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

January 15, 2021

A.   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 do not know how prevalent that belief is, and are unable to test this mechanism, but in any case 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.

Mechanical Effects: Pretrial incarceration may reduce turnout because detainees may be held pretrial on election day, and find voting logistically difficult (though are legally allowed to do so) (Paikowsky 2019). Such a mechanical effect would not weaken modeling assumptions or introduce bias. Instead, it is one possible mechanism for the causal effect of pretrial incarceration. However, this mechanism is too rare to account for the effect of pretrial incarceration: pretrial incarceration extends through the election for only 5% of defendants in our sample.

One might assess this mechanical effect by dropping all defendants with cases close to the election, assuming the proportion incarcerated on election day would thereby decline. This

approach reduces the number incarcerated pretrial during the election without selecting directly on this variable. However, this approach faces its own limitation: it simultaneously varies the timing of treatment, confounding mechanical effects with effect persistence. We present these results with caution, in Table T1, noting these caveats (see Appendix T).

We can rule out an additional mechanical 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).

Finally, we assess an incarceration mechanism of conviction and sentencing to prison, as reported in the paper.

B.   Court Records Data

In this section, we provide additional details on the court records data.

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 estimate effects separately for defendants identified as Asian or Native American/Alaskan Native due to small sample sizes (less than 2,000 defendants), or for defendants whose race is not reported.[1]

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[1] Using surname prediction based on geolocation (Imai and Khanna 2016), we find that among defendants for whom race is not reported (16% of our sample), the predicted share of Whites is similar to the proportion of Whites among defendants with recorded race (27% vs. 32%

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. 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 assessment.[2] This approach generates some false negatives: individuals who identify as Hispanic, but whose surname is not commonly associated with Hispanics.[3] 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.

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respectively). Because surname methods predict nonwhite race imprecisely, we do not use them to predict the race of those with unrecorded race, and we do not subset by unrecorded race (Sood and Laohaprapanon 2018).

[2] As a robustness check, we use a higher threshold: we classify a defendant as Hispanic if the predicted probability is above 90% and White Anglo otherwise. With this classification, the sample size of White Hispanic defendants declines by 11% (n=7,833). The magnitude of the estimates decreases by about half and the standard errors are similar. Using this threshold makes no substantive difference.

[3] This approach may misclassify as Anglo those who identify as Hispanic but have the surname of a non-Hispanic father or spouse.

Prior Cases

The dataset's measure of prior cases captures only those in Pennsylvania state courts. Federal charges are not included. The measure of prior cases 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 911 and the median is 2,164; thus, including offense severity in our analyses is unlikely render estimates sensitive to the inclusion of a particular observation.

Zip Code

We omit defendants with missing zip codes or invalid zip codes. Some invalid zip codes are typos whereas others, like 19100, are used by law enforcement or court clerks when a defendant's address is unknown.

Defendants with Multiple Cases

Most (67%) defendants in our sample have one case between 2008 and 2012, and 20% have two. For individuals with multiple cases, we include only their last case before Election Day.

Among such individuals, their cases are far apart (340 days on average) and we expect the case closest to the election is most relevant for turnout.

Conviction and Case Timing Data

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, incarceration sentence start and end).

C.   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.[4]

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[4] 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).

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 February 2013 Pennsylvania voter files and the official counts around that time.[5] 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[6]). The 2014 files are the first snapshot after the 2012 election for all states and the District of Columbia. Unlike the raw

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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).

[5] 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.

[6] 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.

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.

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.[7]

Third, we confirm that any remaining discrepancies between our voter file data and official records are unrelated to treatment status (our magistrate leniency instrument). If exclusion from the voter file was different for those detained versus released pretrial, we would expect to see a non-negligible correlation between pretreatment voting data and our instrument. The second column of Table G1 presents results from a linear regression of magistrate leniency on various pretreatment covariates, including those constructed from the voter file. We find that pretreatment turnout is not significantly correlated with treatment status, and while pretreatment registration exhibits a statistically significant correlation with the treatment, the magnitude of the correlation

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[7] 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).

is close to zero (-0.00074). In sum, we do not find evidence that any mismeasurement in the voting data that remains after our extensive record linkage procedures would lead to biased estimates.

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.

D. 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).[8] More specifically, we say that record $a \in \{1,\dots, A\}$ in our court data is a potential match of record $b \in \{1,\dots, B\}$ in the voter file if the estimated match probability ψ_{ab} is the largest among all pairs that involve record a. This procedure yields a one-to-one match for each record a with an estimated maximum match probability of $\zeta_a = \max_{b=1,\dots,B} \psi_{ab}$. 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.[9] We convert date of birth to exact

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[8] 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.

[9] 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.

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 six months and 1 year of difference. In other words, we classify as agreements (partial agreements) in age those observations that are at most six months (one 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 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 90,589, we matched 48,740 records (29,101 Blacks and 12,454 Whites), of which 1,904 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 82,191 defendants that were old enough to vote in 2008, we could match 48,107 (28,576 Blacks and 12,912 Whites) records with the 2009 voter

file.[10] 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.

E.   Descriptive Statistics

[Table E1 about here]

F.   Criminal Case Process

Figure F1 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).[11] In our study, if a defendant is unable to post bail within 3 days, we code them as "detained" and otherwise "released." Between

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[10] While our match rates are somewhat larger for Blacks (58% in 2009 and 59% 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).

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

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 B, we lack data for some of these stages.

[Figure F1 about here.]

G.   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 G1 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 G1 about here]

H.   First Stage Results

Table H1 presents the first stage results associated with each analysis in Figures 1-3, and Table H2 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 H1 about here.]

[Table H2 about here.]

Robustness Checks, Estimates from Main Text and Other Analyses

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

Column 3 of Table I1 reports the estimates presented in Figures 1-3, which include year and offense fixed effects, demographic covariates, and additional case controls. Columns 1 and 2 present the estimates from models with fixed effects only and fixed effects plus demographic covariates, respectively. In each analysis, while the magnitude of the estimate varies, the results are substantively consistent with and without covariates.

[Table I1 about here]

J.   Bivariate Probit Regressions

Due to the dichotomous nature of both our outcome of interest and pretrial incarceration, we conduct a robustness check with bivariate probit regression (using the same instrument). Table J1 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 I1, the marginal effect of pretrial incarceration on turnout is -8 percentage points and statistically significant. The biprobit estimates are statistically indistinguishable from their counterparts in Table I1; the t-statistics are -0.32, $p=0.75$ (All defendants); -0.76, $p=0.44$ (Bottom tercile); -0.79, $p=0.43$ (Middle tercile); 1.22, $p=0.22$ (Top tercile); -0.10, $p=0.92$ (Black defendants); -0.76, $p=0.44$ (White defendants); -0.13, $p=0.90$ (Prior case); -0.62, $p=0.53$ (No prior case), respectively.

[Table J1 about here.]

K.   OLS Regressions

Table K1 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 K1 about here.]

L.   Additional Robustness Checks

We assess the robustness of the main effects in Figure 1 and Table I1. Results are presented in Table L1 for the full sample and for the subset of Black defendants, among whom we find a strong demobilizing effect of pretrial incarceration.

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, and day of the week-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.

Continuous Measure of Pretrial Incarceration

Instead of using a binary indicator of pretrial incarceration to calculate the leave-out mean pretrial incarceration rate, we use the logged number of days detained pretrial (row 2). The negative effect of pretrial incarceration remains. With the continuous measure, we find suggestive evidence that longer incarceration is associated with larger demobilizing effects on turnout.

13

Permutation Test

To verify the statistical significance of our main findings we conduct a permutation test (row 3). As in Stevenson (2018), we create 1,000 "false" work schedules for the magistrates. Each false schedule has a magistrate working for five days in a row on the same shift, and each magistrate only works one shift per five-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 than those estimated parametrically and statistically significant at the usual levels, confirming that the estimated effect is unlikely to be due to random chance. [12]

Excluding Years with Magistrate Vacancies

We address potential bias from excluding 11,343 observations with missing data for the bail magistrate. Most (97%) occur in 2008-2009 due the retirement of two bail magistrates (in March 2008 and March 2009), and a delay in hiring replacements, which did not occur until August 2009 and January 2010, respectively. Bail hearings were conducted by one of multiple legal attorneys from the central court office who filled in during this period (Roberts, Gabriel, email communication, July 3, 2018).

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[12] Because the permutation test is computationally expensive, we do not use it for any subset.

This exclusion could introduce selection bias if missing bail magistrate information is not randomly distributed across cases. To address the selection concern, we first assess whether observations with a named bail magistrate differ observably from those without by conducting covariate balance tests on demographic and case covariates (see Table L2). 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. Second, we show that our results persist in the subset of cases in January 2010 onwards, a period with too small a percentage of missing magistrates to introduce substantial selection bias. 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 L1, row 4).

Expanded Definition of "Violent" and "Property" Offenses

We obtain similar results using alternative measures of offense type. In row 5, 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. 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 6, 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 three 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 treatment effects are slightly smaller (within 1.2 percentage points of the estimates presented in the main text), but remain substantively consistent.

[Table L1 about here.]

[Table L2 about here.]

Placebo Test

Table L3 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 L3 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 which 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 L4 presents the results. Pretrial incarceration significantly decreases the likelihood that Blacks but not Whites registered to vote before the 2012 election. We can reject the null hypothesis of less than or equal size effects among Black non-registered defendants compared to White non-registered defendants at $p = 0.041$- 0.054. These results indicate that pretrial incarceration may also interfere with an important step in electoral participation (registration) among Black defendants who were not previously engaged.

[Table L4 about here.]

M.   Testing for Effects Across Groups (Income, Race, and Prior Case Status)

We test if the effect of pretrial detention is different across the subgroups presented in Figures 1-3. We use one-sided t-tests, due to the directional predictions. In the resources analyses (Table M1, row 1), we test the null hypotheses that the effects on defendants in lower terciles in terms of neighborhood average income are less than or equal to the effects on defendants in upper terciles (e.g. bottom tercile ≤ top tercile; middle ≤ top; bottom ≤ middle). In the analyses by racial group (Table M1, row 2), we test the nulls that the effects on Blacks are less than or equal to the effects on White Anglos, White Hispanics, and all Whites (Anglo and Hispanic), respectively, and the effects on White Hispanics are less than or equal to the effects on White Anglos. Finally, we test the null hypothesis that the effects on defendants with no prior case are less than or equal to the effects on defendants with at least one prior case (Table M1, row 3). In the resources analyses, we can reject the null when comparing the effects on those whose neighborhood average income falls in the middle vs. top tercile. In the racial subgroup analyses, we find suggestive evidence that the effects are concentrated among Black defendants, as the test comparing Black defendants and

17

White defendants (Hispanic and Anglo) rejects the null at p = 0.041 to 0.065. Finally, we cannot reject the null for those with and without prior cases.

[Table M1 about here.]

N.   Resource Deprivation By Race

Table N1 replicates the resource deprivation mechanism test by race. For Black defendants, the demobilizing effects of pretrial incarceration are concentrated among the bottom and middle terciles of neighborhood income. However, the effects among White defendants of any income are vanishingly small.[13] These findings suggest that the demobilizing effects of pretrial incarceration hold only for Black low-income defendants.

[Table N1 about here.]

O.   Race and Pretreatment Turnout

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 O1 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 O1 about here]

We further assess whether Black detainees' greater likelihood of prior voting explains the larger effect of pretrial incarceration. Table O2 presents the second-stage estimates of pretrial incarceration separately for Black and White non-voters in 2008 (row 1) and voters in 2008 (row

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

2). We omit defendants who were too young to vote in 2008. Table O3 presents one-sided t-tests, testing the null that effects among Black 2008 voters are less than or equal to the effects on Black 2008 non-voters (row 1), less than or equal to the effects on White 2008 voters (row 2) and less than or equal to the effects on White Anglo 2008 voters (row 3). Results are discussed in the paper.

[Table O2 about here]

[Table O3 about here]

P.   Race and Pretreatment Registration Status

In the main text when we discussed the *Racially Disparate Impact* of pretrial detention on turnout and in Appendix O, 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.

First, Table P1 presents the results by registration status. We divide the sample by pretreatment registration status and exclude defendants who were not old enough to vote in the last general election (2008) to improve the comparability of the samples. We first estimate the effects of pretrial incarceration on 2012 turnout for all defendants not registered pretreatment and separately by race (row 1). We then estimate the effects for all defendants registered pretreatment and separately by race (row 2). For the analyses by race, we pool White Anglo and White Hispanic for statistical power.

19

The effects among White defendants who were registered pretreatment are near zero, whereas the effects among Black defendants who were registered pretreatment are approximately -8 percentage points. These results are not consistent with the theory, though neither effect is precisely estimated, and in t-tests, we cannot reject the null that the effect on Black registered defendants is less than the effect among White registered defendants (p = 0.127- 0.220). Taken together, these results do not confirm the theory that concentrated demobilizing effects among Black defendants are explained by higher levels of baseline engagement. Instead, we find suggestive evidence that pretrial incarceration is more demobilizing to Blacks with existing ties to the electoral process.

[Table P1 about here.]

Q.   Non-Monotonicity By Race

A magistrate may be more lenient than colleagues in cases involving White defendants and more harsh than colleagues in cases involving Black defendants. As Arnold et al. explain, "Bail judges must make on-the-spot judgments with limited information and little to no interaction with defendants. These institutional features may make bail decisions particularly prone to the kind of stereotypes or categorical heuristics that exacerbate racial bias" (2018, 1887). This non-monotonicity may bias estimates in the presence of heterogeneous treatment effects (Stevenson 2018). To account for this form of non-monotonicity and assess whether our results persist, we construct the instrument separately for Black and White defendants based only on other defendants of the same race. Table Q1 presents the results from this specification. The results suggest again that pretrial incarceration demobilizes Black defendants, though the estimate is less precise, and we cannot reject the null for White defendants.

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 from which magistrate leniency is calculated and makes cell counts more imbalanced due to the skewed racial composition of the sample.[14] 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 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 Q1 about here.]

R.   **Mediation Analysis by Income, Race and Prior Case Status**

In Table R1, we report the mediation analysis in Table 1 for tercile of neighborhood income (row 1), for Black and White defendants (row 2), and for defendants with and without prior cases (row 3). The mediator is a binary variable coded 1 if the defendant's case received a disposition before the election and resulted in a minimum incarceration sentence of 1 day, and 0 otherwise. In all subgroups, we find no evidence for the sentencing mechanism: our main estimates of the effect of pretrial incarceration are not explained by increased sentences as a result of pretrial detention.

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[14] See Table E1.

This is not a perfect measure of being in prison before or during the election. Some defendants coded as having been *sentenced* to prison before the election are not actually *in* prison before the election. Their sentence may have been satisfied by time they served pretrial, or may have been delayed until after the election. The effect of pretrial incarceration on these defendants could not be explained by a post-conviction imprisonment that does not take place. In sum, there is error in the mediator, and it is unclear how that affects the mediation results. We regard the analysis as suggestive for that reason, and for the usual reasons with mediation tests.

[Table R1 about here.]

S.   Compliers

Following Dahl et al. (2014) and Dobbie et al. (2018), we characterize the fraction of compliers in our data and check how different they are to our complete sample of defendants. In this setting, *compliers* are defined as the defendants whose pretrial detention decision would have been different had their case been assigned to the strictest instead of the most lenient judge. Formally, we have 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 judge 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 judge 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 S1 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 99th (1st) percentile of our instrument. Across level of severity, the share of *compliers* is estimated to lie between 11% and 16%. 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 S2 compares the sample of compliers (all compliers and separately by race) 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 (6 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. We also find higher levels of pretreatment registration and turnout among Black than White compliers, but other demographic and case covariates differ only slightly (five percentage points at most).

[Table S1 about here.]

[Table S2 about here.]

T. Timing Before the Election

We assess the extent to which our results may decay over time. We re-run the analysis after dropping cases filed within 2, 4, 6, and 12 months of the election (respectively). Results are shown in Table T1. The effect of pretrial incarceration drops by about a third—to 6.9 percentage points—and is less precisely estimated once we exclude cases within 2 months of the election. As we continue to drop cases at greater length from the election, the estimate declines further, suggesting that pretrial incarceration is most demobilizing when it occurs close to the election.

Theoretically, the timing effect could be driven by incarceration during the election instead of decay, but in practice, this alternative explanation is unlikely. As noted earlier, the percentage of defendants who might have been imprisoned post-conviction during the election is small. Specifically, only 4% of defendants with cases in the 6 months before the election had a case disposition date before the election and a minimum incarceration sentence of 1 or more days. In addition, pretrial incarceration during the election is also very rare overall (5% of the sample) and when we drop cases within 2 months of the election (4%).

[Table T1 about here.]

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Tables and Figures for Online Appendix

E. Descriptive Statistics

	Full Sample		Detained		Released	
	mean	s.d	mean	s.d.	mean	s.d.
Pretrial incarceration:						
Detained > 3 days	0.36	0.48	1.00		0.00	
Detained 1 year	0.03	0.18	0.10	0.29	0.00	
Total days detained	51.78	143.95	145.06	211.43	0.23	0.59
Demographic:						
Age (years)	35.66	11.82	34.77	11.36	36.15	12.04
Female	0.19	0.39	0.12	0.33	0.23	0.42
Race:						
Black	0.55	0.50	0.64	0.48	0.50	0.50
White	0.27	0.44	0.26	0.44	0.27	0.45
White Anglo	0.17	0.38	0.14	0.35	0.19	0.39
White Hispanic	0.10	0.30	0.12	0.32	0.08	0.28
Other	0.02	0.15	0.02	0.15	0.02	0.15
Not reported	0.16	0.37	0.08	0.27	0.21	0.41
Zip code average income:						
Bottom tercile (< $25,888)	0.33	0.47	0.38	0.48	0.31	0.46
Middle tercile ($25,888-$34,090)	0.34	0.47	0.34	0.47	0.34	0.47
Top tercile (>$34,090)	0.32	0.47	0.28	0.45	0.35	0.48
Case-related:						
Any drug offense	0.37	0.48	0.29	0.45	0.41	0.49
Any violent offense	0.14	0.35	0.22	0.42	0.10	0.29
Any violent offense (expanded)	0.29	0.45	0.41	0.49	0.22	0.42
Any firearm offense	0.08	0.27	0.17	0.37	0.03	0.16
Any property offense	0.20	0.40	0.29	0.46	0.15	0.35
Any property offense (expanded)	0.21	0.41	0.31	0.46	0.16	0.36
Any DUI offense	0.11	0.31	0.04	0.19	0.15	0.36
Offense Gravity Score (OGS) tercile	1.95	0.82	2.41	0.70	1.70	0.77
OGS less than 3 (tercile 1)	0.36	0.48	0.12	0.33	0.49	0.50
OGS between 4 and 11 (tercile 2)	0.33	0.47	0.35	0.48	0.32	0.46
OGS more than 12 (tercile 3)	0.31	0.46	0.53	0.50	0.19	0.46
Misdemeanor charge only	0.47	0.50	0.20	0.40	0.62	0.48
Any prior case	0.75	0.43	0.86	0.35	0.68	0.46
# prior cases with felony charges	1.31	1.99	2.02	2.37	0.92	1.62
# prior cases with violent charges	0.92	1.72	1.38	2.08	0.67	1.42
# prior convictions	1.64	2.42	2.28	2.76	1.28	2.12
First bail amount ($)	27,647	187,747	70,754	309,427	3,823	14,855
Electoral:						
Pretreatment Turnout in 2008	0.29	0.40	0.24	0.38	0.32	0.41
Post-teatment Turnout in 2012	0.29	0.40	0.21	0.36	0.33	0.41
Voting-age-ineligible in 2008	0.09	0.29	0.11	0.31	0.08	0.28
Pretreatment registration	0.54	0.41	0.51	0.41	0.55	0.40
N	90,589		32,245		58,344	

Table E1: **Descriptive Statistics** for the full sample, for defendants detained pretrial for 3 or more days and 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 B). "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.

F. Criminal Case Process
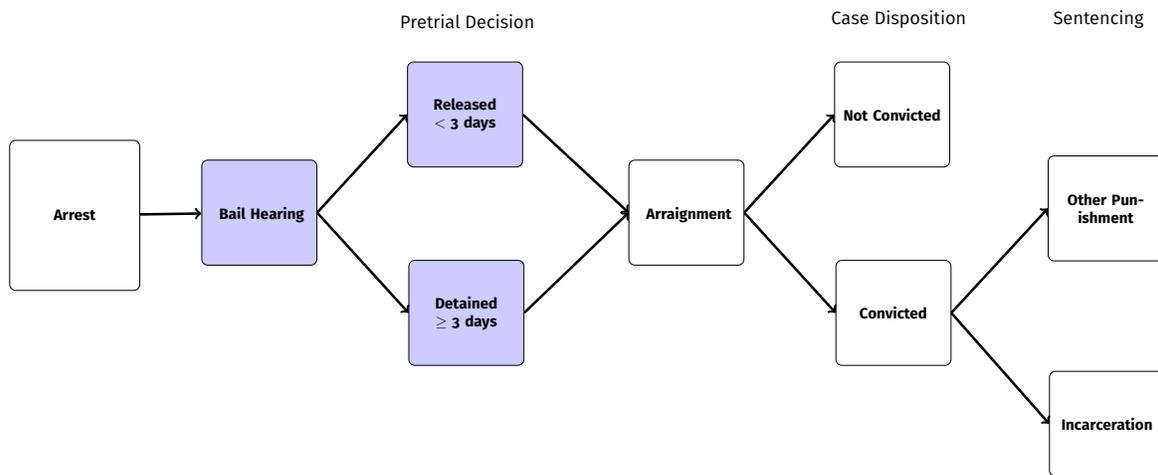
Figure F1: **Criminal Case Process.** See Appendix F.

G. Randomization Test

	Randomization Test	
	Pretrial Incarceration	Magistrate Leniency
Demographic:		
Age	0.00532	−0.00005
	(0.00080)	(0.00007)
Age^2	−0.00007	0.00000
	(0.00001)	(0.00000)
Female	−0.11996	−0.00101
	(0.00349)	(0.00030)
Race:		
White	−0.02712	−0.00059
	(0.00338)	(0.00028)
Other	−0.04570	−0.00049
	(0.00912)	(0.00074)
Not reported	−0.10464	−0.00009
	(0.00415)	(0.00036)
Case-related:		
Any drug offense	−0.02532	0.00109
	(0.00368)	(0.00029)
Any violent offense	0.02987	−0.00090
	(0.00517)	(0.00038)
Any firearm offense	0.20994	−0.00120
	(0.00549)	(0.00044)
Any property offense	0.03141	−0.00076
	(0.00432)	(0.00033)
Any DUI offense	−0.02820	0.00077
	(0.00448)	(0.00047)
Any prior case	0.16846	0.00045
	(0.00355)	(0.00032)
Electoral:		
Pretreatment turnout, 2008	−0.09019	0.00050
	(0.00415)	(0.00034)
Voting-age-ineligible in 2008	0.00989	0.00093
	(0.00592)	(0.00051)
Pretreatment registration	−0.00182	−0.00074
	(0.00424)	(0.00035)
Joint F-test	639.040	5.662
Fixed Effects	✔	✔

Table G1: **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 ($p < 0.001$ in each column). Pretrial incarceration is 1 if jailed three days or more and 0 otherwise. Magistrate leniency instrument: leave-out case mean pretrial incarceration rate. Fixed effects: bail hearing year, month, weekday-shift, and case severity tercile indicators.

H. First Stage Results

	First Stage Estimates		
	(1)	(2)	(3)
1. Main Result:			
All defendants	0.868	0.839	0.853
	(0.039)	(0.038)	(0.038)
First Stage F-test	425.098	422.155	452.956
N	90,589		
2. Resource Deprivation:			
Bottom tercile	0.896	0.881	0.907
	(0.069)	(0.067)	(0.066)
First Stage F-test	143.041	147.258	162.568
N	30,314		
Middle tercile	0.843	0.800	0.819
	(0.068)	(0.066)	(0.065)
First Stage F-test	135.139	129.738	141.17
N	30,729		
Top tercile	0.864	0.828	0.831
	(0.067)	(0.065)	(0.064)
First Stage F-test	146.686	142.988	149.346
N	29,382		
3. Racially Disparate Impact:			
Black	0.908	0.884	0.916
	(0.054)	(0.053)	(0.052)
First Stage F-test	249.031	243.871	273.198
N	49,535		
White Anglo	0.886	0.908	0.907
	(0.089)	(0.088)	(0.087)
First Stage F-test	88.402	94.745	97.306
N	15,440		
White Hispanic	0.828	0.846	0.864
	(0.134)	(0.133)	(0.131)
First Stage F-test	33.133	35.654	38.419
N	8,844		
White (All)	0.893	0.913	0.911
	(0.074)	(0.073)	(0.072)
First Stage F-test	127.755	136.839	140.725
N	24,284		
4. First-Time Defendants:			
Prior case	0.921	0.902	0.926
	(0.046)	(0.045)	(0.045)
First Stage F-test	353.941	348.894	372.699
N	67,648		
No prior case	0.537	0.495	0.505
	(0.069)	(0.067)	(0.065)
First Stage F-test	55.565	49.667	53.670
N	22,941		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table H1: **The Effect of Magistrate Leniency on Pretrial Incarceration Among All Defendants and by Income, Race and Prior Case Status.** Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Fixed effects: bail hearing year, month, weekday-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	0.848	0.850	0.867
	(0.044)	(0.043)	(0.042)
First Stage F-test	326.303	340.486	368.392
N	72,882		
Female	0.787	0.774	0.784
	(0.081)	(0.08)	(0.079)
First Stage F-test	81.378	81.553	86.251
N	17,707		
2. Offense Type:			
Any drug offense	1.022	1.040	1.051
	(0.059)	(0.058)	(0.058)
First Stage F-test	266.855	283.33	295.945
N	33,391		
Any violent offense	0.746	0.724	0.730
	(0.155)	(0.144)	(0.137)
First Stage F-test	23.275	25.222	28.254
N	12,807		
Any firearm offense	0.552	0.613	0.625
	(0.172)	(0.163)	(0.159)
First Stage F-test	10.892	14.810	16.225
N	7,087		
Any DUI offense	0.442	0.454	0.442
	(0.107)	(0.106)	(0.104)
First Stage F-test	19.892	20.522	19.910
N	9,9956		
Any property offense	0.909	0.933	0.972
	(0.106)	(0.100)	(0.097)
First Stage F-test	68.329	79.932	93.917
N	18,043		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table H2: **The Effect of Magistrate Leniency on Pretrial Incarceration By Gender and Offense Types.** Pretrial incarceration is coded as 1 if an individual was in jail for more than 3 days and 0 otherwise. Instrument, covariates and fixed effects are described in the notes to Table H1, 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 H1 as covariates except "any drug offense"). Heteroscedasticity-consistent standard errors are presented within parentheses.

I. 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:			
All defendants	−0.130	−0.105	−0.105
	(0.044)	(0.039)	(0.038)
First Stage F-test	425.098	422.155	452.956
N	90,589		
2. Resource Deprivation:			
Bottom tercile	−0.207	−0.133	−0.134
	(0.076)	(0.066)	(0.064)
First Stage F-test	143.041	147.258	162.568
N	30,314		
Middle tercile	−0.154	−0.177	−0.172
	(0.080)	(0.071)	(0.069)
First Stage F-test	135.139	129.738	141.170
N	30,729		
Top tercile	−0.026	−0.006	−0.009
	(0.076)	(0.067)	(0.067)
First Stage F-test	146.686	142.988	149.346
N	29,382		
3. Racially Disparate Impact:			
Black	−0.145	−0.110	−0.108
	(0.059)	(0.053)	(0.051)
First Stage F-test	249.031	243.871	273.198
N	49,535		
White Anglo	0.048	−0.004	−0.010
	(0.089)	(0.074)	(0.074)
First Stage F-test	88.402	94.745	97.306
N	15,440		
White Hispanic	−0.032	0.061	0.062
	(0.134)	(0.115)	(0.113)
First Stage F-test	33.133	35.654	38.419
N	8,844		
White (Anglo & Hispanic)	0.016	0.012	0.012
	(0.071)	(0.060)	(0.060)
First Stage F-test	127.755	136.839	140.725
N	24,284		
4. First-Time Defendants:			
Prior case	−0.101	−0.092	−0.095
	(0.048)	(0.042)	(0.041)
First Stage F-test	353.941	348.894	372.699
N	67,648		
No prior case	−0.277	−0.162	−0.150
	(0.148)	(0.133)	(0.130)
First Stage F-test	55.565	49.667	53.670
N	22,941		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✗	✔	✔
Case Covariates	✗	✗	✔

Table I1: **The Effect of Pretrial Incarceration on 2012 Turnout Among All Defendants and By Income, Race, and Prior Case Status.** 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 H1. Heteroscedasticity-consistent standard errors are presented within parentheses.

J. Bivariate Probit Regressions

	Bivariate Probit Estimates
1. Main Result:	
All defendants	−0.080
	(0.010)
N	90,589
2. Resource Deprivation:	
Bottom tercile	−0.074
	(0.018)
N	30,314
Middle tercile	−0.109
	(0.019)
N	30,729
Top tercile	−0.106
	(0.018)
N	29,382
3. Racially Disparate Impact:	
Black	−0.100
	(0.016)
N	49,535
White (Anglo & Hispanic)	−0.049
	(0.029)
N	24,284
4. First-Time Defendants:	
Prior case	−0.078
	(0.015)
N	67,648
No prior case	−0.064
	(0.014)
N	22,941
Instrument + Fixed Effects	✔
Demographic Covariates	✔
Case Covariates	✔

Table J1: **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, weekday-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.

K. OLS Regressions

	OLS Estimates		
	(1)	(2)	(3)
	−0.114	−0.073	−0.068
	(0.003)	(0.003)	(0.003)
N	90,589		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table K1: **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, weekday-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.

L. Additional Robustness Checks

	Second Stage Estimates		
	(1)	(2)	(3)
1. Residualized Instrument			
All defendants	−0.132	−0.107	−0.107
	(0.044)	(0.039)	(0.038)
First Stage F-test	425.117	421.154	452.213
N	90,589		
Black defendants	−0.145	−0.111	−0.110
	(0.059)	(0.053)	(0.051)
First Stage F-test	248.871	243.693	273.575
N	49,535		
2. Continuous Measure of Pretrial Incarceration (logged number of days):			
All defendants	−0.034	−0.029	−0.029
	(0.010)	(0.009)	(0.009)
First Stage F-test	417.700	408.388	443.573
N	90,589		
Black defendants	−0.037	−0.030	−0.029
	(0.013)	(0.012)	(0.012)
First Stage F-test	241.981	231.067	261.518
N	49,535		
3. Permutation Test			
All defendants	−0.130	−0.105	−0.105
	(0.047)	(0.040)	(0.039])
p-value	0.015	0.001	0.000
N	90,589		
4. Excluding Years with Magistrate Vacancies:			
Defendants, Jan. 2010 - Nov. 2012	−0.197	−0.147	−0.142
	(0.054)	(0.047)	(0.047)
First Stage F-test	286.849	281.851	293.508
N	72,517		
Black defendants, Jan. 2010 - Nov. 2012	−0.224	−0.155	−0.148
	(0.073)	(0.064)	(0.063)
First Stage F-test	164.963	162.285	175.502
N	39,519		
5. Expanded Definition of "Violent" and "Property" Offenses:			
All defendants			−0.103
			(0.038)
First Stage F-test			452.674
N	90,589		
Black defendants			−0.107
			(0.051)
First Stage F-test			272.432
N	49,535		
6. Finer-Grain Offense Covariates:			
All defendants			−0.098
			(0.038)
First Stage F-test			504.309
N	90,589		
Black defendants			−0.096
			(0.049)
First Stage F-test			313.206
N	49,535		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table L1: **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 three days and 0 otherwise, except in row 2, where this variable is the logged number of days a defendant was detained pretrial. Covariates and fixed effects are described in the notes to Tables E1 and H1, with the following exceptions. In analyses focused on Black defendants, race is omitted as a covariate. In row 5, 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 6, 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.

	Balance on Selected Covariates			
	Named Magistrate	Unnamed Magistrate	Diff.	p-value
Demographic:				
Age	35.66	36.10	0.44	0.00
Male	0.80	0.82	0.02	0.00
Race				
Black	0.55	0.59	0.04	0.00
White	0.27	0.29	0.02	0.00
Other	0.02	0.02	0.00	1.00
Not reported	0.16	0.10	−0.06	0.00
Case-related:				
Any drug offense	0.37	0.41	0.04	0.00
Any violent offense	0.14	0.12	−0.02	0.00
Any firearm offense	0.08	0.08	0.00	1.00
Any property offense	0.20	0.21	0.01	0.60
Any DUI offense	0.11	0.10	−0.01	0.00
Offense Gravity Score	10.02	9.62	−0.40	0.00
Any prior case	0.75	0.77	0.02	0.00
Day of week:				
Weekday	0.69	0.67	−0.02	0.00
Weekend	0.31	0.33	0.02	0.00
Shift:				
Morning	0.31	0.30	−0.01	0.49
Afternoon	0.36	0.40	0.04	0.00
Evening	0.34	0.31	−0.03	0.00
N	90, 589	11, 343		

Table L2: **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)
	0.009	0.010	0.009
	(0.048)	(0.050)	(0.048)
First Stage F-test	381.335	370.856	402.667
N	82,191		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table L3: **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 H1, 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)
All	−0.190	−0.169	−0.161
	(0.081)	(0.083)	(0.080)
First Stage F-test	384.390	374.258	418.788
N	34,675		
Black	−0.270	−0.236	−0.224
	(0.118)	(0.122)	(0.111)
First Stage F-test	198.090	186.636	231.516
N	17,232		
White	0.026	0.043	0.050
	(0.124)	(0.124)	(0.124)
First Stage F-test	149.571	151.870	155.898
N	10,455		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table L4: **Alternative Outcome: The Effect of Pretrial Incarceration on Registration Among Defendants Not Registered To Vote Before Focal Case.** 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 H1 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. In the analyses focused on Black and White defendants, race is used to subset the data and is omitted as a covariate. Heteroscedasticity-consistent standard errors are presented within parentheses.

M. Testing for Effects Across Groups (Income, Race, and Prior Case Status)

	(1)		(2)		(3)	
	t-stat	p-value	t-stat	p-value	t-stat	p-value
1. Resource Deprivation:						
Bottom vs. middle terciles	−0.486	(0.313)	0.446	(0.328)	0.399	(0.345)
Bottom vs top terciles	−1.693	(0.045)	−1.353	(0.088)	−1.352	(0.088)
Middle vs top terciles	−1.159	(0.123)	−1.742	(0.041)	−1.687	(0.046)
2. Racially Disparate Impact:						
Black vs. White Anglo	−1.803	(0.036)	−1.163	(0.122)	−1.091	(0.138)
Black vs. White Hispanic	−0.770	(0.221)	−1.345	(0.089)	−1.377	(0.084)
White Hispanic v. White Anglo	−0.499	(0.309)	0.470	(0.319)	0.532	(0.297)
Black vs. White (Anglo & Hispanic)	−1.738	(0.041)	−1.519	(0.064)	−1.515	(0.065)
3. First-Time Defendants:						
No prior v. prior case	−1.133	(0.129)	−0.508	(0.306)	−0.401	(0.344)
Instrument + Fixed Effects	✔		✔		✔	
Demographic Covariates	✘		✔		✔	
Case Covariates	✘		✘		✔	

Table M1: **Testing for Effects Across Groups (Income, Race, and Prior Case Status).** One-sided tests are reported for differences between subsets presented in Figures 1-3 and Table I1. In row 1, the null hypotheses are that effects on defendants in lower terciles are less than or equal to effects on defendants in upper terciles (e.g. the effects on defendants in the bottom tercile are less than or equal to defendants in the middle tercile and top tercile, respectively). In row 2, the null hypotheses are that the effects among Blacks are less than or equal to White Anglos, White Hispanics, and all Whites (Anglo and Hispanic), respectively, and the effects among White Hispanics are less than or equal to White Anglos. In row 3, the null is that effects on those with no prior case are less than or equal to those with a prior case. Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. We present the corresponding p-values within parentheses. Instrument, covariates and fixed effects are described in the notes to Table H1.

N. Resource Deprivation By Race

	Second Stage Estimates		
	(1)	(2)	(3)
1. Black Defendants:			
Bottom tercile	−0.207	−0.130	−0.131
	(0.094)	(0.083)	(0.081)
First Stage F-test	100.729	98.082	109.936
N	17,533		
Middle tercile	−0.145	−0.149	−0.145
	(0.098)	(0.086)	(0.083)
First Stage F-test	94.038	91.716	102.376
N	19,475		
Top tercile	−0.053	−0.027	−0.022
	(0.124)	(0.109)	(0.106)
First Stage F-test	56.812	56.949	63.171
N	12,456		
2. White Defendants:			
Bottom tercile	−0.068	−0.023	−0.022
	(0.140)	(0.120)	(0.118)
First Stage F-test	30.416	34.124	36.274
N	7,797		
Middle tercile	0.137	−0.012	−0.008
	(0.147)	(0.121)	(0.124)
First Stage F-test	30.221	30.709	30.319
N	5,494		
Top tercile	0.033	0.054	0.045
	(0.105)	(0.088)	(0.087)
First Stage F-test	66.632	70.667	74.423
N	10,928		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table N1: **Resource Deprivation Analyses by Defendant Race.** Bottom, middle and top tercile refers to the subsets of defendants who live in a zip code where the average income is in, respectively, the bottom, middle and top tercile of the sample distribution. 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 H1, except race is used to subset the data and is omitted as a covariate. Heteroscedasticity-consistent standard errors are presented within parentheses.

O. 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		0.003
		(0.000)
Offense Severity		-0.011
		(0.002)
Constant	0.239	0.127
	(0.003)	(0.007)
N	67,059	67,059
R^2	0.018	0.018
Adjusted R^2	0.008	0.028

Table O1: **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. Non-Voters in 2008:			
Black	−0.074	−0.012	−0.023
	(0.059)	(0.059)	(0.055)
First Stage F-test	241.412	233.509	278.051
N	28,598		
White Anglo	−0.011	−0.003	−0.006
	(0.073)	(0.070)	(0.070)
First Stage F-test	90.720	94.071	97.122
N	11,101		
White (Anglo & Hispanic)	0.036	0.042	0.041
	(0.058)	(0.055)	(0.055)
First Stage F-test	140.381	146.486	152.318
N	17,288		
2. Voters in 2008:			
Black	−0.231	−0.196	−0.172
	(0.052)	(0.053)	(0.053)
First Stage F-test	82.333	78.639	81.005
N	15,740		
White Anglo	−0.019	−0.043	−0.065
	(0.079)	(0.077)	(0.079)
First Stage F-test	30.767	31.021	30.755
N	3,606		
White (Anglo & Hispanic)	−0.106	−0.121	−0.124
	(0.068)	(0.069)	(0.069)
First Stage F-test	33.873	32.646	33.715
N	5,433		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table O2: **The Effect of Pretrial Incarceration on 2012 Turnout by Race and Pretreatment Turnout.** Defendants under age 18 during the 2008 general election are excluded. "Voters in 2008" is the sample of defendants who voted in the 2008 general election. "Non-Voters in 2008" refers to the sample of defendants who did not vote in the 2008 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 H1, with exceptions. Race and pretreatment turnout are used to subset the data and are omitted as covariates. Pretreatment registration is omitted in analyses focused on voters (row 2). Heteroscedasticity-consistent standard errors are presented within parentheses.

	(1)		(2)		(3)	
	t-stat	p-value	t-stat	p-value	t-stat	p-value
1. Black voters v. Black non-voters	−1.982	(0.024)	−2.331	(0.010)	−1.950	(0.026)
2. Black voters vs. White (Anglo & Hispanic) voters	−1.458	(0.072)	−0.862	(0.194)	−0.545	(0.293)
3. Black voters vs. White Anglo voters	−2.242	(0.012)	−1.631	(0.051)	−1.121	(0.131)
Instrument + Fixed Effects	✔		✔		✔	
Demographic Covariates	✗		✔		✔	
Case Covariates	✗		✗		✔	

Table O3: **Testing for Effects Across Race and Pretreatment Turnout Groups.** One-sided tests are reported for differences between subsets presented in Table O2. The null hypotheses are that the effect of pretrial incarceration on turnout among Black voters in 2008 is less than or equal to the effect on Black non-voters (row 1), White voters in 2008 (row 2), and White Anglo voters in 2008 (row 3). Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. We present the corresponding p-values within parentheses.

P. Race and Pretreatment Registration Status

	Second Stage Estimates		
	(1)	(2)	(3)
1. Not Registered Pretreatment:			
All	−0.076	−0.047	−0.051
	(0.066)	(0.068)	(0.066)
First Stage F-test	384.390	374.258	418.788
N	34,675		
Black	−0.092	−0.025	−0.042
	(0.099)	(0.103)	(0.094)
First Stage F-test	198.090	186.636	231.516
N	17,232		
White	0.076	0.079	0.080
	(0.098)	(0.098)	(0.098)
First Stage F-test	149.571	151.870	155.898
N	10,455		
2. Registered Pretreatment:			
All	−0.100	−0.118	−0.115
	(0.054)	(0.051)	(0.051)
First Stage F-test	295.778	294.531	311.487
N	47,516		
Black	−0.089	−0.086	−0.080
	(0.07)	(0.066)	(0.064)
First Stage F-test	180.133	180.360	194.464
N	27,106		
White	0.048	0.003	0.004
	(0.098)	(0.088)	(0.087)
First Stage F-test	84.015	89.243	92.693
N	12,266		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table P1: **The Effect of Pretrial Incarceration on 2012 Turnout Among Defendants Registered and Not Registered To Vote Pretreatment.** 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 H1 with exceptions. Pretreatment registration is used to subset the data and omitted as a covariate in all rows. In the analyses focused on Black and White defendants, race is used to subset the data and is omitted as a covariate. Pretreatment turnout is omitted in row 1. Heteroscedasticity-consistent standard errors are presented within parentheses.

Q. Non-Monotonicity by Race

	Second Stage Estimates		
	(1)	(2)	(3)
Black	−0.147	−0.106	−0.103
	(0.063)	(0.056)	(0.054)
First Stage F-test	222.425	215.427	241.312
N	49,535		
White	0.058	0.045	0.043
	(0.084)	(0.071)	(0.071)
First Stage F-test	93.490	99.411	104.507
N	24,284		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table Q1: **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 H1, 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.

R. Mediation Analysis by Income, Race and Prior Case Status

	Mediation Analysis		
	Direct Effect (Pretrial Incarceration)	Indirect Effect (Incarceration Sentence)	Total Effect
1. Resource Deprivation:			
Bottom tercile	−0.078	−0.056	−0.134
	(0.013)	(0.065)	(0.064)
N	30,314		
Middle tercile	−0.101	−0.071	−0.172
	(0.022)	(0.073)	(0.070)
N	30,729		
Top tercile	−0.048	0.039	−0.009
	(0.024)	(0.071)	(0.067)
N	29,382		
2. Racially Disparate Impact:			
Black	−0.093	−0.015	−0.108
	(0.013)	(0.052)	(0.050)
N	49,535		
White	−0.021	0.033	0.012
	(0.017)	(0.063)	(0.061)
N	24,284		
3. First-Time Defendants:			
Prior case	−0.074	−0.021	−0.095
	(0.010)	(0.042)	(0.041)
N	22,941		
No prior case	−0.095	−0.055	−0.150
	(0.047)	(0.139)	(0.130)
N	67,648		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✔	✔	✔
Case Covariates	✔	✔	✔

Table R1: **Sentencing Mechanism: The Mediated Effect of Pretrial Incarceration on 2012 Turnout by Income, Race, and Prior Case (Mediation Analysis).** Pretrial incarceration is coded as 1 if detained for more than 3 days and 0 otherwise. Incarceration sentence takes 1 if individual faced a minimum sentence of 1 day or more. Instrument, covariates and fixed effects are described in the notes to Table H1. Incarceration sentence takes 1 if an individual was sentenced to a minimum 1 day or more of incarceration and their case reached a disposition before the 2012 general election and 0 otherwise. Heteroscedasticity-consistent standard errors are presented within parentheses.

S. Compliers

	Case Severity		
	Low	Medium	High
Compliers	0.111	0.121	0.158
Always-takers	0.077	0.398	0.580
Never-takers	0.812	0.481	0.262

Table S1: **Share of Compliers Across Levels of Case Severity.**

	Full Sample	All Compliers	Black Compliers	White Compliers
Demographic:				
Age	35.656	35.279	35.009	34.383
	(0.039)	(0.329)	(0.967)	(0.525)
Male	0.805	0.801	0.818	0.806
	(0.001)	(0.011)	(0.017)	(0.010)
Race:				
Black	0.547	0.559	1	0
	(0.002)	(0.019)		
White	0.268	0.259	0	1
	(0.002)	(0.008)		
Case-related:				
Any drug offense	0.369	0.323	0.335	0.312
	(0.002)	(0.015)	(0.026)	(0.027)
Any violent offense	0.141	0.200	0.177	0.220
	(0.001)	(0.020)	(0.033)	(0.034)
Any firearm offense	0.078	0.103	0.125	0.071
	(0.001)	(0.008)	(0.017)	(0.010)
Any property offense	0.199	0.246	0.238	0.267
	(0.001)	(0.015)	(0.026)	(0.026)
Any DUI offense	0.110	0.074	0.063	0.088
	(0.001)	(0.008)	(0.011)	(0.014)
Any prior case	0.747	0.742	0.764	0.600
	(0.001)	(0.068)	(0.123)	(0.110)
Electoral covariates:				
Post-treatment turnout, 2012	0.288	0.283	0.317	0.200
	(0.001)	(0.003)	(0.006)	(0.005)
Pretreatment turnout, 2008	0.289	0.285	0.308	0.212
	(0.001)	(0.002)	(0.008)	(0.005)
Voting-age-ineligible in 2008	0.093	0.105	0.133	0.118
	(0.001)	(0.012)	(0.036)	(0.021)
Pretreatment registration	0.538	0.532	0.546	0.483
	(0.001)	(0.008)	(0.018)	(0.013)

Table S2: **Complier Comparison.** This table presents the covariate means for the overall sample and the sample of "compliers" (all and separately by race), following the estimation approach in Dahl, Kostol and Mogstad (2014) and Abadie (2003) described in Appendix S. Compliers are defined as the defendants whose pretrial detention 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.

T. Timing Before the Election

	Second Stage Estimates		
	(1)	(2)	(3)
0 Months:			
Excluding cases 0 months from election	−0.130	−0.105	−0.105
	(0.044)	(0.039)	(0.038)
First Stage F-test	425.098	419.515	452.956
Prop. Detained Pretrial During Election	0.054		
N	90,589		
2 Months:			
Excluding cases 2 months from election	−0.089	−0.068	−0.069
	(0.049)	(0.043)	(0.042)
First Stage F-test	353.205	349.638	378.639
Prop. Detained Pretrial During Election	0.038		
N	84,905		
4 Months:			
Excluding cases 4 months from election	−0.093	−0.054	−0.053
	(0.055)	(0.048)	(0.047)
First Stage F-test	281.568	278.740	298.399
Prop. Detained Pretrial During Election	0.027		
N	79,484		
6 Months:			
Excluding cases 6 months from election	−0.042	−0.009	−0.009
	(0.060)	(0.053)	(0.052)
First Stage F-test	239.333	235.967	254.213
Prop. Detained Pretrial During Election	0.021		
N	74,271		
12 Months:			
Excluding cases 12 months from election	0.047	0.041	0.036
	(0.085)	(0.073)	(0.071)
First Stage F-test	122.387	123.929	136.356
Prop. Detained Pretrial During Election	0.011		
N	60,881		
Instrument + Fixed Effects	✔	✔	✔
Demographic Covariates	✘	✔	✔
Case Covariates	✘	✘	✔

Table T1: **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 H1. Heteroscedasticity-consistent standard errors are presented within parentheses.