	(0.037)	(0.091)	(0.051)	(0.162)	(0.071)	(0.116)	(0.07)	(0.2)	(0.068)	(0.248)	(0.064)	(0.152)
Male	0.805	0.806	0.828	0.828	0.822	0.827	0.794	0.794	0.802	0.803	0.81	0.81
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)
Race:												
Black	0.532	0.539	1	1	0	0	0.578	0.583	0.633	0.637	0.422	0.442
	(0.002)	(0.004)					(0.003)	(0.006)	(0.003)	(0.005)	(0.003)	(0.008)
White	0.266	0.26	0	0	1	1	0.255	0.251	0.178	0.175	0.371	0.351
	(0.001)	(0.003)					(0.002)	(0.006)	(0.002)	(0.004)	(0.003)	(0.007)
Zip code unknown or non-PA	0.111	0.111	0.088	0.088	0.109	0.11	0	0	0	0	0	0
	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)						
Income:												
Bottom tercile	0.297	0.299	0.323	0.323	0.285	0.285	1	1	0	0	0	0
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.006)						
Middle tercile	0.301	0.304	0.359	0.358	0.202	0.206	0	0	1	1	0	0
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)	(0.004)						
Top tercile	0.29	0.287	0.23	0.23	0.404	0.4	0	0	0	0	1	1
	(0.001)	(0.002)	(0.002)	(0.002)	(0.003)	(0.007)						
Case-related:												
Any drug offense	0.361	0.334	0.367	0.36	0.405	0.364	0.424	0.408	0.345	0.322	0.335	0.285
	(0.001)	(0.011)	(0.002)	(0.015)	(0.003)	(0.024)	(0.003)	(0.024)	(0.003)	(0.025)	(0.003)	(0.018)
Any violent offense	0.145	0.175	0.161	0.167	0.11	0.152	0.147	0.167	0.156	0.194	0.131	0.179
	(0.001)	(0.014)	(0.002)	(0.018)	(0.002)	(0.024)	(0.002)	(0.025)	(0.002)	(0.033)	(0.002)	(0.025)
Any property offense	0.2	0.226	0.218	0.223	0.191	0.232	0.191	0.207	0.2	0.229	0.204	0.252
	(0.001)	(0.012)	(0.002)	(0.016)	(0.002)	(0.024)	(0.002)	(0.02)	(0.002)	(0.026)	(0.002)	(0.023)
Any DUI offense	0.111	0.096	0.09	0.085	0.126	0.113	0.076	0.077	0.109	0.104	0.144	0.101
	(0.001)	(0.007)	(0.001)	(0.009)	(0.002)	(0.012)	(0.002)	(0.009)	(0.002)	(0.017)	(0.002)	(0.013)
Any firearm offense	0.08	0.097	0.112	0.116	0.041	0.058	0.086	0.097	0.091	0.112	0.065	0.088
	(0.001)	(0.008)	(0.001)	(0.013)	(0.001)	(0.01)	(0.002)	(0.015)	(0.002)	(0.019)	(0.001)	(0.013)
Any prior case	0.726	0.724	0.834	0.834	0.82	0.814	0.774	0.772	0.74	0.736	0.715	0.712
	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.004)	(0.002)	(0.005)	(0.003)	(0.004)
Total OGS	10.039	11.586	11.181	11.497	8.659	10.737	10.415	11.256	10.432	12.183	9.219	11.882
	(0.035)	(0.667)	(0.055)	(0.937)	(0.062)	(1.192)	(0.067)	(1.179)	(0.065)	(1.593)	(0.064)	(1.269)
Bail Denied	0.008	0.009	0.011	0.011	0.004	0.005	0.008	0.009	0.008	0.009	0.005	0.007
	(0)	(0.001)	(0)	(0.001)	(0)	(0.001)	(0)	(0.001)	(0)	(0.002)	(0)	(0.001)
Electoral covariates:												
Post-treatment turnout, 2012	0.28	0.277	0.316	0.315	0.199	0.197	0.288	0.287	0.31	0.305	0.271	0.268
	(0.001)	(0.002)	(0.002)	(0.004)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.005)	(0.002)	(0.003)
Pretreatment turnout, 2008	0.283	0.281	0.315	0.313	0.221	0.22	0.28	0.281	0.308	0.305	0.285	0.282
	(0.001)	(0.001)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)
Voting-age-ineligible in 2008	0.091	0.096	0.105	0.106	0.062	0.066	0.101	0.102	0.1	0.104	0.076	0.084
	(0.001)	(0.002)	(0.001)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.005)	(0.002)	(0.004)
Pretreatment registration	0.527	0.525	0.558	0.557	0.508	0.505	0.539	0.539	0.54	0.537	0.538	0.533
	(0.001)	(0.002)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.003)	(0.002)	(0.004)	(0.002)	(0.003)

Table O2: **Complier Comparison.** This table presents the covariate means for the overall sample and the sample of "compliers" (all and separately by race and income), following the estimation approach in Dahl, Kostol and Mogstad (2014) and Abadie (2003) described in Appendix O. 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 magistrate. Standard errors in parentheses obtained via 1,000 bootstrap samples. Bottom, middle and top refer to the income terciles.

	Black Voters'08		White Voters '08		Black Non-Voters '08		White Non-Voters '08	
	Sample	Compliers	Sample	Compliers	Sample	Compliers	Sample	Compliers
Demographic:								
Age	39.104	38.455	38.714	38.332	37.175	37.204	36.33	36.305
	(0.094)	(0.29)	(0.157)	(0.352)	(0.063)	(0.151)	(0.072)	(0.097)
Male	0.882	0.876	0.932	0.937	0.795	0.795	0.79	0.793
	(0.003)	(0.004)	(0.004)	(0.017)	(0.002)	(0.002)	(0.003)	(0.006)
Race:								
Black	1	1	0	0	1	1	0	0
White	0	0	1	1	0	0	1	1
Zip code unknown or non-PA	0.071	0.072	0.09	0.093	0.096	0.096	0.116	0.117
	(0.002)	(0.003)	(0.004)	(0.02)	(0.002)	(0.002)	(0.002)	(0.002)
Bottom tercile	0.314	0.318	0.26	0.274	0.325	0.326	0.285	0.285
	(0.004)	(0.005)	(0.006)	(0.065)	(0.003)	(0.003)	(0.003)	(0.007)
Middle tercile	0.375	0.372	0.205	0.209	0.35	0.35	0.199	0.201
	(0.004)	(0.005)	(0.006)	(0.057)	(0.003)	(0.003)	(0.003)	(0.005)
Top tercile	0.241	0.238	0.445	0.424	0.228	0.229	0.4	0.397
	(0.003)	(0.005)	(0.007)	(0.102)	(0.002)	(0.002)	(0.003)	(0.008)
Case-related:								
Any drug offense	0.377	0.299	0.379	0.225	0.363	0.368	0.407	0.391
	(0.004)	(0.033)	(0.007)	(0.101)	(0.003)	(0.017)	(0.004)	(0.031)
Any violent offense	0.144	0.237	0.109	0.28	0.159	0.149	0.104	0.119
	(0.003)	(0.045)	(0.004)	(0.098)	(0.002)	(0.021)	(0.002)	(0.031)
Any property offense	0.183	0.259	0.16	0.328	0.216	0.209	0.194	0.21
	(0.003)	(0.036)	(0.005)	(0.09)	(0.002)	(0.017)	(0.003)	(0.03)
Any DUI offense	0.125	0.082	0.169	0.098	0.084	0.083	0.12	0.114
	(0.003)	(0.024)	(0.005)	(0.253)	(0.002)	(0.009)	(0.002)	(0.015)
Any firearm offense	0.092	0.153	0.037	0.098	0.1	0.094	0.038	0.044
	(0.002)	(0.03)	(0.003)	(0.036)	(0.002)	(0.014)	(0.001)	(0.012)
Any prior case	0.861	0.854	0.836	0.825	0.88	0.88	0.842	0.841
	(0.003)	(0.006)	(0.005)	(0.029)	(0.002)	(0.002)	(0.003)	(0.004)
Total OGS	10.119	15.023	8.206	16.773	10.87	10.397	8.492	9.308
	(0.089)	(2.258)	(0.144)	(4.349)	(0.066)	(1.05)	(0.071)	(1.589)
Bail Denied	0.008	0.013	0.004	0.007	0.011	0.01	0.004	0.004
	(0.001)	(0.003)	(0.001)	(0.003)	(0.001)	(0.001)	(0)	(0.001)
Electoral covariates:								
Post-treatment turnout, 2012	0.597	0.569	0.508	0.493	0.199	0.2	0.127	0.126
	(0.003)	(0.013)	(0.006)	(0.051)	(0.002)	(0.003)	(0.002)	(0.002)
Pretreatment turnout, 2008	1	1	1	1	0	0	0	0
Voting-age-ineligible in 2008	0	0	0	0	0	0	0	0
Pretreatment registration	1	1	1	1	0.331	0.331	0.304	0.304
	(0)	(0)	(0)	(0)	(0.003)	(0.003)	(0.003)	(0.004)

Table O3: **Complier Comparison Continued.** This table presents the covariate means for the overall sample and the sample of "compliers" (separately by race and prior voting history (in 2008)), following the estimation approach in Dahl, Kostol and Mogstad (2014) and Abadie (2003) described in Appendix O. 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 magistrate. Standard errors in parentheses obtained via 1,000 bootstrap samples.

	Black Bottom Tercile		Black Middle Tercile		Black Top Tercile	
	Sample	Compliers	Sample	Compliers	Sample	Compliers
Demographic:						
Age	36.1	36.088	35.678	35.677	36.325	36.115
	(0.09)	(0.245)	(0.089)	(0.399)	(0.108)	(0.413)
Male	0.82	0.82	0.828	0.828	0.834	0.833
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Race:						
Black	1	1	1	1	1	1
White	0	0	0	0	0	0
Zip code unknown or non-PA	0	0	0	0	0	0
Bottom tercile	1	1	0	0	0	0
Middle tercile	0	0	1	1	0	0
Top tercile	0	0	0	0	1	1
Case-related:						
Any drug offense	0.404	0.411	0.362	0.357	0.347	0.326
	(0.004)	(0.026)	(0.003)	(0.035)	(0.004)	(0.034)
Any violent offense	0.162	0.148	0.161	0.182	0.15	0.168
	(0.003)	(0.027)	(0.003)	(0.042)	(0.003)	(0.044)
Any property offense	0.212	0.201	0.211	0.227	0.225	0.244
	(0.003)	(0.023)	(0.003)	(0.035)	(0.004)	(0.04)
Any DUI offense	0.071	0.068	0.1	0.11	0.102	0.085
	(0.002)	(0.01)	(0.002)	(0.025)	(0.003)	(0.024)
Any firearm offense	0.109	0.1	0.112	0.126	0.109	0.123
	(0.002)	(0.019)	(0.002)	(0.029)	(0.003)	(0.034)
Any prior case	0.847	0.849	0.832	0.829	0.836	0.835
	(0.003)	(0.004)	(0.003)	(0.006)	(0.003)	(0.009)
Total OGS	11.113	10.531	11.087	11.945	10.928	12.026
	(0.089)	(1.332)	(0.092)	(2.122)	(0.107)	(2.433)
Bail Denied	0.01	0.009	0.011	0.012	0.009	0.011
	(0.001)	(0.002)	(0.001)	(0.003)	(0.001)	(0.003)
Electoral covariates:						
Post-treatment turnout, 2012	0.32	0.321	0.33	0.33	0.316	0.311
	(0.003)	(0.006)	(0.003)	(0.009)	(0.004)	(0.01)
Pretreatment turnout, 2008	0.309	0.309	0.328	0.329	0.324	0.32
	(0.003)	(0.004)	(0.003)	(0.006)	(0.004)	(0.007)
Voting-age-ineligible in 2008	0.109	0.108	0.106	0.107	0.096	0.099
	(0.002)	(0.004)	(0.002)	(0.007)	(0.003)	(0.009)
Pretreatment registration	0.564	0.565	0.559	0.558	0.571	0.567
	(0.003)	(0.005)	(0.003)	(0.007)	(0.004)	(0.007)

Table O4: **Complier Comparison Continued.** This table presents the covariate means for the overall sample and the sample of "compliers" (separately by race-income subsets), following the estimation approach in Dahl, Kostol and Mogstad (2014) and Abadie (2003) described in Appendix O. 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 magistrate. Standard errors in parentheses obtained via 1,000 bootstrap samples.

P. Resource Deprivation By Race

	Second Stage Estimates		
	(1)	(2)	(3)
1. Triple Interaction Model:			
PTI (Black, bottom tercile as baseline)	−0.183	−0.141	−0.140
	(0.046)	(0.040)	(0.041)
First-stage F-stat	159.278	148.525	123.303
PTI × Middle tercile	−0.006	−0.003	0.000
	(0.018)	(0.015)	(0.015)
First-stage F-stat	733.833	742.296	788.417
PTI × Top tercile	0.005	0.000	0.006
	(0.020)	(0.018)	(0.018)
First-stage F-stat	564.937	567.100	597.203
PTI × White	0.107	0.070	0.066
	(0.022)	(0.020)	(0.020)
First-stage F-stat	440.888	435.393	461.850
PTI × Middle tercile × White	−0.046	−0.025	−0.025
	(0.037)	(0.032)	(0.032)
First-stage F-stat	91.115	91.917	97.838
PTI × Top tercile × White	0.034	0.028	0.024
	(0.034)	(0.029)	(0.029)
First-stage F-stat	174.761	173.046	178.740
N	89,956		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table P1: **Resource Deprivation Analyses by Defendant Race.** "PTI" is the main effect of pretrial incarceration for the baseline category. "PTI × ..." is the interaction term. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Heteroscedasticity-consistent standard errors are presented within parentheses.

Q. Race and Pretreatment Turnout

	Dependent variable: 2008 Turnout	
	(1)	(2)
Black	0.116	0.114
	(0.003)	(0.003)
Female		0.014
		(0.004)
Age		0.004
		(0.001)
Offense Severity		−0.012
		(0.002)
Constant	0.236	0.126
	(0.003)	(0.007)
N	73,157	73,157
R^2	0.018	0.029
Adjusted R^2	0.018	0.029

Table Q1: **Pretreatment Turnout by Race.** OLS regression of turnout in the 2008 general election on race and covariates. Sample includes defendants who were old enough to vote in the 2008 election and who are identified as Black or White in the court records.

	Second Stage Estimates		
	(1)	(2)	(3)
1. Triple Interaction Model:			
PTI (Black non-voter as baseline)	−0.096	−0.080	−0.079
	(0.040)	(0.040)	(0.041)
First-stage F-stat	213.255	196.524	175.089
PTI × White	0.054	0.045	0.041
	(0.014)	(0.014)	(0.013)
First-stage F-stat	765.844	747.170	772.417
PTI × 2008 Voter	−0.185	−0.185	−0.185
	(0.020)	(0.019)	(0.019)
First-stage F-stat	916.899	924.466	929.548
PTI × White × 2008 Voter	0.064	0.063	0.063
	(0.040)	(0.039)	(0.039)
First-stage F-stat	205.538	196.234	202.624
N	91,605		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table Q2: **The Effect of Pretrial Incarceration on 2012 Turnout By Race and Prior Turnout.** "PTI" is the main effect of pretrial incarceration for the baseline category. "PTI × ..." is the interaction term. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Each of the analyses presented in this table include interactions between the covariates of interest (race, 2008 turnout status) and pretrial incarceration. In addition, we interact the instrument with each covariate of interest. Heteroskedastic-consistent standard errors are presented within parentheses. Defendants who were too young to vote in the 2008 election are omitted from the race and prior turnout triple interaction model.

R. Race and Pretreatment Registration Status

	Second Stage Estimates		
	(1)	(2)	(3)
1. Triple Interaction Model:			
PTI (Black non-registered as baseline)	−0.042	−0.025	−0.025
	(0.044)	(0.041)	(0.041)
First-stage F-stat	201.671	184.228	164.382
PTI × Registered Voter	−0.191	−0.177	−0.175
	(0.014)	(0.013)	(0.013)
First-stage F-stat	1667.236	1668.129	1681.576
PTI × White	0.023	0.016	0.012
	(0.014)	(0.013)	(0.012)
First-stage F-stat	765.034	748.209	777.559
PTI × Registered Voter × White	0.110	0.102	0.101
	(0.026)	(0.024)	(0.024)
First-stage F-stat	378.875	378.820	384.501
N	91,605		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table R1: **The Effect of Pretrial Incarceration on 2012 Turnout By Race and Prior Registration.** "PTI" is the main effect of pretrial incarceration for the baseline category. "PTI × ..." is the interaction term. Defendants under age 18 during the 2008 general election are excluded. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Heteroscedasticity-consistent standard errors are presented within parentheses.

S. Timing Before the Election

	Second Stage Estimates		
	(1)	(2)	(3)
Excluding Cases Within 0 Months of Election:			
Pretrial Incarceration (PTI)	−0.130	−0.108	−0.108
	(0.048)	(0.041)	(0.041)
Prop. detained pretrial during election	0.052		
Prop. incarcerated post-conviction during election	0.035		
N	100,871		
Excluding Cases Within 2 Months of Election:			
Pretrial Incarceration (PTI)	−0.087	−0.067	−0.068
	(0.052)	(0.045)	(0.045)
Prop. detained pretrial during election	0.035		
Prop. incarcerated post-conviction during election	0.037		
N	94,316		
Excluding Cases Within 4 Months of Election:			
Pretrial Incarceration (PTI)	−0.091	−0.058	−0.058
	(0.058)	(0.050)	(0.050)
Prop. detained pretrial during election	0.025		
Prop. incarcerated post-conviction during election	0.039		
N	88,227		
Excluding Cases Within 6 Months of Election:			
Pretrial Incarceration (PTI)	−0.028	−0.005	−0.006
	(0.064)	(0.055)	(0.054)
Prop. detained pretrial during election	0.019		
Prop. incarcerated post-conviction during election	0.040		
N	82,298		
Excluding Cases Within 12 Months of Election:			
Pretrial Incarceration (PTI)	0.054	0.046	0.041
	(0.089)	(0.074)	(0.074)
Prop. detained pretrial during election	0.011		
Prop. incarcerated post-conviction during election	0.040		
N	67,401		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table S1: **The Effect of Pretrial Incarceration on 2012 Turnout, Excluding Cases Before Election Day.** For each threshold (from 0-12 months before the election), we exclude the cases filed in that time period. "Prop. Detained Pretrial During Election" reports the proportion of defendants in the sample that were detained pretrial on election day in the focal case. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table I1. Heteroscedasticity-consistent standard errors are presented within parentheses.

T. The Indirect Role of Incapacitation

First 2SLS regression:

$$P_{dtjh} = \alpha_0 + \alpha_1 Z_{dtjh} + X_{dt}^{\top}\Omega + \epsilon_{dtjh}$$
$$I_{dt} = \beta_0 + \beta_1 \widehat{P}_{dtjh} + X_{dt}^{\top}\Lambda + \varepsilon_{dt}$$

Second 2SLS regression:

$$I_{dt} = \kappa_0 + \kappa_1 Z_{dtjh} + \kappa_2 P_{dtjh} + X_{dt}^{\top}\Delta + \upsilon_{dtjh}$$
$$T_{d,2012} = \gamma_0 + \gamma_1 \widehat{I}_{dt} + \gamma_2 P_{dtjh} + X_{dt}^{\top}\zeta + \xi_{dt}$$

Table T1: **Mediation Equations.** In this setting, the direct effect of pretrial incarceration on turnout is γ_2 (red path in Figure T1), the indirect effect of pretrial incarceration that goes through incapacitation is $\beta_1 \times \gamma_1$ (blue path in Figure T1), and total effect is represented by $\gamma_2 + \beta_1 \times \gamma_1$. Here d indicates defendant, j is for magistrate, and h is the offense severity level. $T_{d,2012}$ is an indicator for voting in 2012, P_{dtjh} is an indicator for being detained pretrial more than 3 days, I_{dt} is an indicator for incapacitation on election day, Z_{dtjh} is the instrument (leave-out-case pretrial incarceration rates), and X_{dt} is a set of defendant and case covariates and fixed effects as described in Table I1.

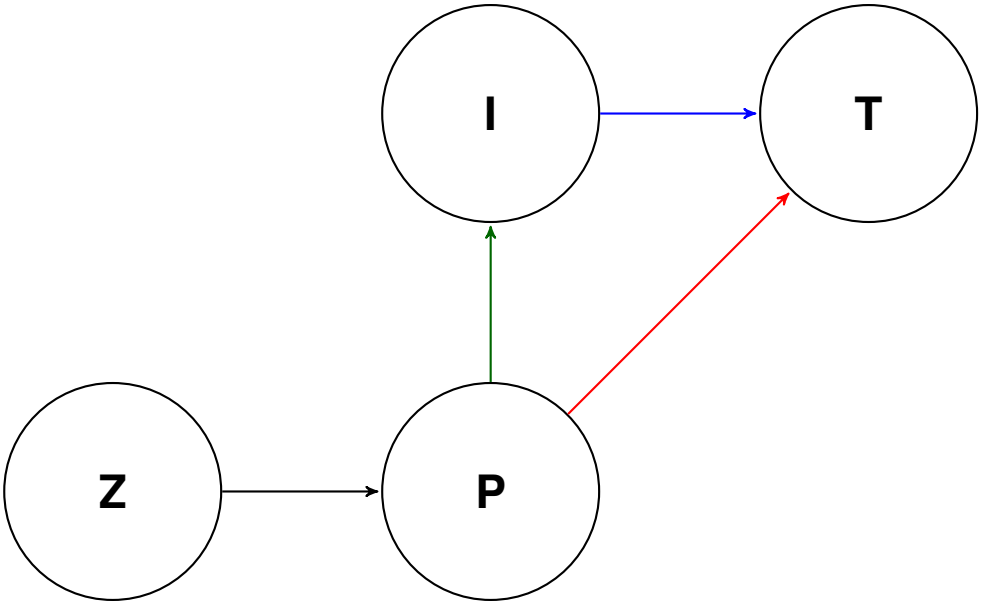

Figure T1: **Mediation Analysis Diagram.** The Direct and Indirect Effect of Pretrial Incarceration (P) on Turnout (T). I represents incapacitation on Election Day. Z is our instrument and represents the leave-out-case pretrial incarceration rate.